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Institutional Setting
and Industrial Conflict

Pearlin
Status and Stress in Marriage

lauser, Koffel, Travis & Dickinson
Temporal Change in
Occupational Mobility

Bengtson
Generation and Family Effects

tewman
System Models of
Occupational Mobility

Cook
Expectations, Evaluations & Equity

lope
Models of Status Inconsistency

Carroll
Revitalization Movements
and Social Structure

Alexander and Eckland
Contextual Effects on
High School Attainment

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13

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ARTICLES

Institutional Setting and Industrial Conflict: Comparative Analyses of France, Italy and the United States ..... DAVID SNYDER 259

Temporal Change in Occupational Mobility: Evidence for Men in the United States ..... ROBERT M. HAUSER, JOHN N. KOFFEL, HARRY P. TRAVIS AND PETER J. DICKINSON 279

Two Markov Models of Open System Occupational Mobility: Underlying Conceptualizations and Empirical Tests ..... SHELBY STEWMAN 298

Models of Status Inconsistency and Social Mobility Effects ..... KEITH HOPE 322

Status Inequality and Stress in Marriage ..... LEONARD I. PEARLIN 344

Generation and Family Effects in Value Socialization ..... VERN L. BENGTSON 358

Expectations, Evaluations and Equity ..... KAREN S. COOK 372

Revitalization Movements and Social Structure: Some Quantitative Tests ..... MICHAEL P. CARROLL 389

Contextual Effects in the High School Attainment Process ..... KARL ALEXANDER AND BRUCE K. ECKLAND 402

13

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ITEMS

June, 1975

■ **DAVID SNYDER** (Institutional Setting and Industrial Conflict) is Assistant Professor of Sociology at Indiana University at Bloomington. He continues to study changes in the form and magnitude of collective action and protest and is beginning, with Paula M. Hudis, a large-scale project on the impact of industrialization on occupational structures and wages in Western Europe and the United States.

■ **ROBERT M. HAUSER** (Temporal Change in Occupational Mobility) is Professor of Sociology at the University of Wisconsin-Madison. The present collaboration grows out of his work with David L. Featherman on a replication in 1973 of the 1962 Blau-Duncan survey of occupational mobility in the United States. The ideas and methods developed in the present paper will be used in comparing the two surveys and in cross-national studies. **JOHN N. KOFFEL**, a former University of Wisconsin graduate student, is independently pursuing his interests in stratification and the sociology of learning. **HARRY P. TRAVIS**, a graduate student at Wisconsin, is completing a dissertation on the stratification of women. His other interests include mortality differentials, effects of status on fertility and log-linear models. **PETER J. DICKINSON** is employed on the occupational changes in a generation project of Hauser and Featherman.

■ **SHELBY STEWMAN** (System Models of Occupational Mobility) is Assistant Professor of Sociology in the School of Urban and Public Affairs, Carnegie-Mellon University. His other work on mobility includes a study of the dynamics of vacancy chains and a comparative study of the dynamics of open versus closed systems for demographic models. In addition, his current research includes investigations in organizational dynamics.

■ **KEITH HOPE** (Models of Status Inconsistency) is a Fellow of Nuffield College, Oxford, and University Lecturer on Methods of Social Research. His current interests are in social and occupational mobility and social policy evaluation. His most recent book is *The Social Grading of Occupations: A New Approach and Scale* (with J. H. Goldthorpe), 1974. He also has a forthcoming monograph reporting the findings of the 1972 British mobility study.

■ **LEONARD I. PEARLIN** (Status and Stress in Marriage) is a Research Sociologist for the Laboratory of Socio-Environmental Studies, National Institute of Mental Health, Bethseda, Maryland, where he is engaged in a program of research into the social origins of stress and normative coping patterns. His most recent book is *Class Context and Family Relations: A Cross-National Study* (Little, Brown, 1971).

■ **VERN L. BENGTSON** (Generation and Family Effects) is Associate Professor of Sociology and Chief of the Laboratory for Social Organization and Behavior at the Andrus Gerontology Center of the University of Southern California. He is director of the "Socio-cultural contexts of aging" program at USC and is continuing long-term research on socialization, age stratification and models of generational influences on social change. His most recent books are *The Social Psychology of Aging* (Bobbs-

(Continued on page 416)

AMERICAN SOCIOLOGICAL REVIEW
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INSTITUTIONAL SETTING AND INDUSTRIAL CONFLICT: COMPARATIVE ANALYSES OF FRANCE, ITALY AND THE UNITED STATES*

DAVID SNYDER

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Economic models and interpretations have dominated empirical studies of industrial conflict. However, recent investigations of French work stoppages assign more importance to organizational and political than to economic determinants of strike activity. These apparent contradictory results and conclusions are ignored by previous work, almost all of which examines a single country and/or limited time span. In this paper, we argue that existing findings differ according to the institutional context of labor relations. Specifically, we identify assumptions underlying each line of argument; argue that the extent to which these assumptions hold influences results and conclusions of various analyses; and show that each set of assumptions holds best within different types of institutional setting. We test this argument with regression analyses of yearly timeseries data for France (1876-1966), Italy (1901-1970) and the United States (1900-1970). Results of these analyses correspond to our hypotheses concerning the influence of institutional setting on fluctuations in aggregate strike activity. We then consider implications of these findings for investigations of other forms of collective protest.

INTRODUCTION

The strike as an organized form of work stoppage has existed for well over two hundred years (Frow et al., 1971:ix). For nearly that long, labor-management disputes have interested analysts of various disciplines and theoretical orientations. Despite this diversity, there are only three current conceptual approaches to the analysis of industrial conflict. These perspectives consider strikes as (1) protests generated by rapid social change; (2) outcomes of the bargaining process or (3) forms of collective action.[1]

The first of these approaches views rapid social change, particularly rapid rates of industrialization and urbanization, as a source of societal and individual strains (Durkheim, 1951:241-76; Smelser, 1963). Such changes allegedly hasten the breakdown of social groups, generate anomie or other aberrant psychological states and consequently lead to protest, of which strike activity is but one manifestation (e.g., Kerr et al., 1960).

A second framework for the analysis of industrial conflict comes directly from theories of bargaining and indirectly from consideration of union-management relations in models of wage determination. In its extreme, this perspective argues that "...the majority of strikes are doubtless the result of faulty negotiations" (Hicks, 1957:146). However, most work concerned with the determinants of strikes, though influenced by this classical approach to bargaining, is more moderate in its assumptions.[2] Although

*This paper reports portions of an unpublished Ph.D. dissertation (Department of Sociology, The University of Michigan, 1974). I am grateful to Robert Cole, Douglas Hibbs, Jr., Paula Hudis, Edward Laumann, Charles Tilly and Gavin Wright for comments on that work. I am, of course, responsible for any errors.

[1] Three in aggregate level explanations. There are of course other perspectives, for example the "human relations approach" (Gouldner, 1954), in which the presence or absence of industrial conflict indicates the degree to which labor-management relations within the plant or firm are harmonious or rancorous. However, the closed system framework of these studies (see Goldthorpe et al., 1968:178-83; Thompson, 1967, for criticism) makes them inappropriate to analyze the issue of *changes* in the frequency and magnitude of industrial conflict which this paper addresses.

[2] Analyses of strikes attribute more strategic

strikes are generally viewed as one of two bargaining outcomes—the other is negotiated settlement—this line of analysis attempts to identify economic fluctuations which affect the relative strength and propensity to bargain of the respective parties (Rees, 1952; Ashenfelter and Johnson, 1969; Pencavel, 1970). Most recent empirical work along these lines specifies the gap between workers' actual and expected rates of real wage change as the most important of these economic determinants of industrial conflict.

A third approach views strike activity as a form of collective action. In this view, whether individuals organize to pursue common interests is problematic (Olson, 1965), and strikes therefore occur to the extent that there is organizational capacity for such action (Shorter and Tilly, 1974). Since strikes involve claims on scarce and competitive resources, this approach considers industrial conflict as a political as well as organizational phenomenon (LeFranc, 1970; Perrot, 1971; Shorter and Tilly, 1974).

This paper examines the latter two approaches and their respective explanations of industrial conflict more closely.[3] The bargaining approach to industrial conflict and the economic models which characterize it identify substantially different determinants of strike activity than does the collective action approach and its organizational/political argument. However, there is empirical support for both explanations, based on analyses of different countries and periods. We account for these discrepant findings and interpretations in terms of the institutional context of labor relations and its effects on assumptions underlying the alternative economic and organizational/political models. We then test this institutional argument with regression analyses of French, Italian and United States strike data. Since the models of industrial conflict closely parallel recent explanations of other forms of collective protest (especially collective violence), we discuss the theoretical and strategic implications of our findings for analyses of these other forms of protest.

BUSINESS FLUCTUATIONS, WAGE CHANGES AND STRIKE ACTIVITY

Economic models and interpretations have dominated quantitative analyses of aggregate strike activity, especially in North America. In the United States, Hansen (1921) began a tradition of analyzing the correspondence between fluctuations in the frequency of industrial conflict and indicators of economic conditions. Later studies of the entire series of pre-World War II U.S. strike data (Griffin, 1939; Yoder, 1940) variously include real and money wages, prices, an index of business activity and unemployment as measures of workers' welfare and business prosperity. Although they observe relatively weak relationships between industrial conflict and each economic indicator, these studies generally agree that strike activity varies positively with economic prosperity.[4]

These early empirical searches for correlates of strike fluctuations lack a consistent conceptual approach to industrial conflict. Rees (1952) avoids this *ad hoc* approach by his choice of indicators and explanation of findings explicitly in light of their probable effects on economic calculations and relative bargaining positions of workers and em ployers. Rees' analysis of monthly data ov s the period 1915-1950 "...revealed a b ly correspondence between strike cycles an ons business cycle for the period 1915-193 atly very little correspondence for the w later postwar years" (Rees, 1952:373). d by primary factor in this positive relat ly ex- between prosperity and the frequ trength itude of

autonomy to labor and management. For example, Ashenfelter and Johnson (1969) argue that (1) even when they are not economically rational, strikes sometimes result from unions' internal political dynamics and (2) stoppages must occur in part to remain convincing bargaining threats. Despite these modifications of the classical bargaining approach, they argue that "...the basic function of the strike is as an equilibrating mechanism to square up the union membership's wage expectations with what the firm may be prepared to pay" (Ashenfelter and Johnson, 1969:39).

[3] We do not consider the rapid change explanation of industrial conflict here. Recent work (e.g., Shorter and Tilly, 1974; Snyder, 1974) seriously challenges it on theoretical and empirical grounds.

[4] Analyses of prewar strike data planations countries (e.g., Knowles, 1952, for Bri ker organi- Girey, 1965, for France) use simila ptual *and* methods and arrive at equivalent ork, based concerning the determination of indus

[5] Rees uses the index of confor integrates measures the correspondence betw argument

strikes is the labor market. Rees argues that improving business conditions and rising employment increase unions' propensity to call strikes by offering them a number of bargaining advantages:[6] "...the employer's reluctance to lose his share of the expanding market; ...his observation of rising wages elsewhere; his ability to replace strikers with nonstrikers diminishes as employment rises; and the strikers have an increased chance of obtaining employment elsewhere" (Rees, 1952:381). Therefore, in good times unions are more likely to press demands because the actual and perceived short run costs of striking are less for workers and greater for employers.

Rees thus follows earlier work in considering strikes as primarily economic phenomena. He acknowledges that noneconomic variables such as government policy, political events (the war) and public opinion may affect the timing of industrial conflict (a necessary admission in view of the lack of conformity between business and strike cycles from 1938 to 1950). However, Rees contends that such influences are irregular and predicts re-establishment of the positive relationship between prosperity and strikes in the postwar period. Weintraub's (1966) finding of perfect conformity between the business and strike cycles in the United States from 1949 to 1961 is consistent with this prediction.

Recent work, however, challenges this apparent closure, on grounds that (1) there are other plausible psychological linkages between economic conditions and strikes; (2) the index of conformity measure does not estimate the relationship between period-to-period fluctuations in the variables and (3) the absence of multivariate analyses precludes determining whether different dimensions of economic activity have independent effects on strike activity (O'Brien, 1965; Ashenfelter and Johnson, 1969; Vanderkamp, 1970).

In an investigation which has heavily influenced subsequent analyses, Ashenfelter and Johnson (1969) overcome many of these objections. They argue that the greater the ratio of the previous profit level to previous wage payments of firms, and the greater the minimum acceptable wage increase to management, then the greater the probability that union and management will agree on a contract prior to a strike (Ashenfelter and Johnson, 1969:37-9). After solving problems of specification and estimation, they report regression coefficients for an equation in which the civilian unemployment rate (following Rees' argument), a lagged moving average of percentage rates of change in real wages,[7] the ratio of corporate profits to total wage compensation, seasonal (quarterly) dummy variables and a linear trend expression are predictors of the number of strikes in the United States (covering the period 1952-1967). Their results show that, except for the profits ratio, all partial coefficients are highly significant and in the expected direction.[8] Moreover, the model explains greater than ninety percent of the variance around the grand mean, and its parameters are

and strike cycles. He attributes the lack of nity in the later years to the unusual tances of World War II, removal of restraints ng at the end of the war and the depressing n strike frequency of the passage of ey in 1947.

argues that business fluctuations alter to strike (as opposed, for example, to pensity coupled with an increase in ization in good times). Although Rees hat union membership fluctuations ain changes in strike activity, he hat union called work stoppages and that union behavior is influenced conomic shifts.

[7] This lagged moving average represents the expectations-achievements gap in real wage rates of change. For a complete derivation of this specification and the assumptions behind it, see Ashenfelter and Johnson (1969:43) and Hibbs (1974a:25-7). Briefly, they assume that workers' expectations are formed by actual wage changes over n previous periods (in which recent experience is weighted most heavily) and by a constant long run increase parameter (in which current and past experiences are weighted about equally). Hibbs (1974a:27) points out that "The difference of these lag functions...is therefore likely to exhibit an inverted U-shaped distribution, which is conveniently estimated by the polynomial distributed lag method of Shirley Almon (1965)." Hibbs also notes (1974a:56) that Almon's procedure (which we employ subsequently) merely estimates the lag function and is relevant to expectations-achievements *only* through the intervening assumptions outlined above.

[8] Prior to estimation, however, Ashenfelter and Johnson indicate the indeterminacy of the expected sign of the profits variable, because "Although management is more likely to give in when previous profits are high, the union is also likely to increase its demands" (1969:41).

stable over the entire 1952-1967 period. Ashenfelter and Johnson conclude substantively that "...it seems that the aggregate level of strike activity is behaviorally related to the degree of tightness of the labor market and previous rates of change of real wages" (1969:47).

Subsequent analyses of post-World War II data from other countries are highly consistent with Ashenfelter and Johnson's findings. Pencavel (1970) reports similar coefficients and good fit of this model estimated with British data. Hibbs (1974a), in one of the few instances of comparative work, pools postwar data from ten industrialized nations in order to test alternative specifications of the expectations-achievements gap in real wages. Hibbs' results show that the Ashenfelter-Johnson formulation is clearly superior to other plausible representations of that gap.[9] And, in an analysis of monthly strike fluctuations in the postwar United States, Skeels (1971) comes to roughly similar conclusions concerning several different dimensions of industrial conflict.[10] Although Skeels adds the political indicators percent Democrats in Congress and party of President to the basic economic model, these variables perform poorly in this postwar period.

The explanatory power and consistency of all these findings lend strong support to economic interpretations of strike activity. However, some recent arguments assign more weight to organizational and political than to economic determinants of strike fluctuations. We summarize this organizational/political explanation below and then account for conceptual and empirical differences between it and the economic models.

WORKER ORGANIZATION, POLITICAL CRISES AND INDUSTRIAL CONFLICT

The idea that worker organization and industrial conflict are related is neither novel nor surprising. Cross-national evidence has long indicated that union members are more likely than unorganized workers to strike (Griffin, 1939:101, 115; Knowles, 1952: 149-50; Goetz-Girey, 1965:146). Despite that finding and the often explicit, though nonempirical, consideration of union membership as a possible determinant of strikes, this work agrees that

> ...there is no marked correspondence between the numbers of Union members and of workers involved in strikes. The numbers of Union members correspond better with the number of strikes, and this probably reveals little more than that the influences associated with the trade cycle affect both sets of fluctuations (Knowles, 1952:150).

Although Griffin, Knowles, Goetz-Girey and others indicate a positive relationship between union membership and the frequency of industrial conflict, they opt for strict economic explanations of strike fluctuations. This close agreement *and* the conclusion itself are surprising because the interpretations are never based on any statistical association which includes unionization. The discrepancy between the findings and conclusions of these studies suggests that economic fluctuation are assigned primary explanatory power sole on the basis of *a priori* assumpti concerning industrial conflict as an inhere economic phenomenon. Moreover, economic analyses, perhaps influence these assumptions, have not rigorous amined the possible impact of union s on changes in the frequency and magn industrial conflict.

Only recently have coherent ex of industrial conflict identified wor zation as an important conce empirical variable. This recent w largely on the French experience worker organization into a line o

[9] Hibbs uses total number of mandays lost as his measure of strike activity. He pools the data in order to obtain a sufficiently long series of observations. Although this method precludes comparison among particular countries, Hibbs shows that the model does not vary significantly across types of bargaining systems (decentralized, centralized and highly centralized) in the postwar period.

[10] Several recent studies (Shorter and Tilly, 1971; Britt and Galle, 1972; Hibbs, 1974a) have shown how total number of mandays lost can be decomposed into frequency, size and duration dimensions. Most of the economic models entirely neglect the latter two dimensions. We also note that Skeels attributes more weight to business prosperity than to wage changes in explaining strike fluctuations. This difference from other recent studies may be due to Skeels' use of the simple percentage rate of wage change, which assumes that expectations are based solely on the experience of the most recent period.

which views the strike more as a political than economic phenomenon. Although LeFranc (1970) and Perrot (1971) give such an interpretation to industrial conflict in France, Shorter and Tilly (1974) provide a theoretical basis and empirical support for that argument.

Shorter and Tilly consider industrial conflict as a form of collective action, contrary to the economic models which view strikes as bargaining outcomes. Their approach relies heavily on three related arguments. First, Olson (1965) shows that whether individuals ever organize to pursue common interests or redress shared grievances is problematic rather than inevitable (as has often been assumed). Second, Etzioni (1968, especially chapter 15) develops the concept of mobilization as (an increase in) *collective* control over resources. Etzioni maintains that a group's collective actions depend on the type and extent of its mobilization. Finally, Tilly (1969; forthcoming) applies these concepts in a political context to explain collective violence. Tilly argues that violence is the byproduct of the mobilization of groups, their consequent contention for political power and the (often forcible) resistance to contenders by current power holders.[11]

On these bases, Shorter and Tilly assert that workers' collective actions depend on the mobilization of labor organizations. Collective action "...by definition consists of the application of some population's resources on behalf of the group's common objectives" (Shorter and Tilly, 1974:5). With regard to labor and strikes, these common objectives are political (i.e., labor organizations act as if political power is salient). In the short run, strikes are designed to, and often do, bring pressure on employers and governments. In the longer run, strikes are a mechanism through which labor organizations contend for membership in a nation's polity. If this entry is accomplished—it is one of several possible outcomes for any group—the consequence is a routine means for labor organizations to make legitimate claims on government resource allocations for themselves and their members.

Shorter and Tilly expect that the mobilization, or organizational capacity for collective action, of workers (measured by union membership) will be the most powerful predictor of strike fluctuations. They also hypothesize that political crisis or change (of which cabinet changes and election years are crude indicators) increases the vulnerability of government to the collective demands of labor unions (and other organized collectivities). That these demands are expressed through strikes completes their rationale for interpreting industrial conflict as a political as well as organizational phenomenon.

Shorter and Tilly's regression analyses of yearly French data (primarily from 1890 to 1938) yield results generally consistent with their theory. Union membership has the largest net effect on fluctuations in the number of strikes and strikers. They also find small, but often not statistically significant, influences of cabinet changes and election year on the strike measures.[12] Additionally, the impact of business conditions and wages is relatively small. However, Shorter and Tilly attribute some importance to short run economic *hardship* in increasing the frequency and size of industrial conflict. Therefore, neither the signs nor magnitudes of coefficients of the economic variables are consistent with those reported in previous studies.

These findings clearly differ from those of economic analyses in France and elsewhere. However, largely due to the absence of work with comparative *and* historical dimensions, there are no attempts to account for theoretical and empirical differences between these results for France and those of the

[11] Gamson's (1968) stable unrepresentation model of political influence is implicit here. Tilly designates "the polity" as that set of groups which have some regular, legitimate influence over government decisions. Organized groups which are not yet polity members are contenders. Shorter and Tilly use a similar conceptual model to analyze industrial conflict in France and take the strong association between year-to-year fluctuations in collective violence and in strike activity as indirect evidence that similar processes underlie both phenomena.

[12] Although it is plausible that larger numbers of strikes draw workers into unions and produce government crises, coefficients of equations using lagged variables are consistent with expected causal directions. However, we note that Shorter and Tilly's empirical analyses necessarily specify the theoretical model indirectly.

business cycle/wage change models for other countries. We propose a resolution of these differences below, and then report some empirical tests of our argument.

UNDERLYING ASSUMPTIONS, INSTITUTIONAL SETTING AND DIFFERENCES IN PROCESS

Despite the apparent similarity of findings from several countries and time periods, two important differences distinguish recent economic analyses (Ashenfelter and Johnson, 1969; Pencavel, 1970; Skeels, 1971; Hibbs, 1974a) from earlier investigations. First, the recent studies construct models explicity in terms of the alleged calculations of workers, unions and employers. Second, they find much stronger relationships between economic indicators and (generally) the frequency of strike activity. Given this considerable empirical support, parties in bargaining situations do apparently make the hypothesized calculations.[13]

However, hypotheses concerning behavioral consequences of aggregate economic shifts and the empirical findings which justify them depend on assumptions about the institutional context of labor relations. But, because of the dominance of studies which analyze data from a single country and/or limited time span, the consequences of such variation in institutional setting for the *process* underlying fluctuations in strike activity are never considered.[14] We argue below that (1) there are major, often implicit, assumptions in the "bargaining approach" to strikes, which are reasonable only within certain institutional settings; (2) these institutional arrangements and the corresponding validity of the bargaining model's assumptions are limited mostly to recent (especially North American and Northern European) labor history and (3) results of studies of these countries and periods, which form the bulk of existing empirical work, are exceptions which have influenced the rules for analyzing and interpreting strikes in other times and places.

Chart 1 lists major and subsidiary assumptions of the economic studies and their "consequences" for analyses of fluctuations in strike activity. Although some of these assumptions are stated explicitly (e.g., sets I and II in the recent business cycle/wage change models), they are all implicit in virtually every economic study. Such studies, however, do not generally recognize that the assumptions must hold well to justify the theoretical and empirical models which characterize them.

The assumptions listed in Chart 1 do hold to a large, though differential, extent for the *recent* experience of countries on which most quantitative work is based. However, for countries such as France and Italy, the entire framework of assumptions breaks down. Employers have not been willing to negotiate with unions, especially the (largest) communist labor confederations. Employers' aversion to negotiation, as well as to union recognition, has a long tradition in both countries (LeFranc, 1967; Lorwin, 1954; Rigola, 1947; Stearns, 1968). The number of collective bargaining agreements has been quite small (Lorwin, 1954; LeFranc, 1967:302; Sellier and Tiano, 1962:546). Because of the small number of contracts, the expectation that strikes occur because agreement cannot be reached prior to the expiration of a previous contract is not justified. And often, individual employers do not honor signed negotiations (LaPalombara, 1957).[15] These studies also indicate that unions are not always (or primarily) concerned with short term economic costs and benefits. Moreover, available data show that union membership, even in the post World War II period, has not been large or stable in either country.

[13] As Ashenfelter and Johnson (1969:38) point out, however, it is not necessary that the parties actually make these calculations, only that they act as if they do.

[14] Ross and Hartman's (1960) study is the only analysis of industrial conflict which incorporates both historical and comparative dimensions. However, they employ institutional differences to explain international and intertemporal variation in the *level* of strike activity (see Eldridge, 1968; Shorter and Tilly, 1971; Hibbs, 1974a, for summaries and challenges of Ross and Hartman's findings and conclusions). We stress that our concern here (as in the work cited previously) is the *process* underlying industrial conflict—i.e., the determinants of short run fluctuations in strike activity.

[15] Although it is still reasonable to view French and Italian labor relations in these terms, some sources indicate weakening of these characteristics in recent years.

Chart 1. Major and Subsidiary Assumptions of Studies on Economic Fluctuations and Industrial Conflict

I. Both employers and union leaders (and/or members) make rational decisions based solely on the short term economic benefits of agreement versus the short term economic costs of a strike.

A. All parties (should act as if they) calculate only short term economic costs and benefits.
B. The costs of striking for workers decrease in times of prosperity.
C. The costs of a strike increase for employers in times of prosperity.

Consequence: Aggregate economic changes affect the costs and benefits of striking for all parties. Therefore, these changes have a primary influence on strike fluctuations, by increasing workers' bargaining strength and/or propensity to negotiate.

II. Collective bargaining is well institutionalized.

A. Parties are willing to negotiate with each other (each recognizes the right of the other to bargain).
B. There is a current contract among parties, as a result of past negotiations.
C. Parties honor agreements for the term of the contract; each expects the other to do so.

Consequence: Strikes are an outcome of the bargaining process; they occur when the parties cannot agree on new terms prior to the expiration of a previous contract.

III. The decision to strike is made by a union (leaders and/or vote of membership).

A. Only union members will strike.
B. Unions are supported by members (both in the decision to strike and with previous dues payments).

Consequence: Unions are a necessary condition for (reasonably large scale) strike activity.

IV. Net of secular trend, union membership does not fluctuate much.

A. Fluctuations in union membership or number of unions do not explain changes in the number (or size) of strikes.
B. If there is some association between changes in union membership and fluctuations in measures of industrial conflict, then it must be due to the joint influence on both of economic conditions.

Consequence: Changes in union membership cannot account for fluctuations in industrial conflict.

Although the United States versus France and Italy do not represent ideal types for the assumptions listed in Chart 1, they clearly differ in the extent to which those assumptions hold. We maintain that this differential validity follows from differences in several related characteristics of labor relations contexts, specifically (1) the extent and stability of union membership; (2) the national political position of labor and (3) the degree of institutionalization of collective bargaining. Where union membership is large and relatively stable, the political position of labor firmly established and collective bargaining well institutionalized, assumptions underlying the economic models hold well. Under these conditions, it is reasonable to assume that (1) workers and unions (act as if they) calculate short term economic costs and benefits of striking; (2) work stoppages occur where parties cannot agree prior to expiration of a current contract and (3) therefore aggregate strike activity fluctuates primarily in response to changes in business prosperity and (actual minus expected) wage changes. Chart 2 summarizes our alternative argument con-

cerning the implications of this first ideal type (and its opposite) institutional setting for theoretical and empirical models of industrial conflict.

In the second (Type B) institutional setting, union membership is neither large nor stable, labor organizations are not legitimate interest groups with some regular influence over resource allocation decisions (i.e., not polity members), and collective bargaining is not well institutionalized. These institutional settings (e.g., France and Italy) invalidate the major assumptions underlying the economic bargaining models. Within these arrangements, labor's organizational strength is problematic and (longer run) political ends such as polity membership are more salient to labor than are short run, strictly economic costs and benefits. We therefore expect that, under *these* conditions, strikes fluctuate with organizational and political currents, as Shorter and Tilly find for France.[16]

Although the assumptions underlying the bargaining approach hold only under certain institutional arrangements, economic models have been employed without regard to variation in institutional setting and the validity of these assumptions (e.g., in analyses of French data). In fact, *only since World War II* have conditions associated with ideal Type A in Chart 2 been firmly entrenched in the United States. Our institutional argument therefore accounts for the discrepancy in the strength of relationships between economic indicators and strike activity in pre- and post-World War II studies of U.S. data. However, the absence of equivalent analyses makes this conclusion tentative. Yoder's (1940) investigation is the only study of the entire series of prewar U.S. data which uses linear correlation and regression techniques (though not multivariate models). Yoder reports no significant association between indicators of prosperity and the yearly number of strikes and strikers.[17] Conversely, studies of postwar data (Ashenfelter and Johnson, 1969; Skeels, 1971) achieve considerable explanatory power with their models.

While our argument appears to account for international and intertemporal differences in findings and interpretations of previous empirical studies, analyses of industrial conflict in France, Italy and the United States reported below test it more thoroughly. Specifically, findings expected from the institutional explanation are: (1) no substantial impact of either business conditions or wage changes on industrial conflict in any of the three countries prior to World War II; (2) considerable net effects of union membership and political indicators on strike activity in each country over the prewar period; (3) for *the United States only*, significant net effects of the business prosperity/wage change measures, but *not* the unionization/political indicators, in the years after World War II and (4) results similar to those for the earlier period (hypotheses 1 and 2 above) for postwar France and Italy.[18] Consequently, interperiod change versus stability of parameters expectations follow, respectively, from these last two hypotheses.

DATA AND METHOD

The data are yearly observations on each variable for France (1876-1937; 1946-1966), Italy (1901-1924; 1947-1970) and the United States (1900-1970).[19] We employ timeseries regression equations to analyze year-to-year

[16] For heuristic reasons, we have presented our institutional argument in terms of the relative validity of assumptions of economic analyses. The assumptions implicit in Shorter and Tilly's model are opposite those listed in Chart 1 and are valid under Type B conditions in Chart 2.

[17] Most other statistical analyses of prewar U.S. data (e.g., Jurkat and Jurkat, 1949; Rees, 1952) use the index of conformity. While this index measures the association between business *cycles* and strike *cycles* (both of which may vary in length), it does not estimate *regular short run effects* of economic indicators on strikes. However, Weintraub's finding of perfect conformity for the 1949-1961 period, which differs considerably from that of earlier studies, is consistent with our argument.

[18] Although the institutional argument is presented in ideal typical terms, we of course expect that empirical results will vary according to the *relative* validity of assumptions under different institutional arrangements. Given evidence of some changes in postwar French and Italian labor relations cited earlier, results for these countries in the postwar period may partly reflect such shifts.

[19] Sources of data used in the analyses are outlined in the Appendix. Space considerations preclude a full presentation and discussion of related issues (e.g., reliability assessments, choice among alternative series). See Snyder (1974:309-22) for that information.

Chart 2. Ideal Types of Institutional Settings and Their Consequences for Models of Strike Fluctuations

	TYPE A	TYPE B
COLLECTIVE BARGAINING:	Well institutionalized	Poorly institutionalized
LABOR UNIONS:	Large, stable membership	Small, unstable membership
POLITICAL POSITION OF LABOR:	Legitimate interest group; routine participation in decisions affecting resource allocation	Illegitimate interest group; no routine participation in government allocation decisions
CONSEQUENCES:	Organizational strength not problematic	Organizational strength problematic
	Economic ends (and economic costs and benefits) are primary focus of action	Political ends (and non-economic costs and benefits) are primary focus of action
	Strikes fluctuate primarily in response to changes in business conditions and in wages	Strikes fluctuate primarily in response to union membership (organizational strength) and political changes

fluctuations in strike activity separately for each country and (pre- and post-World War II) time period. The following points are relevant to all these analyses: first, each equation is estimated separately for two standard measures of industrial conflict, the number of strikes beginning in a year and the number of participants in those strikes (strikers). Although Hibbs (1974a) argues that the total number of mandays lost in work stoppages is the best single indicator of industrial conflict, we use the component frequency and size dimensions for two reasons: (a) most of the work on which we comment uses either or both indicators and (b) available evidence suggests empirical differences between them (e.g., Skeels, 1971). However, employing the composite mandays measure does not support any different interpretation.

Second, we use natural logarithmic transformations of both strike measures as dependent variables, in equations of the form: $\text{Ln (Strikes)} = a + b_1X_1 + b_2X_2 + \ldots + b_kX_k + u$ (disturbance). We use this transformation because of the substantial skewness of the strike measures, especially for France and Italy. Although this form implies a nonadditive model, other analyses (not reported here) indicate that the substantive conclusions are insensitive to the functional form of the estimating equation.[20]

Finally, the analyses employ single equation models. This procedure implies that all independent variables are exogenous, which is not the case. Ashenfelter and Pencavel (1969) and Kramer (1971) have shown, respectively, that union membership and the distribution of national electoral offices by political party are determined in part by economic fluctuations. We are currently undertaking multiequation respecification and intend these results only to illustrate varying applicability of alternative explanations of industrial conflict under different institutional settings.

FINDINGS

The United States

Since the preponderance of economic models and interpretations of industrial conflict are based on U.S. data, this country provides the strongest test of our alternative argument. Table 1 presents partial regression coefficients and summary statistics for models including economic and organizational/political variables, estimated separately for the years 1900 to 1948 (equations 1.1 and 1.2) and 1949 to 1970 (equations 1.3 and 1.4).[21] Table 1 shows that, during the earlier period, net effects of the unemployment indicator (of labor market tightness) and the lagged moving average of real wage changes (which represents the expectations-achievements gap) are weak and not significant.[22] Moreover, the wage specification manifests wrong signs in both equations (1.1) and (1.2). These findings indicate that the labor market (prosperity) and expectations-achievements theories are unsatisfactory during the 1900-1948 period. In these early years, workers do not act as if they make the economic calculations attributed to them by the recent studies which use postwar data. Conversely, unionization, percent Democrats in Congress and party of President in equations (1.1) and (1.2) have strong positive effects on year-to-year fluctuations in the measures of strikes.[23] These

[20] These other analyses include use of raw strike measures, deflation for labor force size and transformations (e.g., square root) other than the logarithmic.

[21] This cutting point is arbitrary, chosen because 1949 is the earliest year included in any economic analysis of postwar data (Weintraub, 1966). Changing the cutting point slightly, and even dropping the war years entirely (so that the equations correspond precisely to the "pre" and "post" war designations), have negligible effects on findings and conclusions.

[22] We employ Almon's (1965) procedure to estimate the expectations–achievements gap. In these and subsequent equations, the estimation uses a second degree polynomial which falls to zero after six periods. Higher degree polynomials do not provide significant improvements in fit. We also note that, despite the presence of two ratio variables (unemployment and unionization) in these equations, they do not produce spurious results, probably because variation in the (common) labor force denominator is very small relative to that in the numerators.

[23] Excepting the influence of percent Democrats on the log number of strikers, the absence of which persists through a variety of estimating equations, Skeels (1971) includes percent Democrats and party of President as measures of political "attitudes" of government toward labor (although he finds little evidence of effects of either measure in the postwar period). Additionally, since the Shorter-Tilly election year variable has a different meaning in U.S. politics, we do not include it in these equations.

Table 1. Estimated Partial Regression Coefficients for Economic and Political Determinants of the Log Number of Strikes and Strikers: The United States (1900-1948; 1949-1970)[a]

		Dependent Variable	E_1	E_2	U	P_1	P_2	T	N	R^2	D-W
			Regression Coefficient (Standard Error)								
PREWAR[b]	(1.1)	Strikes (Logged)	-.0073 (.0085)	.0046 (.0033)	.094 (.013)**	.016 (.005)**	.402 (.143)**	-.051 (.007)**	49	.8256	1.89
	(1.2)	Strikers (Logged)	-.0000 (.0157)	.0643 (.0606)	.070 (.023)**	-.003 (.010)	.811 (.266)**	-.021 (.010)*	49	.6683	1.77
POSTWAR[b]	(1.3)	Strikes (Logged)	-.1326 (.0264)**	-.1156 (.0477)*	-.040 (.078)	-.012 (.008)	-.108 (.108)	-.016 (.029)	22	.7041	2.16
	(1.4)	Strikers (Logged)	-.2090 (.0617)**	-.3923 (.1113)**	.196 (.182)	-.010 (.018)	.046 (.251)	.045 (.068)	22	.5879	2.29

E_1 = unemployment as % of civilian labor force; E_2 = six-year lagged moving average of % change in real wages ($\Sigma \Delta R_{t-i}$); U = union membership as % of civilian labor force; P_1 = % Democrats in Congress; P_2 = party of President; T = time.

[a]For sources, see Appendix. *Significant at .05. **Significant at .01.

[b]Equations (1.1) and (1.2) cover the years 1900-1948.
Equations (1.3) and (1.4) cover the years 1949-1970.

"political attitude" variables represent a different dimension than the Shorter-Tilly political crises. However, we consider the party affiliation effects to be indicative of similar underlying processes. Specifically, governments favorable to labor encourage (though not intentionally) escalation of demands by labor organizations, possibly due to calculations of increased success probabilities. Since labor is still a "contender" in this earlier period, its collective demands are necessarily manifested largely through strike activity. For the postwar period, the expected lack of impact of these political variables is not because Democratic officials should be less "sympathetic" to labor. Rather, given the polity entrance of labor in the postwar United States, we argue that political demands are routinely expressed in less public influence attempts (e.g., lobbying) than strikes.

The results reported in equations (1.1) and (1.2) indicate that standard economic interpretations of prewar U.S. strike activity (Griffin, 1939; Yoder, 1940; Rees, 1952) are seriously misspecified. Instead, the findings are consistent with the organizational/political model with which Shorter and Tilly explain strikes in France. Therefore, it appears that the validity of this model is not peculiar to France, but applies elsewhere under roughly comparable institutional conditions.

Equations (1.3) and (1.4) estimate the same model with data from 1949 to 1970. The coefficients for this recent period change dramatically from those of the earlier years. In both equations, coefficients of unemployment and (actual minus expected) real wage changes are significant and in the expected direction. These results are substantially similar to those reported by other studies of the same period (Ashenfelter and Johnson, 1969; Skeels, 1971), which indicate that workers and unions time their strike behavior according to short run economic calculations.

Equations (1.3) and (1.4) show a similar change from the earlier period in the effects of unionization and the political indicators on industrial conflict. Coefficients of these variables are not significant in either equation, and generally have wrong signs.[24] In fact, dropping the unionization and political indicators from equations (1.3) and (1.4) only marginally affects the values of the summary statistics.[25] We therefore conclude that, in the postwar United States, organizational and political influences on *fluctuations* in the frequency and size of industrial conflict are unimportant. More generally, results shown in Table 1 are highly consistent with our expectations of change over time in the parameters affecting industrial conflict in the United States and with our argument that such changes reflect a corresponding shift in the institutional context of U.S. labor relations.

France

Table 2 presents partial regression coefficients and summary statistics for equations estimated with French data (1876-1937; 1946-1966). Equations (2.1) and (2.2), covering the prewar years, yield results substantially similar to those reported by Shorter and Tilly (1974) for 1890 to 1938.[26]

[24] Anonymous referees raised two salient issues about unionization in the postwar United States: (1) tests of significance in Table 1 should be for interperiod differences in the coefficients. Since the unionization coefficient is larger for 1949-1970 than for 1900-1948, this gives a negative finding. While we can (and do) derive change/stability of parameters predictions, the central argument concerns the validity of alternative explanations of strike fluctuations *within* particular types of institutional setting. We therefore maintain that appropriate statistical tests should *consistently* evaluate those explanations vis-à-vis a null model. (2) The institutional condition of relative stability of union membership artifactually attenuates the empirical relationship between unionization and strike activity. This is a valid point. However, the variance of unionization in the postwar period is not so small that it necessarily implies a nonsignificant relationship with the strike indicators. For example, the standard deviation of the unionization measure for 1949-1970 is 2.78, while that of unemployment (which has significant effects on strike fluctuations) is only 1.08.

[25] For each equation in Table 1, the explained variance is large (although this is common with time ordered data), and the Durbin-Watson (D-W) statistic indicates no significant first order autocorrelation of disturbances. The D-W statistic is symmetric about the value 2.00.

[26] Durbin-Watson statistics for ordinary least squares (OLS) estimates of equations (2.1) and (2.2) did not allow rejection of the null hypothesis of first order disturbance autocorrelation. We assumed that the disturbances were governed by a first order autoregressive process and used generalized least squares (GLS) to re-estimate those equations (see

Table 2. Estimated Partial Regression Coefficients for Economic and Political Determinants of the Log Number of Strikes and Strikers: France (1876-1937; 1946-1966)[a]

		Dependent Variable	Regression Coefficient (Standard Error)								
			E_1	E_2	U	P_1	P_2	T	N	R^2	D-W
PREWAR[b]	(2.1)	Strikes (Logged)	-.0081 (.0052)	-.060 (.051)	.043 (.013)**	.083 (.081)	.353 (.154)*	.026 (.009)**	62	.4358	1.95
	(2.2)	Strikers (Logged)	-.0049 (.0064)	.0057 (.0616)	.056 (.019)**	.086 (.108)	.422 (.205)*	.023 (.011)*	62	.3622	1.49
POSTWAR[b]	(2.3)	Strikes (Logged)	.0194 (.0216)	.0187 (.0168)	.0008 (.0066)	.023 (.176)	-.284 (.228)	-.166 (.179)	21	.2011	2.09
	(2.4)	Strikers (Logged)	.0407 (.0359)	.0147 (.0279)	.0071 (.0109)	.080 (.292)	-.827 (.379)*	-.316 (.298)	21	.3266	1.55

E_1 = national income (000's of francs); E_2 = six-year lagged moving average of % change in real wages ($\Sigma \Delta R_{t-i}$); U = union membership as % of civilian labor force; P_1 = number of cabinet changes; P_2 = election year; T = time.

[a]For sources, see Appendix. *Significant at .05. **Significant at .01.

[b]Equations (2.1) and (2.2) cover the years 1876-1937.
Equations (2.3) and (2.4) cover the years 1946-1966.

The national income indicator of prosperity has negative (though not significant) effects on both measures of strikes.[27] Consequently, short run hardship has a net impact on the frequency and magnitude of industrial conflict in prewar France. Similarly, coefficients of the expectations-achievements gap specification differ from those of recent economic studies. The wage coefficient has the wrong sign in equation (2.2) and is not significant in either. Conversely, the organizational/political variables have large effects on strike fluctuations in this period. Coefficients of union membership, number of cabinet changes and election year (dummy variable) are all in the expected direction, and only the cabinet change variable is not significant.[28] As expected, these results indicate the superiority of the organizational/political model of industrial conflict in pre-World War II France. They are also quite similar to findings for the United States during the comparable period.

Results for post-World War II France (equations 2.3 and 2.4) differ considerably from these earlier findings. In the postwar period, *neither* economic nor organizational/political variables are significant predictors of fluctuations in strike activity (except election year, which has the wrong sign in equation 2.4). Failure of the business prosperity and wage change indicators in these equations is expected from our argument. However, the small effects of unionization and political indicators are opposite expectations concerning (1) the explanatory power of the organizational/political model and (2) absence of change in the parameters from the prewar to the postwar period.

Italy

Table 3 presents equivalent equations for Italy, again estimated separately for the pre- and post-World War II periods (1901-1924 and 1947-1970, respectively).[29] Equations (3.1) and (3.2), covering the earlier period, yield results generally similar to those for the prewar years in France and the United States. Although coefficients of the national income and expectations-achievements gap variables are in the expected direction in both equations, neither is significantly different from zero. As in France and the United States, unionization is the strongest predictor of fluctuations in the frequency and size of industrial conflict. However, we find no significant influence of Shorter and Tilly's indicators of political change or crisis on industrial conflict (although coefficients of the election year and cabinet change variables have the expected positive signs).

Equations (3.3) and (3.4), covering the years from 1947 through 1970, do not show substantial change from the earlier period.[30] Effects of economic indicators and measures of political change on the frequency and size of industrial conflict are weak and often not in the expected direction. Union membership is again the only significant predictor of fluctuations in strike activity. This lack of evidence of change over time in the parameters is consistent with our argument concerning the institutional context of Italian labor relations. The relative success of the organizational/political model (which is, however, based almost exclusively on the performance of the unionization measure in these equations) follows from institutional arrangements which are still inconsistent with the

Hibbs, 1974b:266-9, for more complete discussion of this procedure).

[27] Reliable unemployment series for France and Italy begin in 1920 and 1922, respectively. Although it does not capture the labor market tightness component of prosperity so well as does unemployment, we use national income as the measure of prosperity. However, Skeels (1971) reports similar effects of these indicators in equations estimating several dimensions of industrial conflict.

[28] Unavailability of early national election data and shifting party programs and coalitions preclude use of "political attitude" variables comparable to those employed in the U.S. analyses. We stress that the cabinet change and election year variables indicate a *different* dimension (Shorter and Tilly's crisis or change) than the party affiliation variables used in the U.S. analyses.

[29] The large break in the series (1925-1946) corresponds to the absence of reliable strike information and unionization data for the Fascist period. Strikes were illegal and systematically suppressed under Mussolini (sketchy evidence of criminal prosecutions for participation in work stoppages indicates only about fifty strikes per year). These events suggest important influences of (especially repressive) government actions on strike activity (see Snyder, 1974, for strategies of measuring and modelling such actions).

[30] Original OLS estimation of equation (3.3) yielded significantly autocorrelated disturbances. That equation was re-estimated using the previously cited GLS procedure.

Table 3. Estimated Partial Regression Coefficients for Economic and Political Determinants of the Log Number of Strikes and Strikers: Italy (1901-1924; 1947-1970)[a]

		Dependent Variable	E_1	E_2	U	P_1	P_2	T	N	R^2	D-W
			Regression Coefficient (Standard Error)								
PREWAR[b]	(3.1)	Strikes (Logged)	.040 (.027)	-.014 (.019)	.131 (.026)**	.101 (.140)	.078 (.186)	-.101 (.040)**	24	.7061	1.70
	(3.2)	Strikers (Logged)	.005 (.034)	-.009 (.024)	.181 (.034)**	.061 (.180)	.109 (.239)	-.011 (.046)	24	.7097	2.14
POSTWAR[b]	(3.3)	Strikes (Logged)	-.0004 (.0020)	-.004 (.004)	.041 (.011)**	-.039 (.060)	-.021 (.069)	.059 (.034)	24	.8587	1.47
	(3.4)	Strikers (Logged)	.002 (.006)	.011 (.008)	.103 (.042)*	-.139 (.159)	.416 (.184)*	.024 (.113)	24	.5915	1.97

E_1 = national income (000's of lire); E_2 = six-year lagged moving average of % change in real wages ($\Sigma \Delta R_{t-i}$);

U = union membership as % of civilian labor force; P_1 = number of cabinet changes; P_2 = election year; T = time.

[a]For sources, see Appendix. *Significant at .05. **Significant at .01.

[b]Equations (3.1) and (3.2) cover the years 1900-1924.
Equations (3.3) and (3.4) cover the years 1947-1970.

assumptions underlying the bargaining approach and its economic models.

DISCUSSION AND CONCLUSIONS

These findings for France, Italy and the United States are generally consistent with our argument concerning the impact of institutional setting on the determinants of aggregate strike activity. In all three countries prior to World War II, the organizational/political model explains fluctuations in industrial conflict well. In each case, union membership as a measure of labor's organizational capacity for collective action is a significant predictor of the frequency and size of industrial conflict. Findings on political variables, however, indicate some variation by country in the prewar period. For France and Italy, effects of Shorter and Tilly's measures of political change and/or crisis on strike activity are only occasionally large. For the United States only, political party affiliations of congressional and presidential office holders have a strong impact on strike fluctuations from 1900 to 1948. These results suggest "attitudes" of government officials toward labor (Skeels, 1971), rather than the mere occurrence of change (Shorter and Tilly, 1974), as the political dimension most salient to explanations of industrial conflict. However, the omission of direct indicators of theoretically relevant political processes (e.g., repression) precludes conclusive evaluation of the relative importance of political components of the "organizational/political" model.

As expected from our argument, economic determinants of strikes perform poorly in the prewar period in all three countries. Under the roughly comparable institutional arrangements (on the dimensions we identify as important) which characterized labor relations in that earlier period, the assumptions underlying the bargaining models are invalid, and workers do not act as if they calculate short run economic costs and benefits in their decision to strike and participation in work stoppages. Therefore, for this early period the prosperity and expectations-achievements gap theories are inadequate.

Based on our institutional argument, we expect changes over time in the parameters affecting industrial conflict to the extent that corresponding shifts in labor relations occur. Findings for the United States from 1949 to 1970 are, relative to results for the earlier period, highly consistent with the expectation of substantial change. In the postwar years, unemployment and the expectations-achievements gap in real wage changes are both important predictors of strike fluctuations. These postwar findings indicate that workers act as if they calculate short run economic costs and benefits in their strike behavior, as recent theory and evidence suggest. Conversely, the organizational and political variables which explain fluctuations in strike activity through World War II are unimportant in these recent years. The results therefore support the expected changes over time in the determinants of industrial conflict in the United States.

Postwar results for the European countries are less conclusive. The Italian findings (strong impact of unionization and lack of important influence of economic and political change indicators) resemble those for the earlier period. Therefore, the expected superiority of organizational/political determinants for Italy in both periods holds only for unionization. For France since World War II, *neither* set of variables adequately explains fluctuations in strike activity. The poor performance of economic variables in these years follows from our argument. However, the lack of importance of union membership in postwar France is opposite our expectations of little or no change between the two periods.

Despite this single negative finding concerning the organizational/political model in postwar France, the results indicate a strong correspondence between institutional setting and the process underlying fluctuations in strike activity.[31] Differences in the institu-

[31] Given evidence of no change only for Italy, an alternative explanation of the results is that after World War II all industrialized countries experienced change in the process underlying strike activity (perhaps due to substantial shifts in economic structures). Although this alternative is consistent with Hibbs' (1974a) finding of no differences among types of bargaining systems in the postwar period, we consider it less plausible than ours because: (a) only one hypothesis derived from the institutional argument (positive effects of organizational/political determinants in postwar France) is not supported by the findings and (b) Hibbs characterizes bargaining systems on a dimension different from those we have identified (degree of centralization), thereby including France, Italy and the United States in the same category.

tional context of labor relations appear to account for international and intertemporal differences in empirical support for economic versus organizational/political models of industrial conflict. To the extent that national labor relations systems move toward increased size and stability of union membership, institutionalization of collective bargaining and political integration of labor, the determination of strike fluctuations shifts from a primarily political to an economic process.[32]

IMPLICATIONS

These economic and organizational/political models of industrial conflict have obvious parallels, respectively, in recent relative deprivation (Davies, 1962; Gurr, 1970) and "mobilization" or "power struggle" (Tilly, 1969; forthcoming; Snyder and Tilly, 1972; Oberschall, 1973) theories of collective violence. Based on conceptual and empirical similarities in studies of these two forms of conflict, we briefly list implications of our findings and conclusions for further work on collective protest.

Relative Deprivation Versus Power Struggle Explanations

The importance of union membership throughout our analyses indicates that deprivation theory's emphasis on individual psychology underestimates the importance of organizational capacity for collective action. Although this criticism has been made on substantive grounds (Snyder and Tilly, 1972), difficulties in measurement of organizational strength have left it largely unsupported empirically. However, the findings also suggest that, *given* this organizational capacity, expectations-achievements gap explanations of fluctuations in collective protest perform well.[33]

Analysis of Groups Versus Events

Empirical studies of collective violence focus almost exclusively on analyses of violent events (Davies, 1962; Gurr, 1970; Snyder and Tilly, 1972). Even where these analyses use time ordered data, they are static in that groups are considered only during periods when they become involved in (violent) protest. One strategic implication of our analyses is that the process underlying collective violence may be better understood by following groups *throughout* their "life histories," thereby using groups rather than events as units of analysis.

Time Dependent Processes

A final strategic implication of this study concerns identification of time dependent processes. Substantive work on determinants of collective violence (as well as more general propositions about social conflict) has been particularly intractable in its assumptions of temporal and spatial invariance in conflict processes. Our results indicate substantial change over time in parameters affecting conflict behavior, and therefore challenge such assumptions. Continued failure of analyses of conflict to consider time dependence *and* the substantive dimensions which produce it appears unwarranted.

APPENDIX: DATA SOURCES

THE UNITED STATES

Number of Strikes and Strikers: Griffin (1939:38,44), for years 1881-1937; U.S. Department of Labor (1972:335), for years 1938-1970.

Unemployment: U.S. Bureau of the Census (1960:73), for years 1900-1957; U.S. Department of Labor (1972:26), for years 1958-1970.

Real Wages (computed from money wage and price statistics): Rees (1959:15-6), for years

[32] We do not imply that such changes are necessarily evolutionary or irreversible. We also recognize that concepts such as political position of labor and institutionalization of collective bargaining are not dichotomous (as we have treated them in the analyses and discussion). Empirical representation of these conceptual variables is important for further testing and specification of the argument.

[33] More general applicability of this second point may be limited because other types of protest (e.g., collective violence) may, to a much greater extent than has strike activity, change *form* after organizational strength is less problematic. For further discussion of this issue, as well as suggestions for merging these alternatives within a single conceptual framework, see Snyder (1974:272-84).

1900-1957; U.S. Department of Labor (1972:223, 276), for years 1958-1970.
Union Membership: Troy (1965:93-4), for years 1900-1962; U.S. Department of Labor (1972:333), for years 1963-1970.
Percent Democrats in Congress (third party affiliations excluded from calculations): U.S. Bureau of the Census (1960:691; 1972:367), for years 1900-1956 and 1957-1970, respectively.
Party of President (dummy variable: 1=Democrat; 0=Republican): U.S. Bureau of the Census (1960:691; 1972:367), for years 1900-1956 and 1957-1970, respectively.

FRANCE
Number of Strikes and Strikers: Perrot (1971:61), for years 1876-1889; Shorter and Tilly (1974:361-3), for years 1890-1937; 1946-1966.
National Income: Lévy-Leboyer (1968:804), for years 1876-1900; France, INSEE (1966:561; 1972:654), for years 1901-1937, 1946-1964; and 1965-1966, respectively.
Real Wages (computed from money wage and price statistics): Singer-Kérel (1963:534-7), for years 1876-1937; 1946-1954. France, INSEE, various issues, for years 1955-1966.
Union Membership: Perrot (1971:633), for years 1876-1889; France, INSEE (1938:58*), for years 1890-1914; For years 1915-1925, data aggregated from totals by labor organization (confederation) reported in Labi (1964:Appendix), Branciard (1967:40, 57-8), Dufraisse (1966, throughout text) and Thorel (1947:128-31); Prost (1964:177-96), for years 1926-1937; for years 1946-1966, data aggregated from totals by confederation reported in U.S. Department of Labor (1954-1965), Sellier and Tiano (1962:350), Delasalle (1965:145), Reynaud (1966:128-31) and Branciard (1967:143).
Number of Cabinet Changes: various political sources.
Election Year (dummy variable: 1=yes; 0=no): various political sources.

ITALY
Number of Strikes and Strikers: L. Tilly (n.d.: 13-4), for years 1901-1924; Italy, Confederazione Generale (1949:397), for years 1946-1948; Italy, Ministero dell'Agricoltura (1950-1971), various issues, for years 1949-1970.
National Income: Italy, Istituto Centrale (1958:211), for years 1901-1924, 1946-1955; Ministero dell'Agricoltura (1958-1972), various issues, for years 1956-1970.
Real Wages: Vannutelli (1961:570-1), for years 1901-1924, 1946-1959; Ministero dell'Agricoltura (1961-1972), various issues, for years 1960-1970.
Union Membership: Candeloro (1950:50, 65, 81-2, 86, 94); Horowitz (1963:75, 100-24); Rigola (1947, throughout text); Italy, Ufficio del Lavoro (1904-1920), various issues, for years 1901-1924; U.S. Department of Labor (1954-1965), various issues; Gradilone (1959); Italy, Centro Italiano (1965-1972), various issues; Horowitz (1963:208, 224, 299-303); and Candeloro (1950:179), for years 1947-1970.
Number of Cabinet Changes: various political sources.
Election Year (dummy variable: 1=yes; 0=no): various political sources.

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TEMPORAL CHANGE IN OCCUPATIONAL MOBILITY: EVIDENCE FOR MEN IN THE UNITED STATES*

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In a reanalysis of trend data on occupational mobility among men in the United States, we define ways in which movement among occupation groups across generations might be constant even when the occupational structure is changing. Our analysis suggests that no change has taken place in occupational mobility (as specified by our definition). Rather, the changing occupational distribution is the major factor affecting patterns of intergenerational occupational mobility. This implies the possibility of constructing occupational mobility tables for times when the age-specific occupation distribution is known, but no mobility survey has been carried out. Moreover, rather than treating the underlying process of mobility as a variable in comparative research and the variation in the distribution of occupations as a disturbance, it may be more fruitful to treat transformations of the occupational structure as problematic in comparative mobility research.

Trends in intergenerational occupational mobility or stratification have long been subjects of interest and controversy among sociologists. There are numerous reasons for an interest in mobility trends: a concern with the prospects for social equality or equality of opportunity, efforts to understand the transformation of the labor force in economic development, attempts to analyze the rise and fall of groups competing for power. Research and writing about mobility trends have often suffered from conceptual or analytic failures to separate the several aspects of stratification and mobility. Our purpose is to provide empirical estimates of mobility trends among U.S. men which avoid this confusion.

Variations in professional opinion about occupational mobility trends in the United States have probably been greater than any well-documented trends (Duncan, 1968: 675-80). Indeed, students of social mobility have reached no greater consensus on the matter than has the society they have sought to enlighten. Relying on the few available data, or in some cases on no data, mobility analysts have concluded that American society is becoming more rigid, that it is not becoming more rigid, that there has been no change in rates of mobility, or that we are moving toward a situation of full equality of opportunity. That observers have reached disparate conclusions from the same statistics is a problem in the sociology of knowledge (see Koffel, 1974). In other cases it may be possible to trace differences in conclusions about mobility trends to differences among data and statistical measures applied to them. We shall explore the latter possibility.

Relatively few facts are available about

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trends in intergenerational occupational mobility among U.S. men. Opportunities to enter high status occupations appear to have improved in successive cohorts of U.S. men for at least the last 40 years, irrespective of those men's occupational origins (Duncan, 1965; Hauser and Featherman, 1973; 1974a; 1974b). There is less evidence about changes in the ease of movement among occupational positions from one generation to the next, but a serious and comprehensive effort to assess trend from available data has suggested that the dependence of son's on father's occupation has been remarkably stable for more than half a century (Duncan, 1968; but see Duncan, 1966; Jackson and Crockett, 1964; Blau and Duncan, 1967; Tully et al., 1970). For related evidence of occupational mobility trends among U.S. women, see Featherman and Hauser (1974) and Hauser, Featherman and Hogan (1974).

In this paper we re-examine trend data on occupational mobility among men in the United States. Our analysis of temporal change follows the traditional interest of social mobility analysts in separating parameters of the occupational structure from the process of occupational mobility. This long-standing concern is well expressed by Raymond Boudon (1973) in his exhaustive review of mobility measures: "...a good mobility index should make a distinction between the amount of mobility generated by the changes in the social structure and the amount of mobility generated by other factors. Indeed, the former should be eliminated." Andrea Tyree (1973) has ably documented the parallel arguments by which several authors mistakenly hit upon the ratios of actual frequencies in a mobility table to those expected under independence as indices of "social distance mobility." These ratios are defective because the index for each cell in a mobility table varies inversely with the marginal proportion in its row and column and because the set of such ratios in a table determines both the row and column marginal distributions up to a constant of proportionality (Duncan, 1966; also see Goodman, 1969b). Thus, social distance mobility ratios are intimately related to the marginal distributions of the mobility table from whose influence they were supposedly freed.

Applying the work of Leo Goodman (1969a; 1969b; 1970; 1972a; 1972b) and Otis Dudley Duncan (1966) we shall define particular ways in which the pattern of movement among occupation groups across generations might be constant even when the occupational structure is changing. (Also see Haberman, 1974a:215-27.) We then reanalyze most (but not all) of the existing trend data on father to son occupational mobility in the United States, and our analysis suggests that no change has taken place in occupational mobility (as specified by our definition). That is, there is minimal evidence of change in the process of occupational mobility beyond that induced by the changing occupational structure and the succession of cohorts. This suggests a possible redirection of comparative studies of occupational mobility. It is no longer possible to assume that the underlying process of mobility is problematic in comparative analysis, while variations in the occupation distribution are a nuisance factor or disturbance. Rather, attention should be directed both to the causes of shifts in the occupation distribution and to their consequences in rates and patterns of mobility. We will take up the latter issue in a sequel to the present article (Hauser et al., 1975).

A Multiplicative Model

Suppose we observe tables of son's occupation by father's occupation at several different times. Denote the categories of father's occupation by P(i=1, . . .,I), those of son's occupation by S(j=1, . . .,J), and those of time by T(k=1, . . .,K). We may specify the observed frequencies, f_{ijk}, in the three-way classification of P by S by T by the multiplicative identity,

$$f_{ijk} = \eta\, \tau_i^P \tau_j^S \tau_k^T \tau_{ij}^{PS} \tau_{ik}^{PT} \tau_{jk}^{ST} \tau_{ijk}^{PST} \cdot (1)$$

Here, η is the geometric mean of the f_{ijk}, and the τ-parameters pertain to the probability that an observation appears in the subscripted cell of the superscripted univariate or joint distribution (Goodman, 1972). Thus, the parameters τ_i^P, τ_j^S, τ_k^T pertain to the respective probabilities that an observation appears in the ith, j^{th}, or k^{th} cell of the marginal distributions of father's occupation, son's occupation, or time, relative to the grand

mean. The parameters τ_{ij}^{PS}, τ_{ik}^{PT}, and τ_{jk}^{ST} pertain to the respective chances that an observation appears in the ij^{th}, ik^{th}, or jk^{th} cell of the marginal classifications of father's occupation by son's occupation, father's occupation by time, or son's occupation by time, relative to the probabilities given by the products of lower-order parameters. Finally, the parameters τ_{ijk}^{PST} pertain to the probability that an observation appears in the ijk^{th} cell of the complete three-way classification, relative to the probability given by products of lower-order parameters. (For an introductory discussion of multiplicative models for contingency tables, see Ku and Kullback, 1974.)

We are not especially interested in the complete or saturated model given by equation 1, but rather with the possibility that a more parsimonious model will accurately reproduce the observed cross-classification. According to our first alternative,

$$F_{ijk} = \eta\, \tau_i^P \tau_j^S \tau_k^T \tau_{ik}^{PT} \tau_{jk}^{ST}, \qquad (2)$$

where F_{ijk} is the expected frequency in the ijk^{th} cell. The hypothesis expressed by the model is that the distributions of father's and son's occupations are determined exogenously at each time, and father's and son's occupations are conditionally independent. That is, equation 2 gives the same model as equation 1, except $\tau_{ij}^{PS} = \tau_{ijk}^{PST} = 1$ for all i, j and k. Using Leo Goodman's computer program, ECTA, or other available programs, it is a routine matter to obtain maximum-likelihood estimates of the F_{ijk} and to run a likelihood-ratio (LR) χ^2 test of the departure of the F_{ijk} from the f_{ijk}. A large value of χ^2_{LR} indicates that net of change in occupational structure there is association between father's occupation and son's occupation, or that association varies over time, or both. (For a lucid account of statistical inference in log-linear models, see Davis, 1974.) We do not believe the model of equation 2, since it postulates the statistical independence of father's and son's occupation at each time. (The χ^2_{LR} for this test may be written as the sum over time periods of the χ^2_{LR} for the test of independence between father's and son's occupations at each time.) Rather, it represents a set of baseline conditions of temporal change in the marginal distributions of father's and son's occupation against which we may assess and compare the associations between father's and son's occupations net of shifting occupation distributions.

We can also supplement the model of equation 2 to reflect temporally constant associations between son's and father's occupations:

$$F_{ijk} = \eta\, \tau_i^P \tau_j^S \tau_k^T \tau_{ij}^{PS} \tau_{ik}^{PT} \tau_{jk}^{ST}. \qquad (3)$$

This differs from the model of equation 1 in that $\tau_{ijk}^{PST} = 1$ for all i, j and k, and it differs from the model of equation 2, because the τ_{ij}^{PS} are not constrained to equal one. Equation 3 corresponds to a substantively interesting null hypothesis: there are changes over time in the marginal distributions of father's and son's occupations, but the latter associations are invariant with respect to time. Again, we may obtain maximum-likelihood estimates of the frequencies under the model of equation 3 and run a χ^2_{LR} test of goodness of fit. Here, a large value of χ^2_{LR} indicates the association between father's occupation and son's occupation is not constant across time (after controlling change over time in the occupational structure). Moreover, because the model in equation 3 includes all of the parameters of the model in equation 2, plus the parameters of the father-son association, the difference between the χ^2_{LR} for the model of equation 2 and that of equation 3 gives a test of the null hypothesis that there is no temporally constant association between father's occupation and son's occupation (after controlling change over time in the occupational structure).

While our analyses are not limited to the models specified in equations 2 and 3, all of our analysis does follow the same pattern. That is, in looking at a mobility table (or some aspect of it) we take change in the distributions of father's occupation and of son's occupation as a baseline condition. Then we measure the extent of temporally constant and of temporally variable association be-

tween father's occupation and son's occupation.

An illustration may clarify the implications of the model in equation 3. Suppose at time k the observed frequencies in the sub-table classifying a pair of categories of father's occupation by a pair of categories of son's occupation are as follows:

Father's occupation	Son's occupation 1	2
1	f_{11k}	f_{12k}
2	f_{21k}	f_{22k}.

The model of equation 3 specifies that the odds-ratio

$$\frac{(f_{11k}/f_{12k})}{(f_{21k}/f_{22k})}=\frac{(f_{11k}/f_{21k})}{(f_{12k}/f_{22k})}=\frac{f_{11k}f_{22k}}{f_{12k}f_{21k}} \qquad (4)$$

is the same for all k within the limits of sampling error. For example, expression 4 might describe the chance that the son of a white-collar worker became a white-collar worker rather than a blue-collar worker relative to the chance that the son of a blue-collar worker became a white-collar worker rather than a blue-collar worker. The model of equation 3 says that odds-ratio does not vary across time. Moreover, the model says this invariance over time in the odds-ratios holds for any pair of categories of father's occupation and for any pair of categories of son's occupation, which need not be the same pair of father's and son's occupations. In general, the models investigated here may be described in terms of odds-ratios or certain functions of them, which may or may not vary across time. An implication of the model with temporally constant odds-ratios is that if we observe a table of father's occupation by son's occupation at any one time, and if we know the marginal distributions of son's occupation and of father's occupation at a second time, we can estimate the frequencies in the cross-classification of father's occupation by son's occupation at the second time by a simple iterative procedure (for example, see Deming, 1943; Mosteller, 1968).

In the following section we apply the models given by equations 2 and 3 and other instructive models in a reanalysis of available data on trends in occupational mobility among U.S. men. We shall see that models which are similar to equation 3 fit the data rather well. It should be noted that our choice of years, ages or time periods forms a part of the null hypothesis. That is, aggregation in the temporal dimension of our three-way classification may affect the association between father's occupation and son's occupation within time periods and its variation over time periods. However, our major empirical result does appear to stand up under alternative representations of the temporal dimension.

1962 OCG: Mobility to First Job

Figure 1 gives the names of 12 major occupation groups which combine detailed titles in the 1960 classification system of the U.S. Bureau of the Census. The groups are arrayed in descending order of the socioeconomic status of constituent occupations on the Duncan scale (1961), but much of our analysis requires no assumptions about hierarchy in the occupational structure. Using the classification scheme of Figure 1, we have tabulated son's first full-time civilian occupation after leaving school for the last time by father's occupation when the son was about 16 years old in each of nine 5-year cohorts covered in the March 1962 Current Population Survey supplement, "Occupational Changes in a Generation" (OCG; see Blau and Duncan, 1967). Our analyses pertain to 17,200 male OCG respondents who reported father's occupation and own first occupation. Since father's occupation refers to the sixteenth birthday of the son, while the son's first occupation refers to a fixed point in the son's career (even though son's age at first job varies), comparisons of the nine mobility tables for men of different ages in 1962 represent true intercohort comparisons, flawed only by the possibilities of differential mortality and recall.

We have proportionately adjusted values of χ^2_{LR} for the OCG data downward by a factor of 0.62 to reflect the efficiency of the Current Population Survey sample design relative to simple random sampling. This design factor is based on published standard errors of percentages (Blau and Duncan, 1967:477), but the

Figure 1. Twelve Category Occupation Distribution, U. S. Data from "Occupational Changes in a Generation"

Occupation Group	Duncan SEI
Professional, technical and kindred workers, self-employed	84
Professional, technical and kindred workers, salaried and others	73
Managers, officials and proprietors, exc. farm, salaried	68
Sales workers	49
Managers, officials and proprietors, exc. farm, self-employed	47
Clerical and kindred workers	45
Craftsmen, foremen and kindred workers	31
Operatives and kindred workers	18
Service workers, including private household	17
Farmers and farm managers	14
Farm laborers and foremen	9
Laborers, except farm and mine	7

efficiency of the CPS design may be greater for more complex statistics like those used here (Kish and Frankel, 1974). Thus, our selection of a design factor may incorrectly reduce the likelihood of our rejecting the null hypothesis when it is false. This potential bias may be offset because we have not made an adjustment for simultaneous inference. The effect of this adjustment on our analysis could be large. For example, Table 1 reports 16 tests of significance (specified without looking at the data). Thus, if we wished to maintain an overall significance level of $p = 0.05$ in the set of analyses reported in Table 1, we should reach a nominal probability level of $0.05/16 = 0.003$ in any one test before rejecting the null hypothesis in that test (Goodman, 1969a: 8-10). Moreover, even after the application of our design factor, the OCG sample is so large that we run a substantial risk of rejecting null hypotheses in favor of trivial alternatives at conventional levels of statistical significance.

Table 1 summarizes our analyses of the classification of father's occupation by son's first occupation by age in the 1962 OCG data. There is a line in the table for each model or comparison among models. The left-hand column gives a verbal statement of the null hypothesis, and the columns to the right report a goodness-of-fit test and other descriptive results. In reading this and subsequent tables, it will be helpful to recall that χ^2 with ν degrees of freedom has expectation ν and variance 2ν and $\chi^2(\nu)$ is distributed normally when ν is large. It is also helpful to look at the ratio, $\chi^2(\nu)/\nu$, in assessing the size of a component of association relative to others based on the same number of observations (Goodman, 1972a:1058; Haberman, 1974b: 592-3).

Panel A of Table 1 reports the analysis of change over time in the occupational distributions of fathers and sons. Clearly, those distributions have changed over the half-century represented in the OCG data. Against the null hypothesis of temporally constant occupation distributions, we obtain χ^2_{LR} of 432.22 in the case of father's occupation and 429.95 in the case of son's first occupation. The marginal tables of occupation by time each have 88 degrees of freedom, so the changes over time in the occupation distributions are statistically significant. (We thank Keith Hope for recommending these two calculations. See Hope, 1974, for an elegant analysis of mobility trends in British society.)

In line B1 we estimate the baseline model of equation 2. This model makes the temporal

Table 1. Analyses of Temporal Homogeneity in Mobility from Father's Occupation to Son's First Full Time Occupation: U. S. Men Aged 20 - 64 in 1962 by 5-Year Cohorts (N = 17,200)

Null Hypothesis	χ^2_{LR}	df	p	Δ	χ^2_H/χ^2_T
A. Marginal trends					
1. Constant distribution of father's occupation	432.22	88	.000	--	--
2. Constant distribution of son's first occupation	429.95	88	.000	--	--
B. Full matrix (12 x 12)					
1. Margins vary with time (age)	5344.57	1089	.000	26.31	100.0%
2. Margins vary with time (age), constant father-son interactions	854.19	968	>0.5	8.56	16.0
3. No father-son interaction (B2 vs. B1)	4490.38	121	.000	17.75	84.0
C. Main diagonal blocked					
1. Margins vary with time (age)	3512.87	981	.000	18.94	65.7
2. Margins vary with time (age, constant father-son interactions	739.99	872	>0.5	7.03	13.8
3. No father-son interaction (C2 vs. C1)	2772.88	109	.000	11.91	51.9
D. Five broad groups blocked					
1. Margins vary with time (age)	1213.48	801	.000	8.71	22.7
2. Margins vary with time (age), constant father-son interactions	593.93	712	>0.5	5.56	11.1
3. No father-son interaction (D2 vs. D1)	619.55	89	.000	3.15	11.6
E. Selected tests for trend					
1. Constant occupational inheritance (C2 vs. B2)	114.20	96	>0.1	1.53	2.2
2. Constant interactions within major groups off the main diagonal (C2 vs. D2)	146.06	160	>0.5	1.47	2.7
3. Constant mobility	18.12	8	.022	0.24	0.3
4. Constant upward and downward mobility	25.54	16	.064	0.28	0.5
5. Constant occupation-specific inheritance	96.09	88	.271	1.29	1.8

changes in the occupation distributions of fathers and sons a part of the null hypothesis, but it also attempts to account for all of the frequencies in the three-way classification of father's occupation by son's first occupation by age. Against the baseline model, we obtain χ^2_{LR} of 5344.57 with 1089 degrees of freedom, which is obviously statistically significant at any of the conventional levels. We reject the model with temporally variable marginals but no association between father's occupation and son's first occupation.

The column labeled Δ gives the index of dissimilarity between the distributions of observed frequencies and the maximum-likelihood estimates. (See Taeuber and Taeuber, 1965:195-245, for a discussion of Δ and related measures, and Goodman, 1965, for a related application.) In contrasts between models and in tests for trend, the value of Δ measures the improvement in fit between expected and observed distributions. Values of Δ may legitimately be compared within Table 1 and within later tables, but not between tables, because the index is sensitive to differing levels of aggregation. The model of line B1 misclassifies 26.31 percent of the distribution. This is the percentage of cases which would be shifted to another cell of the classification by a proper specification of the relationship between father's occupation and son's first occupation and changes in it over time.

Finally, the entry of 100.0% in the column labeled χ^2_H/χ^2_T indicates that we take χ^2_{LR} = 5344.57 to represent all of the variation or association in the data which we might wish to explain by subsequent complications of our baseline model. That is, we shall treat the baseline χ^2_{LR} just like the total sum of squares in a conventional analysis of variance. This analogy is developed extensively by Goodman (1970; 1971; 1972a). However, in the present context, explaining all of the variance does not imply accounting for inter-unit variability, but fitting a joint frequency distribution. Thus, the present effort has more in common with the testing of overidentifying restrictions in a path model than with increasing the size of a multiple correlation.

When we estimate the model of equation 3, we obtain the results shown in line B2 of Table 1. Under the hypothesis of temporally constant father's occupation-son's first occupation interactions, we obtain χ^2_{LR} = 854.19 with df = 968. (By "interactions" in this context, we refer to the multiplicative parameters of association between father's occupation and son's first occupation.) Our test statistic is well below its expected value, so clearly we do not reject the null hypothesis. The model of temporally constant father-son interactions accounts for all but 16 percent of the variation in the baseline model, and it misclassifies only 8.56 percent of the distribution.

It is instructive to compare lines A1 and A2 of Table 1 with line B2, for these, respectively, represent change over time in father's occupation, in son's occupation and in their association. The χ^2_{LR} in line B2 is almost as large as the sum of the corresponding entries in lines A1 and A2 but this is misleading because the χ^2_{LR} in line B2 uses many more degrees of freedom. Note the χ^2_{LR} in line A1 and in line A2 is each about 5 times larger than its expectation, while the χ^2_{LR} in line B2 is less than its expected value. (Adjusting components of χ^2_{LR} for their differing degrees of freedom is similar to adjusting R^2, the coefficient of determination, for loss of degrees of freedom.) Thus, we conclude that changes over time in the distributions of father's occupation and of son's first occupation are far more important than changes over time in the association between father's occupation and son's first occupation.

In line B3 we report the differences between corresponding entries in lines B1 and B2. This gives us a test of the hypothesis of no father's occupation-son's first occupation interactions, given son's birth cohort. As might be expected, we obtain a large and statistically significant χ^2_{LR} which accounts for 84 percent of the variation in the baseline model and correctly classifies an additional 17.75 percent of the distribution.

While we have not detected any changes in father-son occupation interactions in the analysis of panel B, we are not yet content to conclude that no change has occurred. The failure to reject null in a global test does not imply that a more narrowly specified hypothesis will not be rejected. For this reason, we have specified and tested a series of hypotheses about changes in the relationship between

father's and son's occupation which involve occupational mobility and/or occupational inheritance or disinheritance. It should be kept in mind that "inheritance" does not here refer to job inheritance in the strict sense, but only to the possibility that a son may enter the same major occupation group as his father (Goodman, 1969a:14).

In panel C of Table 1 we test a series of models following the same logic as in panel B, but we block the entries along the main diagonal of each father-son mobility table. That is, we constrain the entries involving occupational inheritance, forcing each frequency on the main diagonal to take on its observed value. In this way, the null hypothesis presumes the observed pattern of occupational inheritance in each time period. Thus, entries in panel C of Table 1 differ from corresponding entries in panel B by excluding the effects of departures from the null hypothesis where father and son are in the same major occupation group. By comparing entries in panels B and C, we will be able to isolate the effects of occupational mobility and changes therein from the effects of differential occupational inheritance and changes therein (Goodman, 1969a:29-39).

In line C1, we fit the model of temporally-variable margins with no father-son interactions off the main diagonal. Clearly, this model can be rejected, although the main diagonals do account for about 35 percent of the association in the tables (compare lines B1 and C1). In line C2, we fit the model with observed frequencies in the main diagonals and a temporally constant set of father-son interactions off the diagonal. Goodman (1969a:29-30) uses the term "quasi-homogeneity" to refer to models in which two (or more) cross-classifications are specified to be alike in some interactions but not in others. In the model of line C2, patterns of occupational mobility, but not of occupational inheritance, are assumed stable across time. Here, χ^2_{LR} is much less than its expectation, and we are unable to reject the model of quasi-homogeneity, i.e., temporally constant father-son interactions off the main diagonal. Line C3 contrasts the models of lines C1 and C2, and from it we conclude that there is a significant set of father-son interactions off the main diagonal. Thus, while the cross-classification is quasi-homogeneous in respect to patterns of occupational mobility, it does not fit the model of quasi-perfect mobility or quasi-independence (White, 1963; Goodman, 1965; 1968; 1969a), at least in respect to the distinction between occupational inheritance and occupational mobility as defined by the 12 major occupational groups. Indeed, the temporally constant, off-diagonal interactions account for more of the variation from the baseline model than do the observed frequencies in the main diagonals. (Compare line C3 with the difference between lines B1 and C1.)

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In line E1 of Table 1, we contrast the models of line B2 and line C2. Since the former model posits a temporally constant pattern of association between father's and son's occupations, and the latter differs only in permitting occupational inheritance to fluctuate with time, the comparison in line E1 tells whether the pattern of occupational inheritance has changed over time. Changes in occupational inheritance account for only 2.2 percent of the association in the table, and they reclassify only 1.5 percent of the distribution. Effects this small might have occurred by chance more than 10 percent of the time. Thus, we may specify our earlier finding of no significant changes in father-son interactions to say there have been no changes of association in the aggregate of cells involving occupational inheritance or in the aggregate of cells involving occupational mobility.

In the analyses of panel D of Table 1, we

again block the main diagonal entries of each father-son mobility table, and additionally we block cells off the main diagonal of the tables which pertain to movement within five very broad occupation groups. The broad occupation groups are upper white-collar (professional, technical and kindred workers; managers, officials and proprietors, except farm), lower white-collar (sales workers; clerical and kindred workers), upper blue-collar (craftsmen, foremen and kindred workers; operatives and kindred workers), lower blue-collars (service workers, including private household; laborers, except farm and mine), and farm (farmers and farm managers; farm laborers and foremen). As before, when we block cells within the five broad groups (including the main diagonal), we are constraining the model to reproduce exactly the observed frequencies in those cells in each time period. Thus, the null hypotheses in panel D presume the observed patterns of association within the five broad groups and changes in them over time. Looking at panel D, we can assess the amount of association between father's occupation and son's occupation outside the five broad groups; the null hypothesis here (line D3) is quasi-perfect mobility, but in a smaller subset of cells than in the analysis of panel C. Also, we can assess changes over time in association outside the five broad groups, testing the null hypothesis of quasi-homogeneity (line D2), but in a smaller subset of cells than in the analyses of panel C. Finally, by comparing corresponding entries in panels D and C, we can assess the amount and changes in the amount of association between father's occupation and son's first occupation which involves sons in the same broad group, but not the same major group as their fathers.

As shown in line D1, even when we block all of the cells within these very broad occupation groups, there remains a statistically significant and substantial amount of association in the tables, about a quarter of the association in the complete tables. In line D2, we estimate a model which contains temporally constant father-son occupation interactions outside the five broad occupation groups, and χ^2_{LR} is less than its expected value. We are unable to reject this model of quasi-homogeneity, which misclassifies only 5.56 percent of the distribution and accounts for almost 89 percent of the association in the baseline model. As reported in line D3, the temporally constant interactions outside the five broad groups account for a statistically significant 11.6 percent of the association in the baseline model, thus permitting us to reject this weaker statement of the hypothesis of quasi-perfect mobility. Finally, in line E2, we contrast the models of lines C2 and D2, which differ only in that the latter permits temporal variation in the association between father's and son's occupation in cells which are off the main diagonal but within the five broad groups defined above. The χ^2_{LR} in line E2 is well below its expected value, so we are unable to reject the hypothesis of temporal homogeneity in the association between father's occupation and son's occupation off the main diagonal, but within the five broad occupation groups. Thus, our analysis continues to support the hypothesis of temporal homogeneity in occupational mobility between generations, and at the same time, it fails to support the hypothesis of quasi-perfect mobility outside the five broad groups.

Panel E of Table 1 reports other tests for trend in mobility which are related to, but distinct from the earlier ones. Line E3 tests the hypothesis that the pattern of mobility from father's occupation to son's first occupation is the same in every cohort, except the rate of mobility, i.e., the propensity to move versus stay, has changed over time. This model differs from the model of line E1 in introducing time-varying parameters for occupational mobility *per se*, rather than the several occupation-specific parameters of the model in line E1. Our mobility parameter is a special case of the "triangles" parameters described by Goodman (1972b), but our model under the null hypothesis is not a triangles model, but the model of equation 3. If the alternative hypothesis were true, the model of equation 3 would have to be supplemented by time-varying mobility parameters. The addition of the time-varying parameters is nominally significant at the 0.022 level. The changing mobility parameters account for only 0.3 percent of the association in the baseline model, and they allocate only 0.24 percent of the distribution to a different cell in the classification. We regard this effect as substantively trivial and possibly random for the reasons given earlier.

Under the alternative hypothesis just dis-

cussed, the mobility parameter only makes the distinction between movers and stayers, thus neglecting the possibility that propensities to move upward and downward may change over time in different directions. That is, offsetting changes in upward and downward mobility rates may yield no change in overall mobility rates. In line E4 of Table 1 we test the hypothesis that rates of upward or downward mobility have changed (as well as rates of stability). To define "upward" or "downward" mobility (a distinction not used in any earlier model), we ordered the major occupation groups by Duncan scores (see Figure 1), and we introduced separate time-varying parameters for the triangular aggregates of cells pertaining to upward and to downward mobility. However, the model under the null hypothesis is not a triangles model, but that of equation 3. In line E4, the χ^2_{LR} for changes in upward and downward mobility is not even significant at a nominal 0.05 level. Moreover, there is no significant difference between the changes over time in upward and in downward mobility. Changes over time in rates of upward relative to downward mobility account for a χ^2_{LR} of only 7.42 with 8 degrees of freedom, and they account for only 0.2 percent of the association in the baseline model (compare lines E3 and E4 of Table 1).

Finally, in line E5 of Table 1 we report a test of changes across time in occupation-specific inheritance. That is, we ask whether there are any changes over time in propensities to inherit specific occupations, aside from possible trends in propensity toward mobility *per se* (tested in line E3). The reader will note this test contrasts the model of line E1 with that of line E3. That is, changing occupational inheritance permits changes both in general and occupation-specific propensities to move. Again, as shown in line E5, we fail to reject the null hypothesis of temporal homogeneity in occupation-specific inheritance. By an extension of the methods used here it would be possible to assess changes over time in occupational inheritance in each major occupation group, but we have not done so.

It may be useful to summarize our analyses of mobility from father's occupation to son's first occupation by cohort in the 1962 OCG data. First, we have found substantial change in the occupational structure over time, as evidenced by change in the distributions of father's occupation and of son's occupation. Second, we have found strong patterns of association between father's occupation and son's occupation. Considering only the temporally constant association between father's occupation and son's first occupation, the ratios of χ^2_{LR} to its degrees of freedom are 143.13 for the aggregate of cells on the main diagonal, 107.67 for the aggregate of cells off the main diagonal but within the five broad occupation groups, and 6.96 for the aggregate of cells outside the five broad occupation groups. While much of the association in the tables is attributable to occupational inheritance or to movement between closely related occupation groups, there appears to be association throughout most, if not all of the mobility table. Thus, the OCG data on mobility to first occupations do not appear to conform to the pattern of quasi-perfect mobility which Goodman (1965; 1969a; 1969b) has observed in British and Danish mobility tables. This may partly be a consequence of the larger number of occupation groups employed in the present analysis (Haberman, 1974a:216), or perhaps we have not located the subset of cells in which quasi-perfect mobility holds.

Third, we have found remarkable homogeneity in the patterns of association between

Figure 2. Ten Category Occupation Distribution, Indianapolis Data from Rogoff's Recent Trends in Occupational Mobility

Professional

Semi-professional

Proprietors, managers, officials

Clerks and salesmen

Skilled

Semi-skilled

Protective service

Farming

Personal service

Unskilled

father's occupation and son's first occupation when changes in the occupational structure have been controlled. There appear to be negligible differences among cohorts in the propensity to move versus stay; in the propensities to move up, move down, or stay; in the propensity to move up relative to the propensity to move down; in the propensity to inherit one's father's occupation; in patterns of movement among similar occupation groups; and in patterns of movement among disssimilar occupation groups.

Blau and Duncan (1967:107-11) have preceded us in attempting to measure mobility trends by comparing tables of mobility from father's occupation to son's first occupation among men of different ages in the 1962 OCG survey. They compared 4 ten-year age cohorts using a three-category occupation classification (manual, nonmanual, farm), a ten-category classification and scores of detailed titles on Duncan's scale (1961). From standard mobility indexes and contingency measures in the three by three and ten by ten classifications, Blau and Duncan (1967:109) concluded, "the extent of association between origin and destination has been less recently than at an earlier date." However, the product-moment correlations between status scores of fathers and sons are virtually the same in each cohort. We shall see that at least the first of these results may be consistent with the hypothesis of temporal invariance which we have advanced (Hauser et al., 1975).

Five National Surveys: 1947 to 1972

Much of the recent analysis of trends in occupational mobility in the U.S. rests on comparisons among four national surveys in which both father's occupation and son's current occupation were ascertained. These are a 1947 survey by the National Opinion Research Center (NORC), 1952 to 1957 surveys by the Survey Research Center (SRC) at the University of Michigan and the 1962 OCG survey carried out by the U.S. Bureau of the Census. The first three surveys were compared by Jackson and Crockett (1964), and the last was added to the series by Blau and Duncan (1967:97-105). We have added a fifth observation to the series of mobility tables for all U.S. men. This is a table for about 500 adult men in 1972 obtained from an NORC (1972) survey.

Jackson and Crockett emphasized the lack of evidence of increasing rigidity in the stratification system and suggested there may have been some lessening of the dependence of son's on father's occupation. They conclude (Jackson and Crockett, 1964:15), "The data suggest, however, that the rate of occupational mobility in the United States has increased somewhat since the end of World War II. At the least, we found scant evidence that the system of occupational inheritance is growing more rigid." Blau and Duncan (1967:105) reached a similar conclusion.

There are serious questions about the comparability of data across years since there was some variation in the items used to ascertain occupation, and the five surveys were carried out by three different agencies (Jackson and Crockett, 1964:11; Blau and Duncan, 1967: 98-103). In particular, there are peculiarities in the marginal distributions of father's and of son's occupations in the 1947 NORC data. These were recognized by Jackson and Crockett and by Blau and Duncan, but still leave their conclusions in doubt (Koffel, 1974). Unfortunately, the four mobility tables were rendered comparable only in respect to a division among manual, nonmanual and farm occupations.

Our analyses of the three by three tables from five national surveys are summarized in Table 2. We shall not recapitulate the logic of our analysis, which follows that of Table 1. In panel A we report the likelihood-ratio χ^2 for the variation across surveys of father's occupation and of son's current occupation. Each of these is large, relative to its degrees of freedom, but the variation in the occupation distributions across surveys may reflect methodological factors as well as change over time. (We caution the reader against comparison of χ^2_{LR} and our other measures across tables, because of their differing levels of aggregation and numbers of observations.) In the baseline model of line B1, the margins are free to vary across time, but there is no association between father's occupation and son's occupation. The baseline model yields a large and statistically significant χ^2_{LR}, and it misclassifies 17.74 percent of the distribution. As shown in line B2, when we introduce a set of time-constant parameters for the interactions between father's and son's occupations, we account for 99.4 percent of the association in

Table 2. Analyses of Temporal Homogeneity in Mobility from Father's Occupation to Son's Current Occupation: U. S. Men in 1947 (NORC), 1952 (SRC), 1957 (SRC), 1962 (OCG), and 1972 (NORC) by Three Major Occupation Groups

Null Hypothesis	χ^2_{LR}	df	p	Δ	χ^2_H/χ^2_T
A. Marginal trends					
1. Constant distribution of father's occupation	118.40	8	.000	--	--
2. Constant distribution of son's occupation	140.05	8	.000	--	--
B. Full matrix (3 x 3)					
1. Margins vary with time	3268.18	20	.000	17.74	100.0%
2. Margins vary with time, constant father-son interactions	19.52	16	.242	1.02	0.6
3. No father-son interaction	3248.66	4	.000	16.72	99.4
C. Main diagonal blocked					
1. Margins vary with time	30.87	5	.000	0.61	0.9
2. Margins vary with time, constant father-son interactions	1.00	4	>0.5	0.06	0.0
3. No father-son interaction	29.87	1	.000	0.55	0.9
D. Selected tests for trend					
1. Constant occupational inheritance (C2 vs. B2)	18.52	12	.100	0.96	0.6
2. Constant mobility	2.95	4	>0.5	0.16	0.1
3. Constant upward and downward mobility	11.10	8	>0.1	0.64	0.3
4. Constant occupation-specific inheritance	15.57	8	.050	0.80	0.5

Note: N's are NORC-1947, 1153; SRC-1952, 747; SRC-1957, 1023; OCG-1962, 17,615; NORC-1972, 483.

the baseline model. The observed results might easily have occurred by chance if the model of time-constant interactions were correct, and the model misclassifies only one percent of the cases. As shown in line B3, the time-constant interactions are large and statistically significant.

In panel C of Table 2 we report analyses in which the three main diagonal cells of each mobility table have been blocked to permit occupational inheritance to vary freely over time. (Recall that occupational inheritance is defined by our occupation categories, which are very broad.) When the margins are fixed and the main diagonals are blocked, there remains a very small but statistically significant association in the tables (line C1). Slightly more than one percent of the distribution is misclassified, and less than one percent of the association in the baseline model remains. Thus, the five-survey data come very close to fitting a model of quasi-perfect mobility, when they are analyzed in a three by three mobility table. As shown in lines C2 and C3 of the table, virtually all of the remaining association may be explained by temporally constant interaction between father's occupation and son's occupation off the main diagonal. When that association is entered into the model, a virtually perfect fit of the data is obtained.

Line D1 of Table 2 contrasts the models of lines C2 and B2, which differ only with respect to the possibility of changes in occupational inheritance over time. The χ^2_{LR} for this contrast is not significant at even the 0.05 level, so we fail to reject the hypothesis of no change in occupational inheritance. Changes over time in father-son interactions on the main diagonal of the five-survey tables account for less than one percent of the association in the baseline model and reclassify less than one percent of the distribution. We next consider the possibility that there is a changing propensity to move versus stay, but there is no support for this in the data (line D2). There is no support for the hypothesis that propensities toward upward mobility, downward mobility and stability have changed across surveys (line D3), nor is there any support for the hypothesis that propensities toward upward relative to downward mobility have changed across surveys (compare lines D2 and D3). Finally, a test for temporal changes in occupation-specific inheritance reaches a nominal 0.05 level of significance, but such changes account for a negligible share of the association in the five-survey mobility tables. Thus, our analysis of the 25-year time series of mobility tables for U.S. men supports the analyses of other researchers insofar as they have emphasized a lack of change in the association between father's occupation and son's occupation.

Indianapolis: 1910 and 1940

The original and classic study of mobility trends in the United States was that of Natalie Rogoff (1953a; 1953b), who analyzed occupation reports of men about themselves and their fathers obtained from marriage license applications in Indianapolis in years centered around 1910 and 1940. About 10,000 reports were obtained in each period. Rogoff's analyses of these data were based primarily on comparisons of the fatally flawed "social distance mobility ratio" (Duncan, 1966; Blau and Duncan, 1967; Tyree, 1973). Depending on where one looks in Rogoff's writings, her conclusions are discrepant with regard to changes in the dependence of son's on father's occupation. In her monograph, Rogoff (1953a:106) writes: "... the processes by which men selected and were selected for occupations were more closely related to social origins in 1940 than they had been in 1910." However, in a summary of her work (Rogoff, 1953b:451), she concludes, "... no great change has taken place in recent times in the extent to which men may move from the occupational origins represented by their fathers' positions. The channels to social mobility afforded by the contemporary occupational structure are about as easily traversed now as they were at the beginning of the century."

Otis Dudley Duncan's (1966) careful reanalysis of Rogoff's data was primarily methodological in intent. With the possible exception of the analyses most closely resembling our own, his results suggested that the 1910 and 1940 mobility tables were very similar with regard to the dependence of son's on father's occupations. For example, after running regression analyses of the 1910 and 1940 data from Rogoff's detailed tables using several alternative scales of occupational status, Duncan (1966:69-70) writes, "... the socioeconomic status of occupations held by white Indianapolis men marrying in 1940 was no

more closely related to the socioeconomic status of their occupational origins than had been the case for white men marrying in 1910."

Another of Duncan's reanalyses of Rogoff's data was a major stimulus to our thinking about change in mobility tables, though with somewhat different empirical results. After verifying that the 1910 and 1940 ouflow matrices produce different destination vectors when multiplied by the same origin vector, and that the two matrices have correspondingly different fixed point distributions, Duncan (1966:73) asks,

> Is it possible that differences in the 1910 and 1940 mobility patterns are due solely to shifts in the distribution of job opportunities open to young men? . . . Can we, in other words, contrive a comparison between the two mobility tables putting the change in occupation structure in the role of an exogenously determined factor, which then induces a change in mobility patterns? The starting point of the comparison is to test the null hypothesis that all changes in the mobility table are due to proportional adjustments occasioned by changes, 1910 to 1940, in the two marginal distributions—the distribution of sons by their father's occupations, and the distribution of sons by their own occupations.

In order to test this hypothesis, Duncan used a least-square procedure suggested by Deming (1943) to estimate the frequencies in the 1940 table by proportional adjustment of rows and columns of the 1910 table. This null hypothesis is exactly the same as that of our equation 3, namely that the father's and son's occupation distributions vary over time, and there is a temporally constant set of father-son interactions. However, Duncan's statistical analysis differs from our own, in that Duncan fitted the 1940 margins to the 1910 table, while we have obtained maximum-likelihood estimates of the frequencies using the data for both periods.

Figure 2 gives the titles of the ten major occupation groups into which the Indianapolis data were aggregated, listed in order of socioeconomic standing on the Duncan scale. Our analyses of the Indianapolis data (Rogoff, 1953a:44-5) are summarized in Table 3. Again, the logic of our analysis is the same as in Table 1, but the reader should not compare components of variation or indexes of dissimilarity across tables. Between 1910 and 1940, there were large shifts in the distributions of father's occupation and of son's occupation in Indianapolis (lines A1 and A2). Obviously, we reject the baseline model of changes in the occupational structure, but no association between father's occupation and son's occupation (line B1), which misclassifies 17.68 percent of the distribution. As shown on lines B2 and B3, when we also specify a temporally constant set of father-son interactions, we correctly classify an additional 15.04 percent of the distribution, leaving only 2.64 percent wrongly classified. The model of temporally constant interactions accounts for 97.5 percent of the association in the baseline model. As in the OCG data on mobility to first occupations, changes over time in the marginal distributions of the mobility table for Indianapolis are much larger relative to their degrees of freedom than are changes over time in the association between father's occupation and son's occupation.

Since the Indianapolis data are not from a probability sample, it is not clear how seriously we should take the probability levels associated with our goodness-of-fit tests. At conventional levels, we would reject the hypothesis of temporally constant interactions, for $p = 0.003$ in line B2. However, given the very large number of observations and the very small amount of association attributable to changes in the father-son interactions, our analyses of the Indianapolis data lead us to the same conclusion as our analyses of the OCG data on mobility to first job and the five national surveys. There has been little if any change in the association between father's occupation and son's occupation. We have also analyzed the five by four tables of mobility for men in Indianapolis with urban occupations in 1910, 1940 and 1967 with similar results (Tully et al., 1970;192). There are nominally significant differences in father-son interactions between years, but these account for only 1.7 percent of the association in the baseline model. We have given less attention to the 1967 Indianapolis data than to the 1910 and 1940 data because of the different methods used in the 1967 survey.

In panel C of Table 3, we report analyses in which the main diagonals of the 1910 and 1940 tables have been blocked, permitting occupational inheritance to vary over time. When the margins are fixed and the main diagonal is blocked, there is still substantial

Table 3. Analyses of Temporal Homogeneity in Mobility from Father's Occupation to Son's Current Occupation: Men Married in Indianapolis in 1910 and 1940 by Ten Major Occupation Groups (N = 20,146)

Null Hypothesis	X^2_{LR}	df	p	Δ	X^2_H/X^2_T
A. Marginal trends					
1. Constant distribution of father's occupation	686.06	9	.000	--	--
2. Constant distribution of son's occupation	848.55	9	.000	--	--
B. Full matrix (10 x 10)					
1. Margins vary with time	4794.62	162	.000	17.68	100.0
2. Margins vary with time, constant father-son interactions	121.73	81	.003	2.64	2.5
3. No father-son interaction	4672.89	81	.000	15.04	97.5
C. Main diagonal blocked					
1. Margins vary with time	1201.19	142	.000	7.56	25.1
2. Margins vary with time, constant father-son interactions	96.71	71	.046	1.93	2.0
3. No father-son interaction	1104.48	71	.000	5.63	23.0
D. Selected tests for trend					
1. Constant occupational inheritance (C2 vs. B2)	25.02	10	.008	0.71	0.5
2. Constant mobility	4.98	1	.013	0.23	0.1
3. Constant upward and downward mobility	5.13	2	.081	0.26	0.1
4. Constant occupation-specific inheritance	20.03	9	.019	0.48	0.4

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association between father's and son's occupations (line C1), but three-quarters of the association in the baseline model for Indianapolis data may be attributed to occupational inheritance (compare lines B1 and C1). When we estimate frequencies based on temporally constant father-son interactions off the main diagonal (line C2), we account for all but two percent of the association in the baseline model, and we misclassify less than two percent of the distribution. The temporally homogeneous father-son interactions off the main diagonal are overwhelmingly significant (line C3).

When we contrast the models with and without changes over time in occupational inheritance (line D1), we find the temporal shifts to be extremely small, though they would be statistically significant at conventional probability levels. Changes over time in occupational inheritance account for only 0.5 percent of the association in the baseline model, and they reclassify only 0.71 percent of the distribution. The test for changes in the propensity to move between 1910 and 1940 is not even nominally significant at the 0.01 level (line D2), and it accounts for only a tenth of a percent of the variation in the baseline model. Even less substantial results are obtained when we test for possible changes both in upward and downward mobility (line D3), and there is virtually no change over time in the propensity toward upward relative to downward mobility (compare lines D2 and D3). Finally, there is little evidence of change in occupation-specific inheritance in Indianapolis (line D4). Considering the strict comparability of measurement in the Indianapolis data, the thirty-year separation between the two measurements and the very large samples in the two years, the stability of occupational mobility patterns is remarkable.

It is instructive to compare our results with those in Duncan's test of proportional change in the Indianapolis data (Duncan, 1966:74-7). Taking the 1910 matrix as the standard, Duncan's model misclassifies 7 percent of the distribution under the model of proportional change, while we misclassify 2.64 percent of the distribution under the same model. Duncan reports a decomposition of the sum of squares of ratios of 1940 to 1910 frequencies whose interpretation parallels that of the components of association in our baseline model. In Duncan's analysis, 76 percent of the sum of squares is attributable to proportional change in the row or column marginals. There are no corresponding figures in our decomposition since our baseline model incorporates change in the marginal distributions; however, we did note the substantial shifts in the occupational structure from 1910 to 1940. Duncan reports that 7 percent of the sum of squares is "due to proportional change in both distributions, reflecting the initial correlation between son's and father's occcupation," and 17 percent is "due to nonproportional change, or interaction" (1966:74). In our analysis, 97.5 percent of the association in the baseline model is due to temporally constant patterns of association between father's and son's occupations. In our view, this gives a stronger basis to Duncan's conclusion (1966:96) that

> some considerable modifications of the mobility *pattern* (emphasis in the original) . . . occurred in consequence of the change in 'structure' represented by alterations of the frequency distributions of origin and destination classes.

Other Data on Mobility Trends

The analyses reported above do not exhaust the possibilities for measuring trend in occupational mobility among U.S. men. For example, the Johns Hopkins University sample of men aged 30 to 39 in 1968 might be compared with the cohort of that age in the 1962 OCG survey (Coleman et al., 1972). Data from the Six-City Survey of Labor Mobility might be used to contrast mobility patterns in 1940 and 1950, and it may be possible to construct a time-series of data from the Detroit Area Study (Duncan, 1968:697-703). Comparisons might be made between certain male OCG respondents and those in the two male panels of Herbert Parnes' National Longitudinal Surveys (Parnes, Miljus and Spitz, 1970; Parnes, Fleisher, Miljus and Spitz, 1970). These examples probably do not exhaust the resources presently available, and another important test of our hypothesis will become possible when the 1973 OCG data from the national survey arrive (Featherman and Hauser, 1975).

While the data listed above are available in published or machine-readable form, we have not exploited any of them in the present analysis. Rather, we made two other comparisons, neither of which pertains strictly to

temporal change, but both of which bear on the extension of our finding of invariant mobility patterns. First, we compared mobility of (OCG) men in the U.S. in 1962 to the mobility of Wisconsin men in 1973, using data from a statewide survey commissioned by Featherman and Hauser (1975). Second, we compared the mobility of U.S. men in 1962 from father's occupation to son's current occupation, using the same nine five-year cohorts as in our earlier analysis of mobility to first full-time occupations. While both of these comparisons are sound with regard to survey method, the first confounds time and place, and the second confounds time (cohort) with chronological age. We carried out these two comparisons using the same methods as in the analysis of Table 1, with the same results. That is, once controlling for change in the occupational structure, we found no change in occupational mobility.

Discussion

The foregoing analyses apply an appropriate solution to the standard problem in comparative mobility analysis—how to separate the effects of changes of occupational structure, given by shifting marginal distributions, from those of the process of mobility, given by associations between occupational origins and destinations, for the purpose of measuring change in the latter. Analytically, the solution is to adopt the multiplicative or log-linear specification of the frequencies, which posits separate parameters for the marginal distributions and changes therein and for the underlying associations and changes therein. In several large bodies of data on U.S. men, we have observed that the pattern of association between father's occupation and son's occupation is largely invariant with respect to time.

It is always a logically difficult matter to maintain the null hypothesis, but we think we have offered sufficient evidence of temporal invariance to challenge the ingenuity of other researchers who would offer a more plausible alternative hypothesis and supporting data. One simple model of change we have not treated explicitly is linear trend; but in scanning reams of computer output, we have seen little evidence of order in deviations from the model of temporal homogeneity. We did locate one trend across OCG cohorts to increase the ratio of actual to expected chances of mobility from father's occupation to son's first full-time occupation. This trend accounts for about half the variation in mobility propensity across nine OCG cohorts, but the total of that variation represents less than a third of one percent of the association in the baseline model (see line E3 of Table 1). Other models may capture systematic patterns of change in mobility with relatively few parameters, but the explanatory potential of such models is necessarily limited.

We have used our imagination in specifying models of change in mobility, but beyond noting that the OCG data do not conform to the hypothesis of quasi-perfect mobility, the present analysis says little about the pattern of occupational mobility at any one time. That is an important, but distinct, problem. For alternative models of the mobility table, see Goodman (1972b) and Haberman (1974a).

Among serious students of mobility in the United States there has been agreement of late that trends, if any, have been slight. Thus, one might ask what motivates so long and tedious an effort to sustain the hypothesis of no trend. We think the present essay is amply justified by the variety of hypotheses about trend which we have been able to test. However, we believe it equally important that we have eliminated trends in the occupational structure from our measurements of trend in mobility.

Sociologists have long recognized that the occupational structure and changes in it affect mobility patterns. This recognition has generally taken the form of injunctions to control variation in the occupational structure before venturing comparisons between mobility tables and of efforts to construct mobility indexes which would make this possible. However, once trends in the occupational structure are controlled, there are no trends in the occupational mobility of U.S. men. This suggests an inversion of the traditional problem of comparative mobility analysis.

Rather than treating shifts in the occupational structure as a nuisance factor, to be set aside before undertaking comparative mobility analysis, we suggest that shifts in the occupational structure may be both the driving force and the problematic issue in comparative mobility studies. We ought to be asking what changes in observed mobility chances may be induced by transformations of the occupational structure, such as occur in the processes of urbanization and industriali-

zation. That is, even when the relative chances of men (as defined by our model) do not change, it is possible for other interesting properties of the mobility tables to vary systematically with changes in the marginal distributions (Duncan, 1966:76-7). Moreover, if changing occupational mixes affect mobility patterns, students of mobility will want to take more than a casual interest in the sources of occupational transformations. We believe our analysis adds force to Wilbert E. Moore's (1966:196) observation:

> the analytical separation of mobility accounted for by changes in the distribution of occupation within the socioeconomic structure from that accounted for by change in the distribution of opportunity or accessibility is a perfectly legitimate procedure. But there is no reason to say that only the second datum is interesting.

In the sequel to this paper (Hauser et al., 1975), we shall attack the first of the problems just posed. Given a fixed pattern of mobility from father's occupation to son's current occupation, we shall ask what the implications are of changes in the marginal distributions of father's and of son's occupations. In discussing his analysis of proportional change in the Indianapolis data, Duncan wrote (1966:75), "Had only the 1910 table been available, plus the 1940 marginals, the assumption of proportional changes would have been a reasonable basis for estimating the 1940 table." Applying a well-known technique of proportional adjustment (Mosteller, 1968), we have taken tables of U.S. men's mobility from father's occupation to current occupation for certain cohorts in 1962, and we have projected them backward and forward in time to fit earlier and later occupation distributions, while preserving the underlying patterns of association in the mobility tables. Several properties of the resulting hypothetical mobility tables vary systematically across years both within and between cohorts. Changes in the occupational structure and the succession of cohorts have led to increased rates of mobility and of upward mobility, even though the underlying process of mobility has not changed over time.

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TWO MARKOV MODELS OF OPEN SYSTEM OCCUPATIONAL MOBILITY: UNDERLYING CONCEPTUALIZATIONS AND EMPIRICAL TESTS*

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The principal purposes of this paper are to introduce a more general area of application for a refreshing approach to occupational mobility (open systems), to illustrate the general open system approach with two currently existing alternative models including an explication of the two models' distinct representations of the process and to test each model's predictive accuracy for a given job system. The approach focuses on intragenerational occupational mobility in a continuously operative job system. One model views mobility in terms of manflows, the other as interrelated job vacancy moves. Application is to an internal labor market–a state police system. The data are continuous for 43 years (1927-1970). Both models are found to be quite accurate in predictions once stationarity was approximated: 1949-1970. This study along with other open system studies suggests that Markov chain models are more generally adequate than has been concluded from closed cohort analyses. The models have general applicability to job systems, whether or not formally organized.

1. INTRODUCTION

This paper has three principal purposes: (1) to introduce a more general area of application for a refreshing conceptualization of occupational mobility (open systems); (2) to illustrate the general open system approach with two currently existing alternative models, including an explication of the two models' distinct representations of the process and (3) to test each model's predictive accuracy for a given organizational job system. The approach is introduced in Section 2. This general conceptualization includes two facets of the problem not generally addressed by sociologists–the structuring of occupational opportunities by internal labor markets and the operation of occupational structures as open systems. Moreover, even though specific examples of the approach are utilized in the study and a formally organized job system is tested, the general open system approach extends to a much wider area of both modeling and application of occupational mobility. In Section 3 the representations provided by two specific models are presented. Each is from a Markov chain model. In Section 4 the mathematics of the two models and their substantive interpretation for a specific job system (state police) are presented. Each model is then tested in Section 5. The tests are replications. Yet, in neither model's case have extensive replications occurred. In the vacancy chain model, two types of occupational systems have been examined: clergy (White, 1970) and educational administration (Brown, 1975). In the manflow or demographic model somewhat more testing has occurred, mostly in England: Young (1965; 1971); Young and Almond (1961); Forbes (1971); Sales (1971). In the United States, such application is lacking. Both tests are therefore seen as important. Finally in Section 6, implications of the conceptualizations, models and tests are discussed.

2. GENERAL CONCEPTUALIZATION OF OPEN SYSTEMS

Highly industrialized societies are characterized by highly differentiated occupational structures. There are many occupations in which a new entrant to the labor force may begin his career, and many moves possible between occupations for an individual already in the labor force. Yet, there are restrictions on the set of possible moves, restrictions which depend to a large extent upon the occupational grouping to which an individual belongs. For instance, lower level blue-collar

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and white-collar jobs provide rather limited opportunities for upward mobility. On the other hand, a rather extended set of opportunities for mobility may exist in an occupational grouping such as fire departments, police forces and other civil service organizations in which there exists a hierarchical promotion system.

In general, it would appear that there are several distinct groups of occupations or internal labor markets, each having its own restrictions for movement (Caplow, 1954; Doeringer and Piore, 1971). That this is true and has been largely unexplored by sociologists is somewhat ironic. The aspect omitted from the conceptualization has been the organizational nature of such mobility. More specifically, sociologists have overlooked the operative scope conditions or limiting ranges within which the set of possible moves are limited and the internal administration of mobility within such limited boundaries.[1]

A second aspect of intragenerational occupational mobility not generally addressed by sociologists is the dynamics of the occupational structure as an open system. Although net redistributions of occupations over time have been recorded throughout this century, the analyses of such dynamics have been largely descriptive. For example, in terms of explanatory power, no one has been able to show the interrelationships between mobility and occupational redistributions over time (cf. Blau and Duncan, 1967, Chapter 3). Obviously, to conceptualize a dynamic system with changing size is more manageable if we take an internal labor market rather than an entire societal job set as the unit of analysis. For example, the internal labor market may be defined occupationally, such as professions or crafts, or organizationally, such as civil service systems, manufacturing firms or universities. In this case, the open system is that of a job system operating continuously in time (ongoing, real time) with inflows and outflows. We distinguish open systems from closed systems as follows:

(1) a system is a bounded collection of jobs;

(2) an open system allows people to enter and/or leave;

(3) a closed system allows neither.

Since most sociological analyses have focused on closed systems, it is important to briefly examine some limitations of such studies. When people do cohort analysis, they construct an artificial closed system. For example, a sample is taken from a given year. Individual career histories are then examined for movement in prior (or following) years in which the person has been in the labor force. From such data one may analyze an individual's movements and differentiate between individuals in terms of age, race, etc., with respect to proportional number of moves, time intervals between moves, patterns of movement and the like. However, the cohort data are not appropriate for testing occupational system or labor market dynamics for years prior to (following) the sample year since samples of prior (past) populations are not obtained.[2] In brief, while the artificial closed system may provide a valid representation of subaggregate processes for a given heterogenous population, it nevertheless provides a distorted empirical image of the occupational system dynamics in real time prior to (following) the sample year. The individual's movement is taken out of its organizational (open system) context, and important forces operative upon the individual or aggregate sets of individuals are ignored by not conceptualizing system processes as they simultaneously occur in real time. Left unaddressed are questions such as the following: What are the boundaries within which the movement between occupational sets is limited? What are the dynamics of each bounded system and how do they vary across systems? What rewards and opportunity costs are operative within each system?

The open system approach being suggested here utilizes both aspects of the problem generally overlooked—the structuring of occupational opportunities via job systems and the operation of occupational structures as open systems. In fact, to conceptualize an open system requires that we define the system's

[1] Moreover, if it is true that there are distinct groupings of occupations within which mobility generally occurs, then what we normally think of as a national occupational system (with respect to occupational mobility) is actually a national set of systems.

[2] For a somewhat analogous phenomenon see Duncan (1966).

boundaries, thus focusing our attention on job systems. Alternatively, once we consider a job system rather than a population of persons, its dynamics in real time are of interest, thus focusing our attention on flows across boundaries and the system's self-renewal. The two aspects are thus complementary and easily incorporated into a single approach.

It should be pointed out that formal organizations do not exhaust, but rather form a subset of open systems. Thus, the open system approach extends both to formally organized occupational dynamics and to general occupational groupings structured for mobility purposes, but not operative as formal units. The application to formal organizations alone suggests a wide area of application. The extension to more general internal labor markets (e.g., professionals) makes the applicability sufficiently general to include a major part of a post-industrial society's labor. (See Doeringer and Piore (1971) for a discussion of internal labor markets and the extent of labor working in such systems.) Thus, while the approach being suggested has not been around long, its utility appears quite general.

3. TWO OPEN SYSTEM MODELS – REPRESENTATION

The discussion in this section centers around the representation of mobility processes by two open system models. The mathematical models follow in Section 4. The first model's representation is called demographic since it conceptualizes mobility in terms of manpower flows (cf. Young and Almond, 1961; Bartholomew, 1973). The second model's representation is called a vacancy chain. It conceptualizes mobility in terms of interrelated movement of job vacancies (cf. White, 1970). The models are complementary to conceptualizations emphasizing individual motivation and choice. Their explanatory power derives largely from a specification of structural boundaries and internal dynamics which limit individual choices, whether knowingly or not, to the participants.

3.1 Underlying Conceptualizations

We first examine an occupational structure defined by manpower flows. Sociologists typically examine intragenerational occupational mobility by analyzing net flows of persons from their first jobs to their current jobs at the time of the study. Imagine a structure having three strata, as in Figure 1. In this conceptualization, structure is defined processually in terms of manpower flows between and within strata. Observe that one person's move is not related to another's movement in this formulation. In other words, individual persons moving between jobs are represented, but individual jobs are not. For the moment, let us forget whether the person's movement refers to first job, second job, etc. If we now wish to represent distinct or individual jobs as well as persons, then we may view how the persons' movements are interlinked into sequential chains of movement. For example, if one person's move opens a vacancy for another person to enter, then we may represent such interrelated movement by Figure 2. From Figure 2 we may observe the length (total number) of interrelated movements as well as the direction (whether horizontal or vertical) of these movements at each stage. Both length and direction, when thought of together, represent a chain of movement. Within these chains of movement each particular individual's occupational movement occurs.

By again referring to Figure 2, we may further ask how these chains of movement begin and end. Two possibilities occur in each case. Chains of movement begin by a person's leaving the system or by the creation of new jobs within the system. A person may leave a particular system's job market by various modes: retiring, dying, being laid off and quitting. Chains of movement also end by two modes: either a person enters from outside the system (a new recruit) or the position is abolished.[3] Two additional dimensions have now been added. First, we have denoted the alternative means of initiating and terminating chain movement. Secondly, the existence of a system boundary was implied. That is, the modes of entry and exit imply that the remaining movement occurs within a certain boundary which defines the system or the labor market being analyzed. We now add a final dimension to this process. Note that

[3] An alternative way in which vacancy chains originate and end is by means of job exchanges within the system, i.e., you take my job and I take yours.

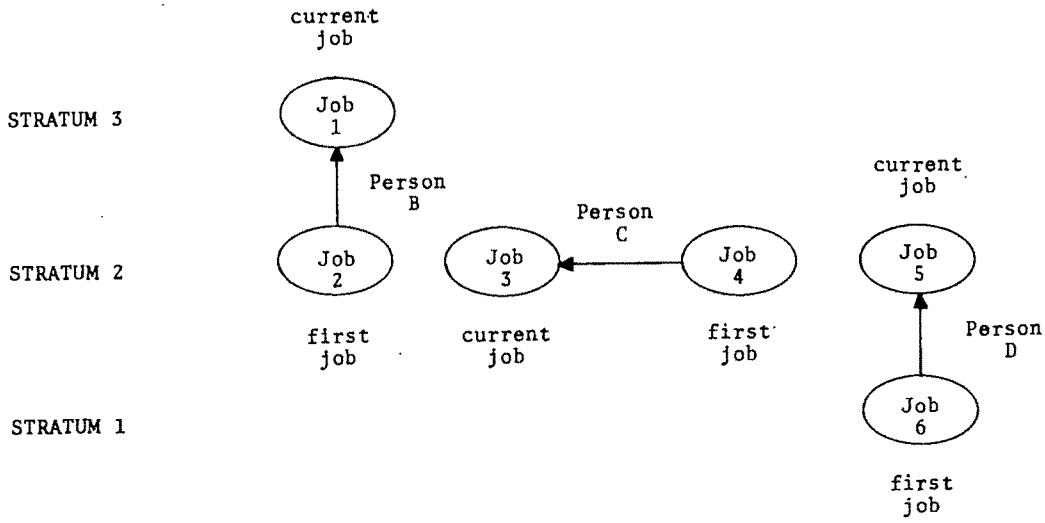

Figure 1. Occupational Structure Defined by Manpower Flows

movement may refer to movement of job vacancies as easily as to movement of men. If we wish to focus upon jobs, as is the case with the vacancy chain model, the appropriate population is that of jobs not men. Movement, therefore, refers to job vacancies. For instance, in Figure 2 the job vacancy moves from Job 1 to Job 2, then to Jobs 3 and 4. By including the modes of beginning and ending chains as described above, we would have the job vacancy moving from outside to Job 1, then to Jobs 2, 3, 4 and finally outside once more, as in Figure 3. Note that with the exception of two cases, the creation and abolition of jobs, job vacancy movements is simultaneous with man movement.

The basic elements of the conceptualization underlying the vacancy chain model are taken into account in Figure 3. By conceptualizing the movement in terms of job vacancies, we also conceptualize the mechanism generating movement. A man moves because a vacancy exists. The job vacancy operates in terms of a pull. Once we view the entire set of vacancy chains within a system, we are viewing the relations between opportunities or the opportunity structure.

In the demographic model's conceptualization, as in the vacancy chain conceptualization, the system has distinct boundaries and the boundary limits are set by the job market. However, flows now refer not to job vacancies

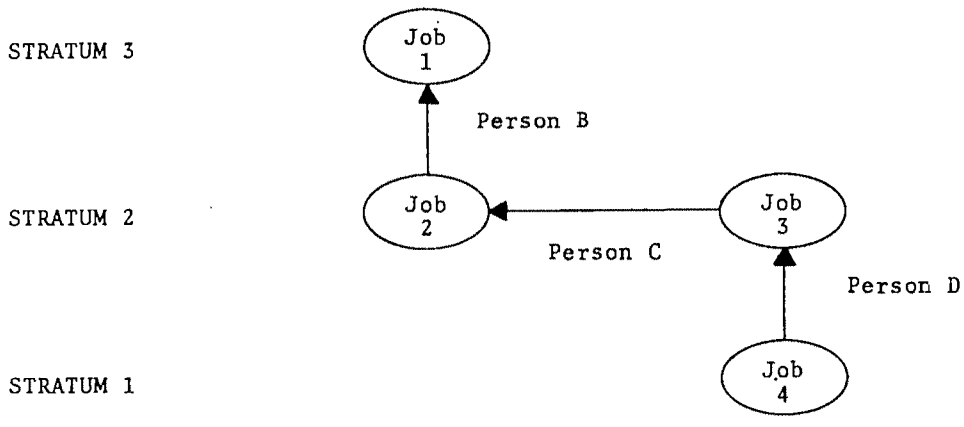

Figure 2. Occupational Structure Defined by Interrelated Manpower Flows

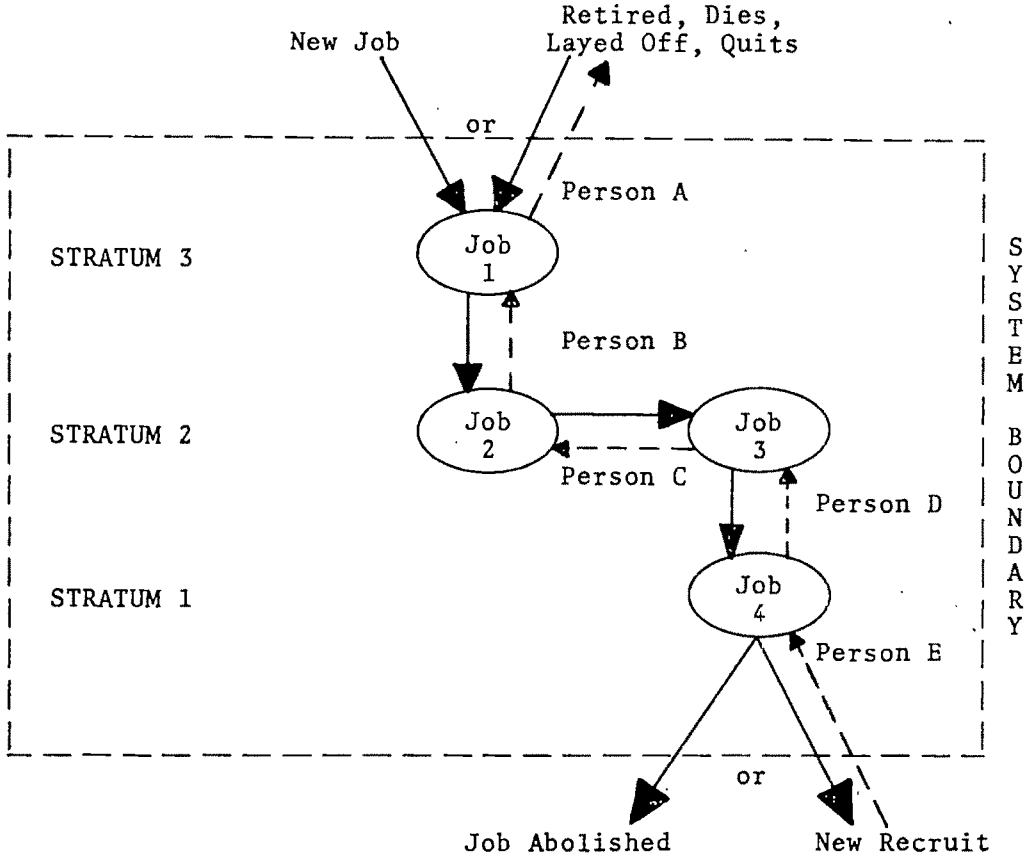

Figure 3. Bounded Occupational Structure as Conceptualized within the Vacancy Chain Model

but to populations of men. In addition, there is neither a conceptualization of sequences of moves nor a mechanism prompting movement. Rather mobility is described in terms of "isolated" manpower flows between strata, as in Figure 4. The result of this mobility is the net redistribution of men among the strata. One major aspect of interest is the individual's chances of movement given he began in a certain stratum.

3.2 Basic Distinctions Between the Two Models

Since each type of conceptualization of mobility has been described, we may now summarize the basic distinctions between them. An obvious difference between the two types of models lies in the object that is moving: in the vacancy chain model it is job vacancies; in the demographic model it is people. A second difference lies in the time span for which movement applies. Within the vacancy chain model, time is essentially irrelevant. Thus, with continuous data and stationary transition probabilities, the entire time span may be analyzed as a unit. On the other hand, since more than one move by the initial population of men is not taken into account by the demographic model, a time span such that only one move may occur is the maximum allowed in its application.

A third distinction relates to transition probabilities. The diagonal entries of the transition matrix of the two types of models refer to separate events. The transition matrix for a three strata system would be as follows:

$$\begin{bmatrix} p_{11} & p_{12} & p_{13} \\ p_{21} & p_{22} & p_{23} \\ p_{31} & p_{32} & p_{33} \end{bmatrix}$$

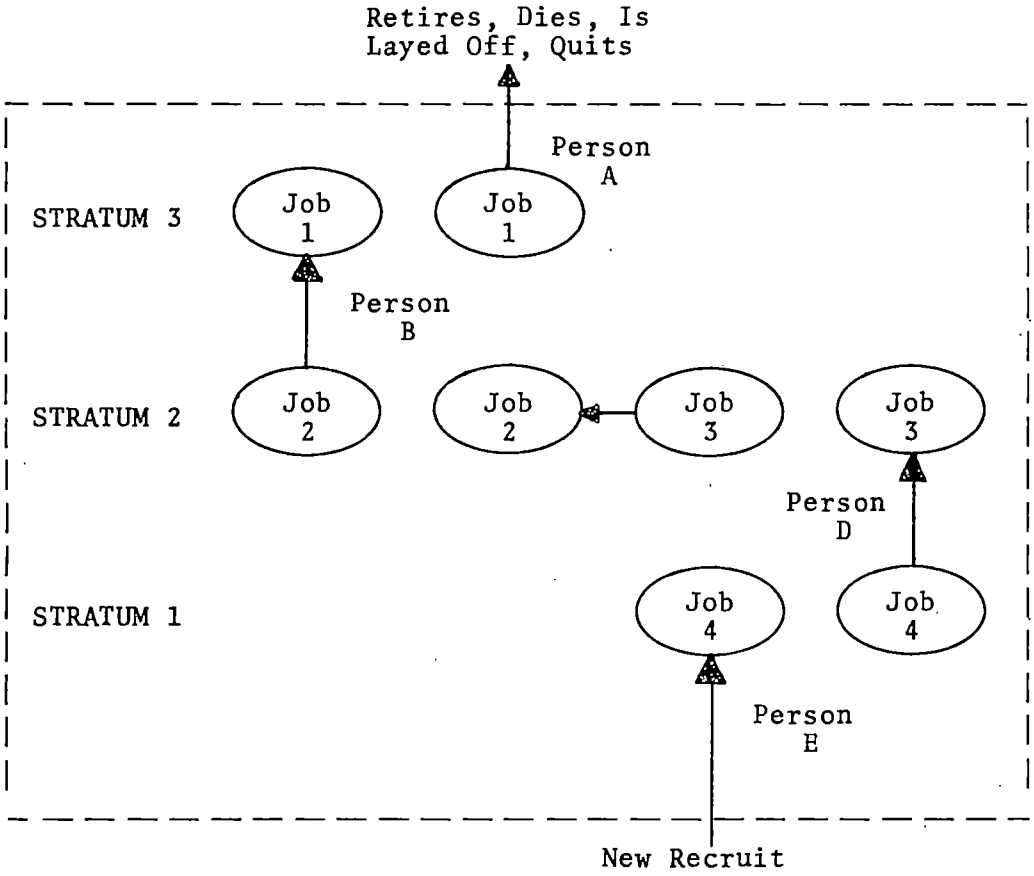

Figure 4. Bounded Occupational Structure as Conceptualized within a Demographic Model

In the demographic type of model the p_{ii} are the result of two things—either a person remaining in his job or a person changing jobs but remaining in the same stratum. No conceptual distinction is made for these two different events. On the other hand, in the vacancy chain model, the diagonal entries denote *only movement* within strata. Since a vacancy chain pertains only to movement, persons who remain in their jobs are not included in such a chain.

A fourth difference is that the mobility parameters for the vacancy chain model are limited to internal and outgoing flows. The third type of flow characterizing mobility (incoming) has been assumed as given. The demographic type of model conceptualizes all three types of flows.

A fifth difference is that the demographic model conceptualizes no mechanism for movement, while the vacancy chain model does conceptualize such a mechanism—a man does not move unless a job vacancy exists. This type of mobility, occurring for example in civil service systems, differs from that of an occupational grouping such as physicians or university professors where status change does not necessarily mean a change in function. Nor does movement within a university, as for example from assistant to associate professor, open a vacancy at the assistant level. Rather, the job moves with the man. In a system in which man and job vacancy movement are distinct (in opposite directions), the vacancy chain conceptualization seems particularly apt.

A sixth distinction between the two types of models may be seen in their conceptualization of structure. The demographic type of model conceptualizes structure as the flows of

manpower between occupations. The vacancy chain model, however, conceptualizes a second degree structure—the relations between the flows themselves or the flow structure (interrelated flows) between occupations.

The final distinction which will be drawn between the two types of models lies in the nature of their predictions. The demographic model predicts strata size over time. Thus, the model's explanation of mobility is by means of a derived outcome of movement—the distribution of a stratified manpower system over time, i.e., system evolution. On the other hand, in the vacancy chain model the generation of job vacancies is taken as given and predictions refer to specific aspects of the internal mobility processes—the length of vacancy chains per stratum of origin and the total number of moves from each stratum. It is the nature of individual movement which the vacancy chain model explains by means of its derivation of interrelated movement.

3.3 Use of the Two Models in the Study

Prior to the development and analysis of the models, we address how the two types of models will be utilized. Observe that each model's strength seems to be the other's main weakness. The demographic model's main contribution is its accounting for relative strata size over time. Its primary weaknesses are its inadequacy to conceptualize a mechanism prompting movement (a major fault of many demographic conceptualizations) and its more simple conceptualization of structure, resulting in a much less comprehensive description of the processes of system mobility. Conversely, the vacancy chain model's major strengths are the two primary weaknesses of the demographic model (mechanism and more comprehensive structural description). One of the principal weaknesses of the vacancy chain conceptualization lies in the fact that its description of mobility processes begins once a job vacancy has entered the system. Therefore, unless the inflow process is constant or models developed such that inflows are derived,[4] the vacancy model's utility for projection purposes is very limited.

[4] Two models would be necessary—a stratified manflow exit model and a stratified job vacancy creation model.

The complementary strengths and weaknesses of the models and the lack of extensive testing of either model suggested that a crucial test to choose one model over the other was premature. Moreover, the data which will be used to test the models cover a continuous time period. Hence, we may be more thorough than previous studies in analyzing the interrelations between other major dynamics or perturbations and mobility. We will examine each model's predictive power as well as the relationship of other system processes and externalities to the mobility processes conceptualized by each model.

4. TWO OPEN SYSTEM MODELS—MATHEMATICS

Pertinent system characteristics and data are first described. The mathematical models are then presented and substantively interpreted for a particular job system—the state police. Included are classification of strata and examination of basic Markov chain assumptions.

4.1 System Characteristics and Data

The system of occupations analyzed was a state civil service department—the Michigan Department of State Police. Jobs entered by persons leaving the State Police organization were not considered. Therefore, the system of jobs or labor market under consideration was formally bounded.

Clearly, in the State Police not all jobs are interrelated with respect to mobility. At least two labor markets may be easily specified: police and civilian. The civilian jobs pertain to business administration, clerical work, data processing and maintenance. Moveover, these jobs are supportive of the more basic services performed by the organization—direct state police work. In this study, therefore, we will focus only on the police job system. The organizational characteristics which are most pertinent to the occupational mobility of this labor market are the following:

1. It is self-contained in terms of mobility (i.e., no exchange of personnel between the state police and other civil service departments).
2. It has a highly formalized authority structure and a clearly defined stratification system.
3. The authority-status structure is diagrammed as follows:

Director
(Colonel)
|
Assistant Directors
(Lieutenant Colonel, Majors)
|
District and Division Commanders
(Captains)
|
Lieutenants, Detective Lieutenants
|
Staff Sergeants, Detective Staff Sergeants
|
Sergeants, Detective Sergeants
|
Corporals, Detectives
|
Troopers

4. As may be noted from the above authority-status structure, the job system has three principal occupations—general policeman, specialized criminal investigators and administrators.

5. Even though the State Police is a government department, it is highly autonomous relative to internal government; therefore, there exists a high degree of endogenous determination of the mobility processes.

6. Mobility is almost entirely over a distance of one stratum only (i.e., if movement is upward, a man moves only to the stratum immediately above the one he presently occupies).

7. All men enter the system at the bottom stratum (i.e., there is no lateral entry) and size of entering cohorts vary greatly, depending upon demand, loss rate and state economics. (Between 1927 and 1971 the range was 6 to 150.)

8. In age, the system is quite young; it was created in 1917 and at the end of the period of this study was but 53 years old.

9. The system has generally been expanding from its inception to the present.

The data were collected between 1970 and 1972. They are of two basic kinds: job vacancy chain movement and manpower size per stratum per year. Since man movement was a sub-class of job vacancy chain movement, it was not necessary to separately collect movement data for each model. The recording of individual job vacancy moves was generally made by comparing two personnel rosters for each job. The positions were usually recognizable since each person was simultaneously listed by specific structural segment of the organization, by rank and by geographical location. In cases where personnel rosters did not provide sufficient information for determining specific job vacancies, additional search procedures were conducted. In such instances, job history files, an abolished job file, termination or transfer records, general organizational records, microfilm records and personnel files were examined as necessary. The above searches apply to each job vacancy move. During the collection process such individual moves were interrelated into a job vacancy chain as exits and entrances to the same job were recorded. From vacancy chain movement, all parameters for the vacancy chain model may be calculated. From organizational strata size and man movement recorded within vacancy chains, the calculations for the demographic model may be made.

The data cover a 43-year period (1927-1970) and were collected for each year. They include all moves related to any vertical occupational movement for the enlisted (police) segment of the State Police system for each year. As a result, neither selectivity of years nor sampling of the total movement per year may be said to account for part of the variation between predicted and observed values.

4.2 Vacancy Chain Model

This model is formulated as a discrete state, imbedded Markov chain model with absorbing states (cf. White, 1970). Recall that the population conceptualized by the vacancy chain model is one of jobs, not men.

4.2.1 Presentation of the Model

Divide the population of jobs into k strata. We will assume that the distribution of job vacancies entering a particular stratum i from the outside (q_{0i}) is given. Hence, the probabilities for job vacancy movement will refer only to movement within or between strata (q_{ij}) and movement to the outside (q_{i0}). Also assume a vacancy must leave the job which it occupies within a maximum span of time. This assumption is reasonable since a permanent job vacancy has no real meaning. In fact, the duration of a job vacancy per job is usually quite short. Since the inflow process is

either observed or derived, the stochastic model's range of possible moves is exhausted by describing the flows within and out of the system. Thus, for each origin stratum the transition probabilities describing the flows within and out of the system must add to one.

$$\sum_{j=0}^{k} q_{ij} = 1. \qquad (1)$$

If we denote $Q = [q_{ij}]$ $i, j = 1, 2, \ldots, k$, i.e., the transition probabilities between strata where q_{ij} lies in the i-th row and j-th column; $Q_0 = [q_{i0}]$; and L represents a column vector of k components, each with a magnitude of 1; from Equation (1) it follows that

$$QL + Q_0 = L. \qquad (2)$$

A job vacancy chain is simply the path of a job vacancy once it enters the system. Its length is the number of moves after its entrance, including the terminal move to the outside. Designate the length of the chain by n. Since the vacancy chain model conceptualizes both horizontal and vertical moves (i.e., all movements), n equals the number of positions a vacancy has occupied. Let us further designate the probability that a chain began in stratum i will end in n moves by p_{in}. We may arrange the probability for a chain of length n into a column vector called P_n, where the i-th row element denotes that the chain of length n began in stratum i. In the matrix Q^{n-1}, the i-th row and j-th column is the probability (that is, the sum of the path probabilities each of which is the product of probabilities) that after n-1 moves a job vacancy begun in stratum i is currently in stratum j. Multiplying Q^{n-1} by Q_0 gives the probability distribution that chains begun in each stratum will end or leave the system in n moves. It is

$$P_n = Q^{n-1} Q_0. \qquad (3)$$

The predicted distribution of chain lengths (P_n's) serves as a test of the model's adequacy. Other predictions which can be made from this model are the mean length of a chain which began in stratum i, overall mean length of chains and the total number of moves from each stratum.

The parameters for this model have been estimated in the following manner:

$$\hat{q}_{ij} = \frac{\sum_t a_{ij}(t)}{\sum_t a_{i\cdot}(t)} \qquad (4)$$

and

$$\hat{q}_{i0} = \frac{\sum_t a_{i0}(t)}{\sum_t a_{i\cdot}(t)} \qquad (5)$$

Terms: $a_{ij}(t)$ The total number of observed vacancy moves from stratum i to stratum j in the time period t.

$a_{i0}(t)$ The total number of observed terminal moves from stratum i in time period t.

$a_{i\cdot}(t)$ The sum of all observed moves from stratum i, including the terminal moves in time period t.

$\hat{q}_{ij}$ The estimated value of q_{ij}.

$\hat{q}_{i0}$ The estimated value of q_{i0}.

The two primary aspects of mobility which the vacancy model predicts are chain length distribution and total number of moves from each stratum. In this study we will analyze only the more interesting and complex predictions, chain length distribution. As White (1970:33) notes, the same data of chains can yield both the observed length distribution and the transition probabilities estimated in Equation (4). Since the transition probabilities cannot be deduced from the observed length distribution, the empirical test of this prediction may be made using data from the same time period. The principal reason for this is that parameter estimates are based on individual job vacancy moves and observations are on the chains as entities (White, 1970:105ff.).

4.2.2 Classification of Strata

Within the State Police system, the set of occupations includes several distinct types. They are general policemen, specialized criminal investigators and administrators. Due to the system policy of no lateral entry, all recruits enter at the bottom stratum. Hence, the administrators and detectives are selected

from the police patrol occupation. Moves between detective and administrative occupations also occur. Consequently, there are movements interrelating all three occupations.

The State Police system, organized along a paramilitary hierarchy, has a clearly defined authority and status structure. Although authority and status are analytically separable structures, within the State Police system they have identical structures, i.e., 1-to-1. This authority-status structure and the strata selected for substantive predictions may be represented as in Figure 5.

The system's nine strata were lumped into five strata on the basis of numbers per stratum and substantive meaning of authority-status levels.

To show how movement of job vacancies in the State Police system generally occurs, the transition matrix to be used for subsequent predictions is provided in Table 1. In this system, virtually all moves are across only one stratum, i.e., one step in length. In terms of total movement, demotions are rare, meaning that job vacancies move downward, not upward or $q_{ij} \approx 0$ for $i > j$. Since movement is generally in only one direction and one step in length, $q_{ij} \approx 0$ for $j > i + 1$.

4.2.3 Examination of Basic Assumptions

In this section we consider the meaning and validity of the three assumptions underlying the simple Markov chain model: homo-

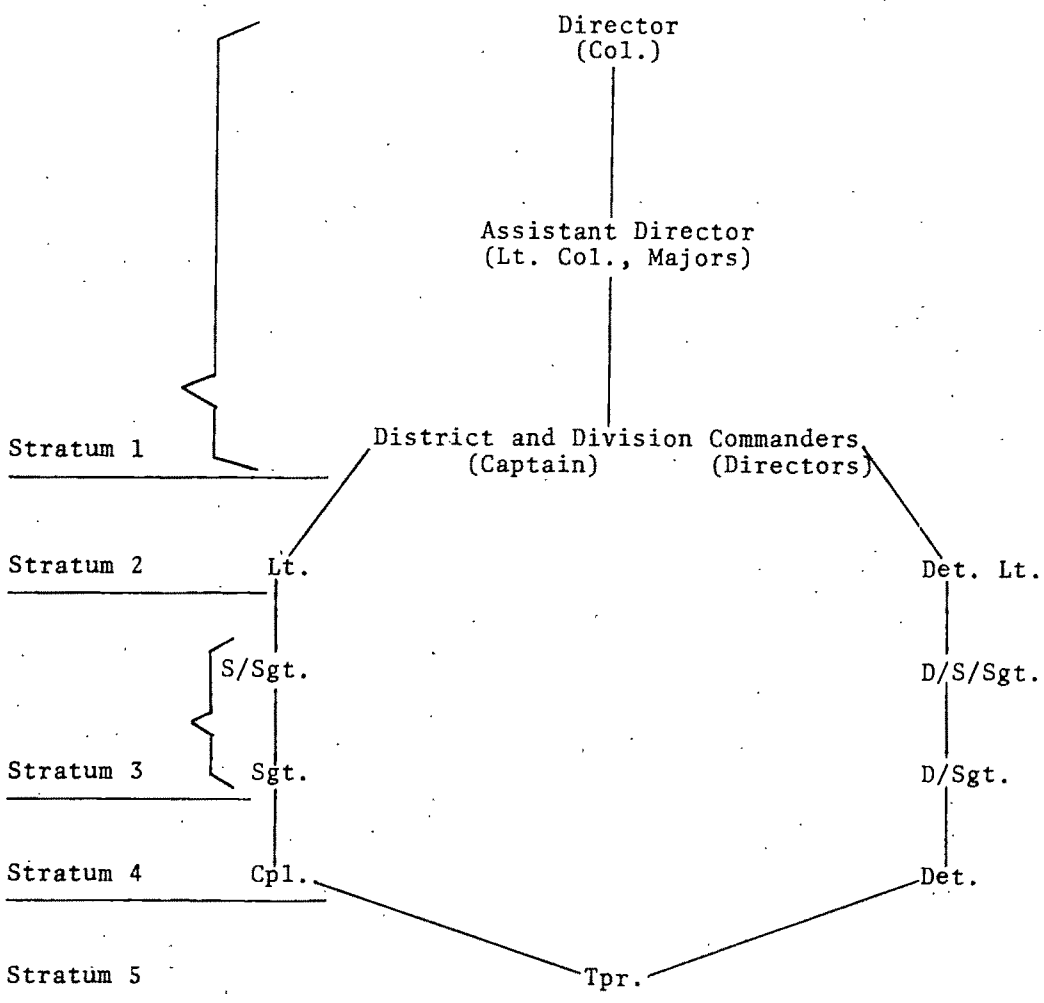

Figure 5. Combination of Authority-Status Levels to Define Strata

Table 1. Estimated Transition Probabilities of Job Vacancies for 1949-1969*

Origin Stratum	Destination Stratum: Col., Capt.	Lt.	S/Sgt., Sgt.	Cpl., Det.	Tpr.	Out	Total Number of Moves
Col., Capt.	.2115	.6635	0	0	0	.1250	104
Lt.	.0056	.1404	.7022	.0169	0	.1349	178
S/Sgt., Sgt.	0	0	.3443	.5539	.0015	.1003	668
Cpl., Det.	0	0	.0024	.2811	.6125	.1040	1231
Tpr.	0	0	.0004	.0031	.0495	.9470	2268

*Maximum likelihood estimations were made using Equations (4) and (5).

geneity, stationarity and the Markovian nature of dependence. The assumption of homogeneity of population presents much less of a problem in the job vacancy chain model than it potentially does in the manflow model. Job vacancies per job do not have long histories, and hence there does not exist a "cumulative inertia" effect of seniority per stratum. Job vacancies obviously occupy specific jobs. It is also true that there is some heterogeneity of jobs within strata. The important question is whether or not a job vacancy's occupancy of job A affects its probability for movement any differently than had the vacancy occupied job B within the same stratum, where jobs A and B are any two jobs. Stated somewhat differently, is job A likely to be filled by the same population of men as job B? In virtually all cases for our system this is true, with perhaps the exception of certain of the higher level communication and detective positions. Not only are these positions few in number but the transition probabilities are not necessarily different, given the possible exceptional nature of movement. With respect to the latter point, we are once again dealing with aggregate flows; and, therefore, it is the transition parameter which is of import rather than the distinct job entered. It seems reasonable, therefore, to view all job vacancies which exist within a stratum as homogeneous for movement purposes.

The second assumption, path independence, states that the job vacancy movement at time t is dependent only upon the job vacancy's location at time t-1 and not upon locations at times t-2, t-3, ... This Markov assumption, meaning that job vacancy history before the job presently occupied is not influential, seems easily acceptable.

The main assumption remaining to be examined is the stationarity or constancy of transition probabilities. Recall that for chain length predictions, estimations may be made from the same time period as the predictions. Stationarity in this case means that transition probabilities are stable throughout the entire time period. For yearly periods, stationarity means there are no changes in job vacancy flows within the year. For periods in which job vacancy chains originating in several years are combined, stationarity means that transition probabilities are constant both within yearly periods and across all "yearly" periods combined. Certainly the assumption of stationarity within yearly periods seems reasonable. Observation of yearly transition probabilities from 1927 through 1969 reveals that it is only from 1949 to 1969 that stationarity is approximated across years. The transition data for each of the five decades is given in Table 2. The effects of the Great Depression and World War II are evident in each stratum in the pre-1950 years. In general, after 1950 the dynamics appear somewhat stabilized.[5] This contrasts with White's findings (1970) of stability in clergy systems across five decades of the 1900s. Type and age of internal labor

[5] Examination of yearly transition matrices in the late 1940s also reveals the 1949 transitions are quite similar to those of the 1950 and 1960 decades. Thus, the general period of stationarity is 1949-1969.

Table 2. Transitions of Job Vacancies from Stratum by Decade

Origin Stratum and Decade	Destination Stratum: Col., Capt.	Lt.	S/Sgt. Sgt.	Cpl., Det.	Tpr.	Out	Total Number of Moves
Col., Capt.							
1960-1969	.2083	.6875				.1042	48
1950-1959	.2292	.6458				.1250	48
1940-1949	.2727	.3636	.1818			.1818	22
1930-1939	.1333	.4333				.4333	30
1927-1929	.3333	.5000				.1667	12
Lt.							
1960-1969		.1566	.6627	.0120		.1687	83
1950-1959		.1266	.7342	.0253		.1139	79
1940-1949	.0345	.0690	.7586			.1379	29
1930-1939	.1077	.0923	.2000	.0923		.5077	65
1927-1929		.2381	.4286			.3333	21
S/Sgt., Sgt.							
1960-1969			.4015	.5099		.0887	406
1950-1959			.2593	.6250	.0046	.1111	216
1940-1949	.0049		.3654	.3846	.0577	.1875	208
1930-1939		.0915	.3137	.3399	.0196	.2353	153
1927-1929			.2143	.2857	.1071	.3929	28
Cpl., Det.							
1960-1969			.0013	.2996	.6057	.0934	771
1950-1959				.2488	.6294	.1219	402
1940-1949			.0154	.2585	.5538	.1723	325
1930-1939		.0042	.0212	.2966	.2627	.4153	236
1927-1929			.0109	.3804	.2500	.3587	92
Tpr.							
1960-1969				.0039	.0621	.9340	1273
1950-1959			.0011	.0022	.0338	.9629	917
1940-1949			.0029	.0019	.1472	.8480	1026
1930-1939		.0018	.0072	.0450	.1838	.7622	555
1927-1929				.0055	.2320	.7624	181

market may be important. Certainly the nature of the strata are different. White's strata were defined in terms of church size. Neither the Depression nor the War caused major dislocations of clergy and churches. On the other hand, each of these perturbations had major effects on the personnel and administration of the state level police system. Nevertheless, that general stability in the police system's dynamics extended across 21 years (1949-1969) is quite significant.

4.3 Demographic Model

The demographic theory below pertains to a discrete state Markov chain model for an open system (cf. Young and Almond, 1961). Extensions in terms of the limiting structure of the process and a continuous time analogue of the model have been made by Bartholomew (1973).

4.3.1 Interpretation of Model

As in the case with the vacancy chain model, we will divide the population into k strata. The population in this model refers to men. Let $n_j(t)$ denote the number of people in stratum j between times t and t + 1 (t = 0, 1, 2, . . .). The initial stratum sizes $n_j(0)$ (j = 1, 2, . . ., k) are assumed to be given and we define the total system size at time t as

$$N(t) = \sum_{j=1}^{k} n_j(t). \qquad (6)$$

Since the initial (t = 0) stratum sizes are given, the initial total system size is also given [Equation (6)]. The primary task of the demographic model is to predict the size of each stratum for t > 0. That is, the model predicts the end result of movement by

populations of men. Hence, for $t > 0$ the stratum sizes are random variables to be accounted for by this stochastic model. Thus, the accuracy of the derived *expected* number of people in stratum j at time t will be a principal determinant of the model's utility. This expected number will be denoted by $\bar{n}_j(t)$.

Let the probability that an individual in grade i will move to grade j in one discrete time period be p_{ij}. For this model, probabilities are assumed to be time-homogeneous (stationary) and, therefore, a time notation argument is unnecessary. Since, in an open system, transitions to the outside are possible, in general

$$\sum_{j=1}^{k} p_{ij} < 1. \qquad (7)$$

In the State Police two system characteristics affect the transition matrix. Recall that virtually all moves are across only one stratum boundary and demotions are rare. Therefore, movement is generally a promotion from current grade or stratum to the one immediately above it or a lateral move within the stratum. Hence, generally $p_{ij} = 0$ for $j < i - 1$ and $i < j$. The probability of an individual's leaving the system from the i-th stratum, denoted $p_{i,k+1}$, is thus

$$p_{i,k+1} = 1 - \sum_{j=1}^{k} p_{ij}. \qquad (8)$$

The transition matrix for manflows, to be used in subsequent predictions, is provided in Table 3.

All new recruits enter the lowest stratum. Moreover, since virtually all men entering (including re-entrances) the system are new recruits or Troopers, we usually have $p_{oj} = 0$ for $j \neq 5$ where o designates outside. Hence, the vector of entrance will generally be the following one:

$$P_o = \begin{bmatrix} 0 \\ 0 \\ . \\ . \\ . \\ 1 \end{bmatrix}$$

To allow for all possibilities for the model, we may assume that proportion p_{oj} enter the j-th stratum, $j = 1 \ldots, k$. Hence,

$$\sum_{j=1}^{k} p_{oj} = 1. \qquad (9)$$

As illustrated above, the matrix notation for a recruitment distribution $[p_{oj}]$ will be P_o.

We have now accounted for all three types of manpower flows. The p_{oj}'s represent the inflows, the p_{ij}'s the internal or thru flows and the $p_{i,k+1}$'s the outflows. Initially in this section, a population unit was introduced which will either be multiplied by these manflow terms to derive stratum redistribution or will be the stratum size derived. The population unit was $\bar{n}_j(t)$ representing the

Table 3. Estimated Transition Probabilities of Manflows for 1950-1969*

	Destination Stratum						
Origin Stratum	Col., Capt.	Lt.	S/Sgt., Sgt.	Cpl., Det.	Tpr.	Out	Total Number of Personnel
Col., Capt.	.8501	0	0	0	0	.1499	347
Lt.	.1571	.7408	0	0	0	.1021	382
S/Sgt., Sgt.		.0557	.8616	.0005	0	.0822	1994
Cpl., Det.			.0844	.8856	.0022	.0278	4180
Tpr.				.0543	.9054	.0403	13524

*Maximum likelihood estimates were made using Equations (12), (13), and (14).

expected number of people in stratum j at time $t > 0$. For $t = 0$, the stratum size is observed and we have $n_j(0)$. One additional type of population unit needs introduction before formulating our basic equation—entrants or "recruits" to the system. The expected number of new recruits to the system in the time interval (t-1, t) is denoted by R(t). In all cases, R(t) will consist of either all new entrants (i.e., new Troopers) or a large proportion of actual new entrants. In other words, men returning to the system who are in a stratum above that of Trooper are very few in number. Moreover, since our system is generally either expanding or maintaining zero growth the number of recruits is generated from two processes—men leaving and system expansion. In order to maintain zero growth, the system must replace members who have left. Similarly, if the system is to expand, the system must not only replace leaving members but recruit additional entrants to fill new positions. In either case, the expected number of recruits required at time t is

$$\bar{R}(t) = M(t) + \sum_{i=1}^{k} p_{i,k+1}\bar{n}_i(t-1) \quad t = 1,2,\ldots \quad (10)$$

where M(t) denotes the increase or zero growth at (t-1, t).

All elements necessary for the equations relating the expected stratum sizes at successive points in time have now been introduced. The equation to be presented describes the size of each of the system's strata as the result of varying types of manflows. In brief, the derivation of expected manpower distribution at time t is produced by multiplying appropriate time homogeneous transition probabilities by the actual or expected manpower distribution at time t-1 and by the size of the recruit cohort entering during (t-1, t). We therefore have

$$\bar{n}_j(t) = \sum_{i=1}^{k} p_{ij}\bar{n}_i(t-1) + \bar{R}(t)p_{oj} \quad (11)$$

where

$$\bar{R}(t) = M(t) + \sum_{i=1}^{k} p_{i,k+1}\bar{n}_i(t-1)$$

$$t = 1, 2, \ldots$$
$$j = 1, 2, \ldots, k$$
$$\bar{n}_i(0) = n_i(0).$$

The equations for estimating the parameters of this model will be designated following the definition of terms. These terms are as follows:

1. $n_{ij}(t)$ the observed number of men moving from stratum i to stratum j during the time period (t, t+1);
2. $n_i(t)$ the observed number of men in stratum i at the beginning of the time period (t, t+1);
3. $n_{io}(t)$ the observed number of men leaving the system from stratum i during the time period (t, t+1);
4. $n_{oi}(t)$ the observed number of men entering the system at stratum i during the time period (t, t+1);
5. $n_{o\cdot}(t)$ the observed number of men entering the entire system during the period (t, t+1);
6. $\hat{p}_{ij}$ estimated value of p_{ij};
7. $\hat{p}_{oj}$ estimated value of p_{oj};
8. $\hat{p}_{i,k+1}$ estimated value of $p_{i,k+1}$.

The following equations give us the maximum likelihood estimates:

$$\hat{p}_{ij} = \frac{\sum_t n_{ij}(t)}{\sum_t n_i(t)}, \quad i \neq j \quad (12)$$

$$\hat{p}_{i,k+1} = \frac{\sum_t n_{io}(t)}{\sum_t n_i(t)} \quad (13)$$

$$\hat{p}_{ii} = 1 - [\hat{p}_{i,k+1} + \sum_{\substack{j=1 \\ i \neq j}}^{k} \hat{p}_{ij}] \quad (14)$$

$$\hat{p}_{oj} = \frac{\sum_t n_{oj}(t)}{\sum_t n_{o\cdot}(t)}. \quad (15)$$

Since more than one move by the initial population of men is not taken into account by the demographic model, a time span such that only one move per man may occur is the

Table 4. Transitions of Personnel from Stratum by Decade

Origin Stratum and Decade	Destination Stratum: Col., Capt.	Lt.	S/Sgt. Sgt.	Cpl., Det.	Tpr.	Out	Total Number of Moves (N)
Col., Capt.							
1960-1969	.8550					.1450	200
1950-1959	.8435					.1546	147
1940-1948	.9320					.0680	103
1930-1939	.9048	.0667				.0286	105
1927-1929	.9474					.0526	19
Lt.							
1960-1969	.1569	.7402				.1029	204
1950-1959	.1573	.7416				.1011	178
1940-1948	.0638	.8936				.0851	47
1930-1939	.1263	.6842	.1368		.0105	.0421	95
1927-1929	.1282	.7949				.0769	39
S/Sgt., Sgt.							
1960-1969		.0466	.8603	.0008		.0923	1181
1950-1959		.0689	.8635			.0677	813
1940-1948	.0058	.0175	.9259	.0019		.0487	513
1930-1939		.0366	.9231	.0037	.0147	.0220	273
1927-1929		.1591	.7727			.0682	44
Cpl., Det.							
1960-1969			.0793	.8824	.0023	.0360	2586
1950-1959			.0929	.8907	.0019	.0144	1593
1940-1948			.1120	.8720		.0160	375
1930-1939		.0214	.1250	.7500	.0893	.0143	280
1927-1929			.0610	.8049		.0341	82
Tpr.							
1960-1969				.0563	.9037	.0400	8295
1950-1959				.0513	.9080	.0407	5229
1940-1948			.0034	.0452	.8417	.1097	2918
1930-1939			.0013	.0401	.8730	.0855	1520
1927-1929			.0105	.1053	.4737	.4105	190

maximum allowed. In the State Police system, moves from the Sergeant stratum and above are possible after a waiting period of one year from the date of entrance. Hence, for this particular system the time span is limited to periods of one year.[6]

4.3.2 Examination of Basic Assumptions

As with the vacancy chain model, an examination of the assumption of stationarity finds the transition probabilities obtain a rather high degree of stability only in 1950 and thereafter.[7] Transitions of personnel by decade are presented in Table 4. The stability between the 1950 and 1960 decades is particularly significant: it extends across all strata and the variation is quite low. The largest difference between transitions is .03 and in the great majority of cases the difference is less than .01. Less stability holds in the

[6] Though Bartholomew develops further analytical work concerning the limiting structure (1973), it is generally inapplicable to this particular system. While the Q matrix (see Bartholomew) is stochastic, it is neither triangular nor regular. Moreover, for a governmental system such as the State Police, political and economic fluctuations are too great to permit constant or geometric growth rates.

[7] The discrepancy between the two models for the year in which stability was ascertained to have been reached is due to the fact that some men originally in the Lieutenant stratum in 1949 moved more than once during the year. The demographic model allows but one move per man per year. Hence, there does not exist a 1949 transition matrix for the demographic model and for 1949-1969; the earliest year for consideration is 1950.

pre-1950 decades. In 1927-1929, transitions are variant from those of the 1950 and 1960 decades in Tpr., S/Sgt.-Sgt., and Col.-Capt. strata. In the Depression years, the Cpl.-Det., Lt., and Col.-Capt. strata vary, while in the 1940s the greatest departure occurs in the Lt. stratum. Still, even for the 1940 decade, the stability across decades does not approach that attained in the 1950-1970 period.

A second assumption refers to the mobility process having a path independence or Markov property. That is, the probability that a member of stratum i will move to stratum j is conditional only upon his present state or location, not the history of his locations. Although there is no immediate method of checking this assumption, there is reason to believe the assumption valid. First, movement is of a one-step nature, occurring across adjacent status strata rather than occurring to any stratum. Secondly, movement is almost always in an upward direction. It is generally true that $p_{ij} = 0$ for $i < j$ (demotions) and also for $j < i\text{-}1$ (2-step promotions). In addition, there are minimum waiting periods for movement. These three facts indicate that it is one's stratum position which serves as a selective factor for movement to another stratum rather than one's historical path. It seems reasonable to assume the process is Markovian.

The final assumption relates to homogeneity of stratum populations. The assumption is that all members of each stratum are subject to identical sets of transition probabilities.

> It is this assumption which permits one to use the *proportion* of persons making a particular transition as an estimate of the corresponding transition probability to which any particular person is subjected. (McFarland, 1970:464)

The assumption is probably not correct since certain selective factors are operative. For instance, it has generally been the case that a one-year waiting period was required within any stratum before further promotions were possible. In addition, seniority in terms of duration of service in the stratum has had an important influence. (See *Rules and Regulations of the Michigan State Police,* 1936 and 1945.) As for changes in procedure for promotions since 1954, the greatest apparent change has been the inclusion by the Civil Service Commission of written examinations for several of the promotions. While these tests may possibly nullify to some extent the effect of seniority, they by no means have made it unimportant. Although the general nature of the promotional criteria and decisions is known, the exact weighting of specific criteria over time is not. Hence, from an administrative point of view, data are lacking to test the import of seniority. An inexact method does exist. We might examine the effects of seniority upon promotion by looking at career histories of certain cohorts. This was not undertaken in this particular study. Instead logical criteria and the general understanding of the system were utilized. Several factors seemed important—the waiting period, the effect of seniority beyond this waiting period, the effect of written examinations, one's location in the geographical region which is expanding at the fastest rate at particular time periods, previous interactions with those being promoted at the fastest rates and rate of expansion of the administrative versus the detective "wing" of the hierarchy. The list could obviously have been extended further. However, one important criterion precluded such an extension: the approximation to stationarity of transition probabilities from 1950 to 1970.

5. TESTING THE MODELS

Of what utility are the models for forecasting? Analyses of each interpreted model are reported below.

5.1 Vacancy Chain Model Test

From our examination of the transition probabilities from 1927 through 1969 (Section 4.2.3), it was only in 1949 that the system's internal dynamics seem to have reached some degree of long-term stability. Hence, for present purposes it is the years after 1949 which are of most interest—years in which the assumption of stationarity appears sufficiently reasonable. The data upon which yearly transition probabilities are based, however, point to a problem for testing purposes. The number of chains per year are not sufficient for a test of the upper strata. Two possible approaches exist: combining strata or combining yearly data.

The first possibility—for instance, combining the two upper strata—means deciding the

present 5-strata model is not a Markov chain. To be more specific, *if* both the original 5-strata and the new 4-strata (from lumping the upper 2 strata together) systems are Markov chains, then $q_{13} = q_{23}$. That this condition is not met is easily seen in the transition matrix for job vacancies, Table 1. In the State Police $q_{ij} \approx 0$ if $i > j$ or $j > i + 1$. Obviously, $q_{23} \not\approx 0$. That is, some vacancies from the Lt. stratum do enter the S/Sgt.-Sgt stratum. Also, $q_{13} = 0$ or Col.-Capt. to S/Sgt-Sgt. moves do not occur. Since $q_{13} = 0$ and $q_{23} \neq 0$, the lumpability condition for a Markov chain is not satisfied (cf. Kemeny and Snell, 1960). Therefore, one of the two stratified systems cannot be a simple Markov chain. Consequently, to lump the upper two strata is itself a decision that the processes do not conform to a 5-state Markov chain. Moreover, such a decision would be empirically uninformed since the yearly data are inadequate for a test.

The alternative, combining yearly data, seems the better choice. First, yearly stationarity was approximated for a 21-year period. Secondly, a collapse of the two upper strata would lose much substantive information about the system. The 5-state description seems to be more practical and meaningful. It is practical since more than five strata certainly cannot be tested. It is substantively meaningful because fewer than five strata provide too little important information.

For this study, vacancy chains were labelled "yearly" if the job vacancy initiating the vacancy chain began in that year. White called his vacancy chains "yearly" if any vacancy of the chain occurred within the year sampled from the decade. In both cases a standardized procedure for "yearly" was used. Yet, this standardization is simply an artifact for examining the data. By thinking of the processes sequentially, one can readily see that the processes such as movement of job vacancies have no necessary beginning or ending points once the process is in motion. Therefore, any standardized method of partitioning the dynamic process is likely to have overlapping segments. If we trace a job vacancy chain which has begun at time 1 until its end at time 12 (for example, a 12-month period), other job vacancy chains have likely begun at times 2, 3, ... Hence, the chronological breaking points at time periods which we are accustomed to thinking about, such as years, are purely arbitrary breaking points. The system's processes do not stop, accelerate or decelerate according to calendar years, but according to internal policies, political pressures both from within and without, economic fluctuations, turnover of high level administrators, stages of structural differentiation, number of man-hours worked per week, technological innovations and international wars.

The period we have chosen to test the model is 1958-1963 in which the state's economic instability precluded any system growth. Since the transition estimates (Table 1) include extreme growth years and movement resultant from such growth, the test concerns the general system dynamics and their sensitivity to such exogenous changes. The data are reported in Table 5.

The 1958-1963 predictions of chain length distribution by stratum of origin are quite accurate. They are off in three instances by 16, 14 and 10 percent. Otherwise, in over 25 cases the difference is generally less than .05, or five percent between predicted and observed distribution values. The distribution of errors is given in Table 6. An inference which can be made from the predictions is that the dynamics of the system were extremely stable during the somewhat "holding" period.

Overall during the more extended time period (1949-1969), the predictions for both chain length distribution and the total number of moves from each stratum were also quite accurate, suggesting the general adequacy of the model for predicting interrelated dynamics of occupational mobility within the police system (cf. Stewman, 1973). Particularly noteworthy was the finding of a 21-year period from 1949-1969 in which the underlying dynamics of the police system were rather stable in spite of many obvious historical changes.

5.2 Demographic Model Test

Once again a direct examination of the transition probabilities from 1927 through 1969 (Section 4.3.2), revealed that it was only in 1950 that a reasonable degree of long-term stability began. It is in this period of stationarity that the model's predictive power may be tested. Combinatorial schemas are unnecessary for this model since movement refers to individual manflows rather than entire chains in which several individual moves occur. Thus,

Table 5. Predicted (P)* and Observed (O)** Distribution of Vacancy Chain Length by Stratum of Origin for General System Based Combined Data: 1958-1963 (Percent)

	Stratum of Origin									
	Col., Capt.		Lt.		S/Sgt., Sgt.		Cpl., Det.		Tpr.	
j[a]	P	O	P	O	P	O	P	O	P	O
1	12.5	11.1	13.5	20.0	10.0	5.6	10.4	11.1	94.7	87.7
2	11.6	27.8	9.2	5.0	9.4	16.9	61.0	61.7	4.7	7.5
3	8.5	5.6	9.0	10.0	37.0	32.4	20.0	18.5	.4	4.1
4	7.7	.0	27.6	20.0	23.8	21.1	6.0	6.2	.1	.7
5	20.0	5.6	20.8	10.0	11.5	18.3	1.8	1.2	.0	.0
6	18.0	27.8	11.1	20.0	5.0	4.2	.6	1.2		
7	11.2	5.6	5.2	15.0	2.0	1.4	.2	.0		
8	5.8	5.6	2.2	.0	.8	.0	.1	.0		
9	2.7	5.6	.9	.0	.3	.0	.0	.0		
10	1.2	5.6	.4	.0	.1	.0				
11	.5	.0	.1	.0	.0	.0				
12	.2	.0	.1	.0						
13	.1	.0	.0	.0						
14	.0	.0								
15										
16										
(N)	18		20		71		81		146	

*Estimates of transition probabilities are from Equations (4) and (5) for 1949-1969. See Table 1.

**Observations are for 1958-1963 data.

[a]j denotes chain length.

yearly predictions will be made over the extended time period (1950-1970). In the test, we will be concerned with the model's predictive power and its sensitivity to perturbations from such factors as accelerated growth and internal policy changes. Observed data used in the predictions are system growth [M(t)] and the initial strata sizes n_i (1950). After 1950 all predictions are based on derived strata size, $\bar{n}_i(t)$. We assumed the system either grew or held its size. For years in which system contractions occurred, M(t) was set at zero. Moreover, entrants from the outside over the 20 years were distributed as follows: Col., Capt.: 1, Lt.: 1, S/Sgt., Sgt.: 1, Cpl., Det.: 10, Tpr.: 1,999. Therefore, we set $\hat{p}_{oj} = 1$ for j = Tpr.

The predictions and observations for the 20-year period are reported in Table 7. From the observed values, we find there is a cyclic growth process for all strata which the model cannot forecast. In particular, in the Cpl., Det. stratum the model cumulatively overpredicts from 1959 to 1966. By 1966, however, the system's growth in this stratum was accelerated such that the remaining predictions are

Table 6. Distribution of Errors for Predicted Vacancy Chain Length by Stratum of Origin: 1958-1963 (Percent)

Chain Length	Col., Capt.	Lt.	S/Sgt.; Sgt.	Cpl., Det.	Tpr.
1	1.4	-6.5	4.4	-0.7	7.0
2	-16.2	4.2	-7.5	-0.7	-2.8
3	2.9	-1.0	4.6	1.5	-3.7
4	7.7	7.6	2.7	-0.2	-0.6
5	14.4	10.8	-6.8	0.6	
6	-9.8	-9.9	0.8	-0.6	
7	5.6	-9.8	0.6	0.2	
8	0.2	2.2	0.8	0.1	
9	-2.9	0.9	0.3		
10	-4.4	0.4	0.1		
11	0.5	0.1			
12	0.2	0.1			
13	0.1				

very accurate. Prior to 1959, the predictions for this stratum were also very accurate.

Observations for the S/Sgt., Sgt. stratum also reveal cyclic growth patterns (e.g., 1957-1958 and 1968-1970). The linear model's predictions are rather accurate until 1966 at which point the cumulative linear growth rate is obviously too great. The observed fluctuations within the Lt. stratum do not reveal a discernable pattern. Perhaps, a fluctuating age structure and hence retirements would account for the manner of observed changes. In any event, the predictions for this stratum appear generally reasonable between 1959 and 1963. Thereafter, the model again cumulatively overpredicts. The Col., Capt. predictions appear much more accurate, especially considering the size of the upper two strata. Finally, for the Tpr. stratum the greatest errors occur between 1957 and 1963, a period in which general system size declined due to economic instability. The errors are thus largely accounted for by the assumption of zero or positive growth. Setting $M(t) = 0$ for 1958 and 1959, for instance, would account for a cumulative overestimate of 50 men. With this in mind, the error in this stratum due to promotions or exits seems rather small.

For the great part, the cyclic growth patterns and the model's largest errors are accounted for by two perturbations. The first occurred in 1956-1957, the second in 1958-1963. Expansion accelerated in 1956. More specifically, there was a major change in transition probabilities throughout the system due to the greatest expansion in the system's history. The Legislature authorized 257 new positions (a 35 percent increase) during 1956 primarily as a result of increasing highway deaths. The inflows and outflows of S/Sgt., Sgt.; Cpl., Det.; and Tpr. strata were greatly affected. After 1956 the system manpower flows again stabilized to the pre-1956 level. However, since the predicted errors are cumulative, the predictions for the years immediately following were also off. The second major change was a period of zero growth. Between 1958 and 1964, growth occurred in only one year—an increase of 15 positions in 1961. The effect of the economic recessions was considerable on state level governmental processes. It impacted state police manpower change directly. As for the model's capacity,

Table 7. Predicted (P) and Observed (O) Stratum Sizes by Year Based Upon 20 Year Maximum Likelihood Estimations*

Year		Stratum				
		Col., Capt.	Lt.	S/Sgt., Sgt.	Cpl., Det.	Tpr.
1951	P	13	12	69	129	419
	O	13	15	71	133	409
1952	P	13	13	70	137	417
	O	12	17	71	149	400
1953	P	13	14	72	144	436
	O	14	19	72	148	425
1954	P	13	14	74	151	426
	O	14	19	72	146	404
1955	P	13	14	77	157	468
	O	14	17	79	146	450
1956	P	13	15	80	164	491
	O	14	18	82	146	480
1957	P	13	16	83	172	749
	O	17	19	92	185	697
1958	P	14	16	86	193	861
	O	17	24	104	205	797
1959	P	14	17	90	217	831
	O	19	19	102	214	767
1960	P	15	18	96	237	804
	O	19	21	105	221	731
1961	P	16	19	103	253	779
	O	19	23	109	233	703
1962	P	17	20	110	266	772
	O	19	22	111	232	718
1963	P	18	21	117	277	752
	O	21	19	116	230	713
1964	P	19	22	124	286	734
	O	18	17	118	232	714
1965	P	20	23	131	293	845
	O	19	17	120	228	842
1966	P	21	24	138	305	839
	O	19	20	115	231	856
1967	P	22	25	145	315	968
	O	22	17	121	297	932
1968	P	23	27	152	331	1038
	O	22	24	129	333	976
1969	P	24	28	159	349	1168
	O	22	24	137	349	1110
1970	P	25	30	166	372	1202
	O	22	24	146	389	1128

*Equations (12), (13), and (14) where t = 1950, ..., 1969. The estimates are given in Table 3.

its equations are linear and thus cannot account for such fluctuations. However, the cyclic nature of the growth, from extreme expansion to zero growth, allowed the errors of the first effect to be corrected by those of the second effect. Moreover, the errors in the Cpl., Det. stratum of increasing over prediction of personnel from 1959 to 1966 resulted from the no-growth period. A further perturbation affecting this predictive error occurred in 1966. Manpower flows changed as a result of a shift from a 54 to 48 (1963) and then 48 to 40 hour work week, the latter occurring in 1966. Moreover, the latter Civil Service ruling instituted not only a 40-hour but a 5-day work week. The effect of this ruling was to increase the amount of manpower necessary to maintain the same amount of service and to generate a policy of a minimum of three Cpls. per post. It was the latter policy of at least three Cpls. per post which increased transitions from Tpr. to Cpl. in line with the model's predictions. Thus, after 1966 the model's predictions are rather accurate. Two observations seem pertinent. First, forces generating changes in manflows are not specified by the model; for example, economies of scale for administrators, structural differentiation by the addition of new services and time dependencies related to state economics. Second, it appears the model will in the long run tend to over-predict upper level strata; for example, see the 1970 row of Table 7. The latter observation may, however, be wrong in that twenty years is a rather long period and the model has predicted rather well. The conclusion regarding the model's predictive power may be better grasped by examining the relative errors of the 20-year predictions we have been discussing. Table 8 reports these data. With the exception of the Lt. stratum, the model's predictions seem fairly accurate, as indicated by the 20-year average in relative error. With the exercise of care that transition probabilities remain stable or are re-estimated in cases of major change, the model's utility for deriving system evolution seems very good.

6. DISCUSSION

The principal purposes of this paper were to introduce a refreshing approach to occupational mobility, to provide formal models specifying examples of the new approach, to describe each model's representation of mobility processes by examining its underlying conceptualization of the problem, to interpret the models for a particular internal labor market and to test each model's predictive accuracy. The open system approach via the stochastic models was found to have a high utility in the internal labor market analyzed. First, each model provides a specific means of examining system dynamics over time. The comparison of transition probabilities themselves is important in that their stability denotes an underlying stability in the structural dynamics of the system in spite of observable surface level change. Such general structural stability was found over a 21-year period (1949-1969). Second, each model derives predictions regarding specific system dynamics—in the one case, the distribution of vacancy chain length, in the other the manpower system's evolution. The predictions from both models were found in general to be highly accurate. Third, not only are the components of the mobility processes delineated by the models but their relations are specified providing an explicit and precise conceptualization of the mobility processes. The strengths and weaknesses of the models' conceptualizations were found to be somewhat complementary. However, the vacancy chain model's representation was more comprehensive structurally. Fourth, specific structural constraints operative upon individuals' work lives were analyzed. They included the boundedness of the labor market considered (no lateral entry), the type of flows across strata (one step, one direction), the mechanism generating a move (a job vacancy), the interrelatedness of movement (a job vacancy chain), the impact of general system dynamics (1956 expansion, 1966 policy change for the work force [e.g., three Cpls. per post], and the small, but cyclic fluctuation of the p_{ij} (pre-1958, 1958-1963, post-1963), suggesting the political system's dependency on the cyclic fluctuations of state economics.

Since the study was of a single job system, a basic question remains: What is the scope of applicability of the conceptualization and models? This study's scope was restricted to one particular type of internal labor market, a police hierarchical promotion system. The organizational or open system conceptualization is, however, generally applicable to occupational systems (i.e., occupations within

Table 8. Relative Error[a] between Stratum Size Observed and Predicted[b] from the Demographic Model

Year	Stratum				
	Col., Capt.	Lt.	S/Sgt., Sgt.	Cpl., Det.	Tpr.
1951	.0	.25	.03	.03	.02
1952	.07	.31	.01	.09	.04
1953	.07	.36	.0	.03	.03
1954	.07	.36	.03	.03	.05
1955	.07	.21	.03	.07	.04
1956	.07	.20	.03	.11	.02
1957	.31	.19	.11	.08	.07
1958	.21	.50	.21	.06	.07
1959	.36	.13	.13	.01	.08
1960	.27	.17	.09	.07	.09
1961	.19	.21	.06	.08	.10
1962	.12	.10	.01	.13	.07
1963	.17	.10	.01	.17	.05
1964	.05	.23	.05	.19	.03
1965	.05	.26	.08	.22	.0
1966	.10	.17	.17	.24	.02
1967	.0	.32	.17	.06	.04
1968	.04	.11	.15	.01	.06
1969	.08	.14	.14	.0	.05
1970	.12	.20	.12	.05	.07
1951-1970 Average	.12	.23	.08	.09	.05

[a] $\frac{|\text{Observed} - \text{Predicted}|}{\text{Predicted}}$

[b] Using maximum likelihood estimates.

which mobility is generally limited). The models have also been applied to university faculty systems (Young, 1965), clergy systems (White, 1970), the British Civil Service (Forbes, 1971; Sales, 1971), and a national insurance company (Mahoney and Milkovich, 1971). Moreover, the conceptualization (general or either specific model) represents an open system, whether or not the limiting boundaries for movement are formally organized. Hence, the conceptualization is general, extending to formal organizations, professional and craft internal labor markets, and to occupational groupings which are perhaps less formally organized but within which exchanges and/or one-way flows of personnel occur.[8] Clearly, from the current applications and from the discussion of the underlying conceptualizations, we may conclude that the models are occupational system models, including but also extending beyond formal

[8] For a discussion of internal labor markets, see Caplow (1954:59-123, 142-80) and Doeringer and Piore (1971). Patrick Dorelan has pointed out that perhaps the models' applicability is more to the primary labor market than the secondary labor market. For some secondary jobs, the application of an internal labor market is problematic, but the models are generally applicable to both markets. For a brief discussion of the application of internal labor markets to the secondary work force see Doeringer and Piore (1971:163-83).

organizations. Hence, if we can specify the boundaries for movement, the models are also applicable to inter-organizational or disaggregated general labor market movement.

Perhaps the principal reason an open system approach has not generally been applied by sociologists, who are generally strong on structure, is the problem of data collection. The first problem is to delineate a system of jobs. Once these bounded sets are defined, longitudinal data are necessary, presenting two additional problems. First, such systems do not typically have the type of memories that we can tap in a few moments. Instead, tedious record reviews seem necessary, requiring access as well as existence (i.e., not thrown away). The second data collection problem is that such systems will in some cases, have units which are a "considerable" distance apart requiring a cooperative data collection effort or "considerable" monies. For example, large corporations may possibly be considered a system. In this case, system and internal labor market (as used by economists) are synonymous. However, if a large number of exchanges or one-directional flows exist between two formal organizations (e.g., corporations), then the occupational labor market contains sufficient elements of both internal labor markets and competitive markets. In the latter case, internal labor market and bounded occupational system are clearly not the same. Initial studies could perhaps be confined to formal systems such as bureaucracies (see White, 1970; Mahoney and Milkovich, 1971; Sales, 1971; Forbes, 1971), or ecological units such as cities.

What are the major limitations of the models presented? First, the demographic model represented only an expanding system.[9] Second, the analysis indicated that perhaps one of the major assumptions, stationarity is limited to the technological, political or economic conditions. Consequently, parameters might have to be re-estimated for time periods separated by major perturbations.[10] Third, the models did not conceptualize differences between individuals within strata. Such specification was not necessary in this study.[11]

[9] Modification to a system of constant size is easily achieved by setting $M(t) = 0$. However, for declines in system size theoretical work has just begun (see Bartholomew, 1971; Young, 1965).

[10] More theoretical work is needed concerning specification of the conditions of change for occupational mobility processes. Moreover, modifications of a variety suggested by other researchers (e.g., Blumen et al., 1955; McGinnis, 1968; McFarland, 1970; Mayer, 1972) do not appear helpful since the source of the fluctuations did not appear to be differential movement of certain individuals within a given stratum. Rather, the major sources of fluctuations appeared to be external economic or political change and internal policy decisions.

[11] Neither was it necessary in other open system analyses (see White, 1970; Forbes, 1971; Sales, 1971). However, it was necessary to conceptualize seniority per stratum for Young and Almond (1961). Additional analytical work for open system heterogeneity has also been conducted by Young (1971), Bartholomew (1973) and Gilbert (1973).

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MODELS OF STATUS INCONSISTENCY AND SOCIAL MOBILITY EFFECTS*

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The author draws out the conceptual implications of technical procedures which he employed in an analysis published in this journal (Hope, 1971). He defends an earlier statement of status-inconsistency theory (Lenski, 1954; 1956) against attacks on its coherence, and he provides a model which accurately represents the theory (and an equivalent theory of mobility effects) and may be used to test it. By contrast with this "diamond" model, he shows that the usual "square-additive" model cannot test for the presence of status inconsistency or mobility effects, whether these are defined additively or interactively. The technique of "design matrix regression analysis" is introduced as a means for exploring the relations between models in order to grasp their structure and implications before they are fitted to data. The square-additive model is criticized for its conceptual vagueness, but a theoretical position is suggested which incorporates the main positive feature of the model while constituting a worthy rival to inconsistency theory.

Status Inconsistency Theory

The purpose of this paper is to reopen an issue which many sociologists regard as settled, by questioning a model which has been generally accepted and widely used. In this undertaking we are faced with three serious problems. The first is the obvious difficulty of persuading readers to reconsider a position which has, in many cases, become part of their standard intellectual equipment. The second is the existence of alleged ambiguities in statements of the theory which the model is supposed to test. And the third is the absence of any detailed explication of the model itself which would make clear which of its features are essential, and which are adventitious, to its claim to test the theory which it purports to test.

The theory to which we address ourselves is the theory of status inconsistency as set out by Lenski in his paper "Status crystallization: a non-vertical dimension of social status" (Lenski, 1954; "consistency" has now replaced "crystallization" as the usual term for the phenomenon). Lenski says:

> The basic hypothesis tested in this study is as follows: individuals characterized by a low degree of status crystallization differ significantly in their political attitudes and behavior from individuals characterized by a high degree of status crystallization, when status differences in the vertical dimensions are controlled.

The argument of the present paper turns on the appropriate interpretation of the phrase "when status differences in the vertical dimensions are controlled."

Having stated a general theory, Lenski distinguished one possible way in which such a difference might manifest itself. Observing that "without common scales [on his four status hierarchies or axes], a measure of status crystallization would be impossible" he establishes his "quantitative measure of status crystallization . . . by taking the square root of the sum of the squared deviations from the mean of the four hierarchy scores of the individual and subtracting the resulting figure from one hundred."

As he makes clear in this and in a subsequent paper (Lenski, 1956), the sug-

*The earliest version of this paper was cited in a note published in this Journal (Hope, 1973). Successive drafts benefited from detailed critical scrutiny by Professor O. D. Duncan, Professor R. W. Hodge and the editor. The author is also indebted to Mr. J. S. Flemming, Miss A-M. Skrimshire and Miss P. Thorburn for comments. The penultimate draft was delivered to the World Congress of Sociology meeting in Toronto, August, 1974.

gested quantitative measure of sheer extent of inconsistency is not the only possible representation of inconsistency; it is also possible to distinguish different patterns of inconsistency, and these might be represented by contrasts between hierarchies in which the signs of differences are retained. I refer to such signed contrasts as "status discrepancies."

Although Lenski later endorsed the attempt to represent his theory which I am about to criticize (Lenski, 1964), I can see no significant incoherence or ambiguity in his two original papers. It is true that he sets out at least two ways in which inconsistency effects might occur, and it is also true that, at first sight, there might appear to be some difficulty in reconciling his verbal formulations of the theory; but both these difficulties vanish if we concentrate on his fundamental point, which is that previous workers, such as Warner, have ignored possibly important aspects of their data by aggregating the positions of persons across different status dimensions. Lenski is simply saying that the singular "vertical" dimension studied by Warner[1] may not be the only one to affect political action (or other dependent variables), and in order to investigate this we must define non-vertical dimensions of one sort or another. It is true that he speaks of "vertical dimensions" in the plural, and he refers to status axes as "parallel vertical hierarchies which usually are imperfectly correlated with one another." But the apparent ambiguity is immediately resolved if we consider and compare the two most common diagrammatic ways of representing the profile of a person across a set of axes. One method is to draw a set of parallel vertical lines such as those on a personality profile chart and to represent the individual by a stylized graph. The second method is to draw a vector diagram in which axes appear like the arrows under the American eagle; the cosine of an angle between vectors being set equal to the correlation between the variables which they represent. The position of an individual is then represented by a point in the vector space. There is no obvious way of representing a "non-vertical dimension of status" on the "parallel vertical hierarchies" of the profile chart, whereas in the vector space a non-vertical dimension is any dimension orthogonal to the singular dimension which we choose to stand for the vertical dimension. The vector representation of status axes has been lucidly discussed by Jackson and Curtis (1968) and it is an essential element in Hope's (1972a) concept of a "stratification space."

Lenski's theoretical statement has been discussed at some length because comments by sociologists on earlier drafts of this paper revealed that the 1954 and 1956 papers were commonly believed to be confused and incoherent, and the quotations given above were cited as evidence of this.[2] In fact, however, all his statements can be interpreted in the light of one or other of the two diagrams which have been described, and these are merely alternative ways of representing the same situation. The appropriate criticism which could be advanced is not that the theory is confused, but that the means suggested for testing it are not quite strong enough for their purpose. The logic of the theory is that an effect of status inconsistency *per se* has been established only if the inconsistent person differs from the consistent person who lies at the same level of the vertical dimension. Lenski, however, confined himself to checking that the mean of all inconsistent persons on the dependent variable does not differ from the mean of all consistent persons. Brandmeyer (1965) recognized the inadequacy of this control but did not succeed in producing a better one.

Lenski's proposal for preserving information omitted by earlier workers is quite

[1] Cf. also Sorokin's (1927) emphasis on the vertical dimension in *Social Mobility*. Sampson (1963) gives a clear statement of the historical background of Lenski's work.

[2] This generally-held opinion may well have been important in persuading subsequent writers that they need not take the theory too seriously and that they were at liberty to redefine it in terms of their own models. Whatever Lenski may have said elsewhere, I can find no justification for the view that he was confused in the 1954 and 1956 papers. On the contrary, the confusion occurred with the introduction of the additive model (which is discussed below), and with the failure of its proponents to explain why they thought it adequately represented the theory. The crucial point to grasp is that Lenski was starting from the current practice of examining only a single vertical dimension, and he (like Benoit-Smullyan, 1944) was asking us to consider the possibility that in doing this we are ignoring potentially important aspects of the data, which might take a variety of forms.

straightforward: granted that we wish to work with a vertical dimension, somehow defined, then we can preserve all the available linear information by retaining as many dimensions at right-angles to the vertical (and to one another) as there are degrees of freedom left when the space is collapsed across the vertical dimension. If we wish also to look at certain interactions among these dimensions, then we can do so by defining appropriate contrasts such as absolute deviation from the vertical dimension. An alternative way of preserving all the linear information present in the data is to retain all the original status variables without defining a vertical dimension and *a fortiori* without defining any non-vertical dimensions perpendicular to it. The critical purpose of the first part of the present paper is to point out that subsequent empirical work on status inconsistency has fallen into conceptual confusion by representing the second alternative as a model of the theory which was stated in terms of the first. The paper develops themes which were introduced in an earlier paper published in this journal under the title "Social mobility and fertility" (Hope, 1971).

In 1964 Mitchell published a discussion of status inconsistency theory as it had been formulated by Lenski and by Jackson (1962). The important point to note about Mitchell's paper is that it was quite explicitly a proposal to supplant status inconsistency theory by the alternative theory formulated in terms of the original status axes. But it was subsequently misinterpreted as a statement, not about the theoretical priority but about the conceptual coherence, of status inconsistency theory.[3] Mitchell's position is made quite clear when he writes, with reference to Lenski and Jackson:

> However, rather than first allowing the traditional methods and variables to account for as much of the variation as possible and relating crystallization to the residuals, the authors begin with their crystallization measures and then work backwards.

By contrast, Lenski (1954:405), had applied the adjective "traditional" to the concept of a single unidimensional hierarchy. To put the point crudely, Lenski found himself with a vertical dimension and only a vertical dimension, and he set out to elaborate his theoretical baggage by the addition of non-vertical dimensions. Mitchell found himself equipped with "traditional" variables from which no vertical dimension had been derived and he therefore had no need to specify non-vertical dimensions. Both authors outlined self-consistent theoretical positions, but subsequent writers confused the picture by representing the latter as a means for testing the former. There is in fact no empirical way of deciding between the two theories because one is merely an orthogonal rotation of the other.[4] If, for example, one and only one of Mitchell's original status axes[5] is related to the dependent variable, and if Lenski's vertical dimension is not identical with that axis, then a status inconsistency effect is present in the data.

There is a further respect in which Mitchell's arguments constitute an attempt to bypass, rather than to overthrow Lenski's theory, and it may be mentioned here in order to avoid further confusion. Mitchell criticized Lenski for concentrating on status inconsistency rather than on variables which might be supposed to intervene between inconsistency

[3] Blalock (1966:59) says "Hyman and Mitchell argue in effect that the identification problem (though they do not use this term) has in the past . . . ." Mitchell's paper has nothing to say, either explicitly or implicitly, on the concept of identification. Hyman's (1966) paper points out that there is no way of choosing empirically between Lenski's and Mitchell's formulations and inclines to the latter. The problem of identification does not arise until we attempt to use Mitchell's model as a *means for testing* the existence of Lenski's status inconsistency effects. So long as they are merely alternative ways of looking at the same data (i.e., alternative sets of axes spanning the same stratification space), and we do not introduce aspects of one into a model based on the other, then identification is not an issue.

[4] They are special cases, respectively, of models three and two in my paper "Social mobility and fertility" (Hope, 1971) where I give the rotation matrix explicitly.

[5] This is clearly the alternative which Mitchell thinks is most likely to explain Lenski's findings, his preferred variable being ethnicity. It is true that he also speaks of combining the status variables, but in referring to this he seems to use "interaction" and "addition" interchangeably, and he rather mysterlously suggests that we add dimensions together "one at a time." The fully-fledged model involving recourse to additive combinations of *all* the status axes is not really explicit in Mitchell's paper.

and the dependent variable. In particular, he reserved the term "status incongruence" to refer to reversals in relations of dominance and subordination. The distinction between inconsistency and incongruity is quite straightforward. Incongruity is a relational phenomenon which takes the form of saying that (individual or group) A prevails over B in one situation, but not in another. Inconsistency, on the other hand, is a distributional phenomenon which differs from incongruity in that one can speak of the inconsistency of a single individual or group, without explicit reference to a second individual or group, simply by specifying its position on a set of axes. Mitchell's proposal was that incongruity, being an observable phenomenon of social interaction, is more worthy of study than the remote structural condition of inconsistency. It may be replied that while incongruity is undoubtedly a legitimate object of investigation there is no reason to suppose that it is invariably pathological—it may simply reflect a division of social labour—or that it is an invariable concomitant of inconsistency, or that it is the only possible mechanism transmitting the effects of inconsistency to a dependent variable. As Mitchell himself says (1964:318n), "A concern with single individuals [i.e., with inconsistency] is, of course, just as legitimate as a concern with social relations. However, the investigator should be clear about the distinction between the two."

The Linear Additive Model

Two models have been advanced as ways of testing for the existence of status inconsistency effects. The first is the linear additive model (Lenski, 1964; Blalock, 1967). In his paper, Lenski acknowledges his indebtedness to Blalock and frankly admits that his knowledge of the statistical properties of the model is limited. He was clearly not aware that it misrepresented his original theory. Since his initial presentation of the theory is sound and he admits his uncertainty over the statistics, the most reasonable interpretation of his position is to concentrate on the theory and to ignore his endorsement of the model. The second model to be suggested as a test for the presence of status inconsistency effects is the non-linear additive model which is discussed below.

Whatever these two models do, they cannot test for the existence of status discrepancy effects (correlates of signed contrasts between the status axes) even though a status discrepancy effect was taken by one of their authors[6] to be the most obvious example of a status inconsistency effect (Blalock, 1966b; 1967; cf. also Hodge, 1970). This point was, of course, made by Blalock when the linear additive model was first introduced, and Hodge gives a very clear algebraic account of the reasons why it is so. The question one must ask, therefore, is why research workers persevered with the new model when it proved to be incapable of testing for the existence of status inconsistency effects in the most straightforward sense of that term. No explicit defence of this perseverance appears to have been made, but it may be surmised that the identification problems faced by the linear additive model were taken to be evidence of obscurity or error in the original theory.

The essence of the difficulty may be grasped by considering the case of just two status axes x_1 and x_2. The linear additive model purports to test for the presence of a status discrepancy effect by first of all computing the regression of a dependent variable y on x_1 and x_2,

$$\hat{y} = b_1 x_1 + b_2 x_2$$

and then adding a discrepancy term to the equation,

$$\hat{y} = b_1 x_1 + b_2 x_2 + b_3 (x_1 - x_2).$$

The problem is that the estimates $\hat{y}$ yielded by the second equation[7] are identical with those yielded by the first. The covariance matrix for the three terms x_1, x_2 and $(x_1 - x_2)$ is singular and hence no unique set of regression coefficients b_1, b_2 and b_3 is specified by the second model. We may employ any one of an infinite number of sets

[6] And also by others who did not advocate the linear additive model (Zelditch and Anderson, 1966).

[7] Note that this formulation differs from the usual one in that it focuses primarily on the identity of the estimated values of the dependent variable under the two equations, rather than on the indeterminateness of the estimates of the regression coefficients b_i in the second equation. This shift in emphasis was introduced in my previous paper (Hope, 1971).

of coefficients, all of which yield the same estimates as one another and the same estimates as the first equation.

When a model proposed as a test of a theoretical position runs into immediate mathematical difficulties, we may either question the coherence of the theory or impugn the claim of the model to represent the theory. It seems that no writer has taken the latter course. In fact, however, the model misrepresents the theory, and in strict logic sociologists were faced with a choice between, on the one hand, retaining the model and dropping the pretension to test for the presence of status inconsistency effects, and, on the other, adhering to the original theory and finding a better model for it. In fact they tried to ride both horses at once, persevering with the model and purporting to investigate status inconsistency theory.

In order to salvage what they could of the theory, they redefined status inconsistency, identifying it with the occurrence of interaction between status axes. While the original formulation had certainly allowed that some elements of interaction may represent status inconsistencies, it had not claimed that any and every interaction may be so identified. To put the point specifically, Lenski's non-linear definition of a status inconsistency effect is stated in terms of an interaction between his vertical and his non-vertical dimension. It takes the form of saying, for example, that, at a given level of the vertical dimension, political discontent is greater in those at the extremes of the non-vertical dimension than it is in those who lie at the middle. But the additive model defines status inconsistency as all and every interaction between the two status axes. It might be inferred therefore, that although it completely fails to test for the presence of signed status inconsistencies, the linear additive model is generous in its definition of other types of inconsistency, since it subsumes in its concept of interaction all the variance explicitly indicated by status inconsistency theory plus other variance as well. We shall see below, however, that the procedures actually employed to test for the presence of interaction are not equivalent to the simple linear additive model and that they are much more restrictive in their definition of interaction.

Thus the linear additive model fails completely to test for the presence of signed status discrepancies, and it tests for absolute inconsistency effects only to an uncertain degree and in an ambiguous sense which has never been explained. The model is erroneous in that it fails to represent status inconsistency theory while being confounded with a perfectly straightforward model which does represent it.

The starting and finishing points of the reformulation of the theory which is necessitated by a mistaken choice of model were succinctly stated by Blalock (1967) when he wrote (a) "In both the status inconsistency formulations of Lenski and Jackson, and discussions of the effects of social mobility, these strains are conceptualized as being produced by marked *differences* in hierarchical statuses or in rapid changes of status" (p. 791, my italics) and (b) "Duncan notes that the mobility thesis depends, for verification, on the existence of an interaction effect in exactly the same way that tests of status inconsistency have relied on showing an interaction effect. If one assumes that the main effects of father's and son's occupations are additive, then there is no need to introduce the 'strains-due-to-mobility' argument unless the *interaction* term is significant. Exactly the same applies to the inconsistency factor, as Lenski has noted." (p. 795; I have omitted some italics and inserted others.)

It is not reasonable to conclude without further ado that, because a mathematical model has run into immediate difficulties, the theory which it is intended to express must be defective. Direct conceptual analysis of Lenski's verbal statement of the theory shows that it is coherent. The fault, therefore, must lie in the model, and one need not seek far to find it. The crucial misrepresentation is contained in a statement by Blalock (and echoed by many users of the model since) to the effect that "an underlying thesis of all these approaches is that dependent variables may be influenced not only by the separate effects of several independent variables but . . . ." (p. 790). In fact Lenski did not, in his original paper, define inconsistency by contrast to the *separate* effects of the status variables. Rather he defined it as deviations from a vertical dimension which specifies the *shared* effects of the status variables. The logic of the geometric structure he describes is that, in controlling for the vertical dimension of status, *some* aspect of *each* of the status

axes should be controlled for, but not that *all* (linear) aspects of *all* axes should be controlled for. Thus a status discrepancy is present to the degree that the estimates obtained from the regression of y on the set of independent variables (each contributing its separate effect) fit the data better than estimates obtained by regressing y on the vertical dimension, however defined. Putting the matter at its simplest and considering only signed differences, we may say that the existence of a status inconsistency effect has been disproved if the equation

$$\hat{y} = b_1 (x_1 + x_2) + b_2 (x_1 - x_2)$$

yields the same estimates as

$$\hat{y} = b_1 (x_1 + x_2).$$

This is not to deny that the vertical dimension of status might be defined as a weighted sum of x_1 and x_2. The point is that neither a simple sum nor an *a priori* given weighted sum should be confused with an empirically derived sum of weighted x_1 and weighted x_2. The weights appropriate to the definition of the vertical dimension need not be the same as the weights which optimally predict the dependent variable. Insofar as they differ, a status inconsistency effect is present. The crucial step which invalidates the model as a bearer of the theory is the assumption that the parameters of χ_1 and χ_2 are to be separately estimated from the data.

The Distinction Between Construction and Application of Variables

At its root, the confusion engendered by the linear additive model stems from a failure to distinguish two quite separate activities of empirical inquiry, namely measurement of a characteristic or dimension and testing for its association with a dependent variable. The logic of status inconsistency theory demands that we first define status, so that we can define status inconsistency as orthogonal to it (or as an interaction between the vertical dimension and dimensions orthogonal to it), and then that we test whether the inconsistency dimensions or contrasts are related to the dependent variable. This two-stage nature of the model may be indicated by using parentheses to distinguish between two orders of weighting coefficient. In the two-axis case for the investigation of status discrepancy effects, we first find weights b_i such that

$$(b_1 x + b_2 x_2)$$

is a measure of status and

$$(b_3 x_1 - b_4 x_2)$$

is orthogonal to it. We then examine the coefficients c_i in the equation

$$\hat{y} = c_1 (b_1 x_1 + b_2 x_2) + c_2 (b_3 x_1 - b_4 x_2)$$

to see whether the difference term is contributing anything over and above the sum term which represents the vertical axis of general status. In the case of three or more status axes, more than one set of contrasts will, in general, be required if all linear deviations from the vertical axis (or interactions between the vertical axis and axes orthogonal to it) are to be explored.

Our first object, then, in testing for the presence of status inconsistency effects on a dependent variable y should be to ensure that we have a measure of status which is uncontaminated by our knowledge of how y is related to the status axes.[8] Such a measure may be arrived at by a variety of routes, which may be enumerated under the following heads (cf. Jackson and Curtis, 1968):

(1) A given set of weights is derived from *a priori* considerations. At its simplest, this might involve standardizing the axes and summing them, perhaps scaling them all to some common distribution before standardizing.
(2) The first principal component of the status axes is taken.
(3) Some criterion measure of status z is observed, and an equation is computed for the regression of z on the status axes x_i. It is of course desirable that errors and biases in the measurement of z should be independent of errors and biases in the measurement of the

[8] This point should not be confused with the suggestion that we should employ an independent measure of the intervening variable (e.g., psychological stress) by which inconsistency is supposed to exert its effects (Blalock, 1966a).

variable y which is to feature in our search for status inconsistency effects.

(4) A large number of dependent variables, perhaps including y, is measured and the first explanatory variate provides the weights for the x_i in the definition of status. Explanatory variate analysis was introduced by the author for the analysis of contingency tables but it was pointed out that it applies equally to the analysis of continuous variables (Hope, 1972b). The first explanatory variate is defined as that weighted linear combination of the x_i variables which predicts as much as possible of the variance of a set of variables y_j. It must be sharply distinguished from the first canonical variate which maximizes correlation rather than explained variance. Unlike a canonical variate, an explanatory variate does not remain unchanged under alteration of the relative variances of the variables y_j. Although including our dependent variable of primary interest y among the y_j of an explanatory variate analysis introduces an element of contamination between the definition of status and its application to the investigation of status inconsistency, this will not be a serious drawback if there are many variables on the right hand side of the analysis.

It is important to note that the explanatory analysis must, for present purposes, be abandoned after the extraction of the first variate because subsequent variates, although orthogonal to the first within the y-space, are not orthogonal to it within the x-space. Having employed the method to find that status axis which has maximum explanatory force over a whole range of dependent variables, the analyst may define the remaining, inconsistency, dimensions as principal components of the x-space after collapsing it across the status axis.

The logic of this operation is that we are not sure how to define overall status, but we wish to define it so as to minimize the chances of finding an inconsistency effect. If a dimension orthogonal to the first explanatory variate is significantly associated with variance in the y_j variables over and above the variance accounted for by the explanatory variate, then the presence of a status inconsistency effect has been established.[9]

As with all techniques of social measurement, the procedures enumerated suffer from a variety of defects, but they do not all suffer from the same defects, and we might be advised to employ more than one method. But the crucial point is that our method of synthesizing an axis should be independent of our analytical application of the resulting vector.

Status and status inconsistency are concepts which stand or fall together.[10] Either it makes sense to aggregate status axes by some means into an overall measure of general status, in which case dimensions orthogonal to general status (and some interactions between the vertical and the non-vertical) can be plausibly said to represent status inconsistency, or we must give up the attempt to investigate status inconsistency because it cannot be defined as a non-vertical dimension where no vertical dimension exists. It is a paradox of empirical research that investi-

[9] If confusion is to be avoided, it must be pointed out that the coefficient of multiple determination R^2 may vary from dependent variable to dependent variable even though only one explanatory variate exists; what is at issue is the *collinearity* of the projections of the dependent variables in the space of the status axes, not the strength of the relations.

If sociologists are willing to specify corresponding points on the various status axes but are unwilling to confine themselves to a linear model (perhaps because they are not confident that the axes are properly scaled), then the model of the halfway hypothesis (Hope, 1971; this generalizes very easily to three or more variables) can be employed on the left-hand side of the equations. If corresponding points on the axes cannot be identified with certainty, then the square-additive model may be employed. It should be noted that introducing the *design matrices* for these models into an explanatory variate analysis is not the same thing as imposing the models *in toto* and estimating the sums of squares associated with all their degrees of freedom. On the contrary, far from aggregating degrees of freedom, in explanatory variate analysis we are forcing the data to yield up one shared dimension which we propose to take as our best estimate of social status.

[10] This is a statement in conceptual analysis. It is not a claim that status effects and status inconsistency effects both exist.

gators have purported to test for the existence of status inconsistency effects while refusing to identify an overall dimension of status. If, in a particular analysis, we do not recognize the existence of such a dimension because we regard the axes as incommensurable, then we cannot meaningfully say that a person is consistent or inconsistent. As Lenski (1954:407) said:

> Having established the structure of these vertical hierarchies, the next problem was to establish common scales for all of them, so that the relative position of respondents in the several hierarchies might be compared. Without common scales, a measure of status crystallization would be impossible.

The converse of this point is that if we are in principle willing to make a judgment of relative status *tout court* for all pairs of persons, then we are committed to the existence of a general factor and have reserved for ourselves at least the logical possibility of making judgments of relative position on different axes.

Because it is possible to think of relations of distribution as being expressed in distances and directions in a continuous "stratification space" (Hope, 1972a) and because we often wish to think in terms of three or more stratification axes (in which case we can no longer comprise all additive deviations from consistency in a simple difference between variables), it is convenient to introduce a term which allows us to quantify extent and direction of inconsistency. I propose that we should employ the theory-neutral term *status polarity* to stand for perpendicular distance from the general status axis. Status consistency, or perfect crystallization, then becomes *zero status polarity*. Direction of deviation may be indicated by reference to the principal components of the residuals remaining after the extraction of the general status axis, however that has been defined.[11]

Although the present paper is mainly concerned with conceptual questions rather than with the question of empirical fit which has dominated much of the literature on the additive hypothesis, it is perhaps not out of place to point out that empirical investigators, by overlooking considerations of measurement theory, have in consequence failed to appreciate the fact that inconsistency effects will always be attenuated by measurement error to a greater degree than status effects. Hence the chances of demonstrating the presence of inconsistency effects are relatively low. When we add or average two equally-unreliable, standardized and positively correlated, status axes to construct a measure of status, the latter has a higher reliability than its two constituent axes. When we take the difference between the two axes as a measure of inconsistency, this has twice the error variance of the individual axes. Thus correlations with the dependent variable suffer greater attenuation in the horizontal than in the vertical dimension.[12] It is quite possible that some of the difference variables which have been investigated have reliabilities not appreciably greater than zero. The implication of these remarks is that a dependent variable which happened to correlate equally with status and with status-discrepancy, measured by means of perfectly reliable variables, would correlate more highly with the former than with the latter if the status axes were less than perfectly reliable. The difference between the two observed correlations would be exaggerated if partial regression coefficients were computed.

The Square-Additive Model

The upshot of the introduction of the linear additive model was the identification of

[11] Dr. D. R. Ploch has drawn my attention to his unpublished Ph.D. thesis (University of North Carolina, 1968) in which he employs spherical coordinates to distinguish types of inconsistency. Like Jackson and Curtis, whose paper appeared in the same year, he adopts Lenski's approach of defining status inconsistency as deviation from an overall status axis. All subsequent work seems to have adopted the additive model.

It is notable that Ploch and Jackson and Curtis use geometrical models just as I have done in introducing the concept of a "stratification space" (Hope, 1972a). It is therefore no accident that, working in complete ignorance of one another, Ploch has employed polar projection while I have proposed the term "status polarity." The difficulties of the additive model became really clear only when a geometrical interpretation of it is essayed.

[12] A further consequence is that selection of inconsistent (or mobile) people for special investigation is more fraught with regression artifacts than is selection of people in terms of status. It is probably necessary to take at least two determinations of each axis before estimating a difference.

status inconsistency with all and only the variance representing interaction among the status axes x_i. There are three observations to be made about this redefinition. The first is that the definition appears to extend the concept of inconsistency to include all deviations from the additive model and not only those explicitly specified by inconsistency theorists. Such an extension might, however, be defended. The second is that, as users of the model often point out, the definition fails entirely to cover an obvious and important type of status inconsistency, viz. signed deviations from the vertical dimension. The third is that the definition contains a fundamental ambiguity, in that it does not make clear whether inconsistency is identified positively with interaction variance or negatively with residual variance. In practice, as we show in this section, inquiries purporting to test for the presence of inconsistency have defined it narrowly as interaction rather than broadly as deviation from the linear additive model. Furthermore, it is shown that the model which is commonly employed deprives inconsistency of some of the interactive elements which rightly belong to it.

If our aim is to explore interactions among continuous axes, then a natural first step is to chop each axis into segments or levels in order to study multiplicative relations between segments belonging to different variables. Having segmentalized our variables in this way, there is in fact more than one way to partition the model, but as a matter of historical fact only one way has been employed. The basic additive model against which to test for the presence of interaction was set out by Duncan (1964; 1966; see also Treiman, 1966). It has been used in many subsequent investigations which will be found listed in recent papers (Vorwaller, 1970; Laumann and Segal, 1971; Jackson and Curtis, 1972; Jackman, 1972). Because occupational mobility may be regarded as inconsistency between status assessed at two points in time (and indeed as a special case of Jackson's (1962) category of inconsistency between an ascribed and an achieved status), the model has been applied in tests for the occurrence of mobility effects, as well as in tests of inconsistency theory. For reasons which will become apparent, I propose to refer to the model as the square-additive model.

Many of the research reports which employ the model begin by pointing out that inconsistency or mobility is confounded with the basic linear additive model, and they present the square-additive model as a route by which the analyst may escape from this impasse. It is also sometimes claimed that the square-additive model is more parsimonious than the status inconsistency model because it does not incorporate a redundant term (see, for example, Hodge and Siegel, 1970).[13] In my earlier paper on the topic (Hope, 1971), I presented a technical re-examination of fertility data reported by Berent (1952) which were reanalysed by Duncan (1966) to illustrate the square-additive model. The conceptual analysis of the present paper has its roots in the technical innovations of the earlier paper and it is therefore convenient to employ the same data for purpose of illustration.

Duncan's analysis took the form of a 4 x 4 non-orthogonal analysis of variance (also known as a multiple classification analysis) in which the cells of the design were formed by cross-classifying a man's origin status and his final status (each being categorized into four levels). The observation recorded in a cell is the mean number of children born to men in that cell (Fig. 1a).[14] Duncan used a model

[13] The argument of the section on the linear additive model has shown that the identification problem is a burden which users of the additive model have imposed upon themselves. Like Bishop Berkeley's contemporaries, "we have first raised a dust and then complain we cannot see." The argument from parsimony may be ignored insofar as it alludes only to redundancy introduced by the analyst himself. However parsimony or simplicity is an important criterion of choice between theories. Following Wrinch and Jeffreys (1921) and Popper (1959, section 42), I regard a function as simple insofar as it has few freely adjustable parameters, which means, in its present application, that the degree of simplicity of a model is given by its degrees of freedom. The two concepts of degrees of freedom and of rotation may be regarded as the technical principles guiding the theoretical analysis of this paper. They inform the maxim which was expressed in the earlier paper as follows, "Many of the difficulties which research workers find themselves in when handling difference concepts such as mobility or inconsistency would dissolve if they would learn to ask not 'are these two sets of equations identical?' but 'are the *spaces* mapped by the two sets of equations identical?'" (Hope, 1971:1028n).

[14] In a personal communication, Professor Duncan has emphasized the tenuousness of the theoretical link between the occupational position

(a) Conventional Mobility Table

Husband's Origin	Present Social Class I	II	III	IV	All
I	1.74	1.79	1.96	2.00	1.81
II	2.05	2.14	2.51	2.97	2.38
III	1.87	2.01	2.67	3.69	2.81
IV	2.40	3.20	3.22	3.68	3.44
All	1.88	2.17	2.73	3.56	2.77

(b) Mobility Diamond

								Means
				2.40				2.40
			3.20		1.87			2.60
		3.22		2.01		2.05		2.57
Mobility	3.68		2.67		2.14		1.74	2.73
		3.69		2.51		1.79		3.10
			2.97		1.96			2.71
				2.00				2.00
Means	3.68	3.50	2.75	2.25	2.09	1.91	1.74	2.77

Mean Level

Figure 1. Conventional Square Mobility Table Rotated to Form a Mobility Diamond. The Elements of the Table Are Mean Numbers of Live Births to Married Couples (Berent, 1952).

which is additive in the rows and columns of the table to test for the existence of mobility effects on fertility. In this, and in most of the many other analyses which have been carried out using the same model, the interactions have been either non-significant or negligible in degree, and so it has been concluded that mobility or status inconsistency (the interpretation varies according to the nature of the marginal variables) effects are either non-existent or unimportant.

The first point to be noted about this square-additive model is that it is not the linear additive model. In the two-axis case the linear model has two degrees of freedom, one for each axis. But the 4 x 4 square-additive model has six degrees of freedom, one for each of the three independent constants fitted to the rows, and one for each of the three independent constants fitted to the columns.[15] In the earlier paper, I defined linear effects for the table of Fig. 1a by means of the linear polynomial for four points: −3, −1, +1 and +3. It is an easy matter to show (using the techniques described in the section on exploring a model, see below) that the two linear polynomials in the rows and columns of the square table are wholly subsumed in the square-additive model. Since these two linear terms are equivalent to the linear additive model for two four-valued status variables, we see at once that the six degrees of freedom of the square-additive model incorporate the two degrees of freedom of the linear additive model. But we also see that the square-additive model is augmented by a further four degrees of freedom, which are, in fact, the quadratic and cubic terms for the rows and columns of the square. Thus the square-additive model is doing something over and above the mere fitting of the linear additive model.

Concentrating for a moment on the strict linear model, we find that this is an orthogonal rotation of the vertical and non-vertical dimensions in the table, where the vertical dimension is the sum of a man's two statuses and the non-vertical dimension is the (signed) difference. The two pairs of axes are completely equivalent to one another; each has two degrees of freedom and each yields the same estimates of cell means. Thus the square-additive model has the same property as the linear additive model: it cannot test for the presence of a mobility (status-discrepancy) effect because it incorporates such an effect within its own variance. It may or may not be defensible as a distinct theoretical position, but it cannot be a *test* of mobility or status inconsistency theory. It cannot even claim the virtue of parsimony because (in the present example) it fits six independent constants as against the two fitted by a simple linear mobility analysis.

The relations between the linear terms of the mobility or status inconsistency model and the linear terms of the square-additive model have the merit of being clear and comprehensive; the two pairs of terms are exactly equivalent to one another. The importance of this equivalence is two-fold: (a) the linear additive model incorporates the status-discrepancy term in its entirety, leaving no untidy remainder, and (b) deviations from the linear additive model are identical in all respects with deviations from the mobility or status inconsistency model.

When, however, we move on to the quadratic and cubic terms which take up the remaining four degrees of freedom of the square-additive model, the situation becomes much more obscure because these terms are partially confounded with both additions and interactions between the vertical and non-vertical dimensions of the inconsistency or mobility model, and the degree of confounding varies with the distribution of observations across the cells of the table. The nature and extent of these relations varies from table to table and may be teased out by the technique

and mobility of men on the one hand and the fertility of their wives on the other. However, the theoretical deficiencies of this particular application of the square-additive model are irrelevant to the conceptual analysis of the present paper. The reader is at liberty to rename the independent and dependent variables in Fig. 1 to achieve a more defensible analysis.

[15] It is worth noting that if we fit a constant to every row and column the model suffers from an identification problem in that the covariance matrix is singular (it has two zero roots, Hope, 1971:1030f) and so there is no unique set of coefficients for the eight axes of the model. This does not matter of course (indeed it is a universal feature of the analysis of variance) because we are interested, not in the regression coefficients, but in the estimates of cell means, and these are invariant under choice of one of the sets of coefficients which maximize the proportion of variance explained. Identification is a problem only when it is a sign that our conceptualization has fallen into confusion or that our techniques of empirical discrimination have failed.

described below in the section on exploring a model. We shall not pursue the analysis of the relations in detail here because users of the square-additive model have not provided a rationale for the inclusion of quadratic and cubic terms in the model. They did not, of course, conceive the model in terms of polynomials and so the need for an explanation of non-linear terms was not apparent to them. An obvious justification which they might have presented is that non-linear terms are required to compensate for irregularities in the scaling of the row and column axes. But this explanation does not get away from the facts, first, that the basic linear additive model has been complicated to improve its chances of fitting the data, and second, that the more complex model is wholly or partially confounded with important elements of the theoretical position which it is alleged to be testing. (The quadratic term is in fact correlated with a row term in the diamond model which is described in the following section.)[16]

When the six independent row and column terms (whether these are sets of polynomials or the more usual binary constants, which are entirely equivalent to the polynomials), have been fitted to a 4 x 4 table we are left with nine degrees of freedom which are lumped together and treated as a residual. The failure of users of the additive model to explain the nature of this residual variance might be regarded as the basic conceptual weakness of their claim to test for the presence of inconsistency or mobility effects, since we cannot know what they have disproved unless we are told the theoretical nature of the variance which they have found to be non-significant. If certain cells were to deviate sufficiently from the additive model to yield a significant interaction term, what name would we give to this effect, other than purely technical appellations such as "interaction" or "non-additivity?"

If a proponent of the square-additive model ventures to construct a conceptual interpretation of its interaction term, he may find it useful to note that each degree of freedom for interaction can be uniquely assigned (apart from the covariance introduced by the non-orthogonality of the design, i.e., the unequal numbers of observations in the cells) to the product of one of the three row polynomials and one of the three column polynomials.

The Diamond Model

In order to pursue the analysis further, we introduce here a substantive interpretation of a technical stage in the analysis of the previous paper. It was pointed out there that the basic conceptual axis of a table such as Fig. 1a is the diagonal containing the cells of non-mobile men. Taking the square table of the fertility analysis and rotating it through 135° to the position of Fig. 1b gives us a diamond-shaped table, and I propose to refer to the model whose structure is represented by that table as the *diamond-additive* model. In Fig. 1a the rows and columns of the table represent class of origin and destination class respectively. In the mobility diamond, the rows represent mobility (equivalent to status inconsistency if the two measures of status are simultaneous rather than successive) and the columns represent a mean of origin and destination class. The column effect in the diamond has more levels than there are classes because the model takes account of the fact that (if class-intervals are equal) a man who has crossed an odd number of class boundaries, let us say from I to II or from IV to I, must, in the absence of a mobility effect, be halfway between stayers in one class and stayers in the other. In an inconsistency diamond, a column effect is a defensible specification of Lenski's vertical dimension of status and row effects represent status inconsistency. (The precise interpretation of non-linear terms in the columns, i.e., higher-order polynomials, depends on the degree of our confidence in the scaling of the axes.) It is perhaps unfortunate that a vertical dimension should be represented by a column rather than a row effect, and analysts may prefer to

[16] In the earlier paper (Hope, 1971), I introduced a model called the halfway hypothesis which was designed to correspond more accurately than does the square-additive model to a feature of the verbal formulation of the latter, in that the estimated mean for movers between class *i* and class *j* lies exactly at the unweighted mean of the estimated mean of stayers in *i* and that of stayers in *j*. The halfway model differs from the square-additive model in that it is not perfectly confounded with a simple mobility effect (indeed I showed that such an effect is required to supplement the variance explained by the halfway model in Fig. 1a) but it shares most of the properties of the additive model and is open to the same objections.

reverse this situation. Such a reversal is not carried out here because mobility is obviously best represented in an up-down orientation and inconsistency is logically equivalent to mobility.

As with the linear model, the introduction of a mean or sum variable clears up some of the puzzles which beset analyses of difference variables (Duncan and Hodge, 1963; Hawkes, 1972). More important than that, however, is the conceptual clarification which it introduces into the discussion of status inconsistency and mobility. In the diamond model a status inconsistency effect is positively identifiable as an effect in the rows of the diamond. When the diamond model is brought into conjunction with Lenski's analysis it makes obvious sense to claim that differences among row means are a consequence or concomitant of status inconsistency or mobility. The row differences may take the form of a simple linear ordering, as in my earlier paper, or they may be a non-linear contrast between the extremely mobile on the one hand and the immobile on the other, that is they may be signed or unsigned mobility effects (cf. Blau, 1956); or they may be associated with some higher-ordered polynomial. The important consideration, however, is that it is possible to specify terms in the columns which have the effect of indicating where the mobile or inconsistent would have projected on to the dependent variable if they had been non-mobile or consistent. It is then a simple matter to test for the presence of inconsistency or mobility effects by examining the upshot of adding constants or polynomials for the rows to a model made up of constants or polynomials for the column effects. Unless we are prepared to entertain a subjunctive or counterfactual conditional proposition of this sort, we cannot conceptualize a mobility or inconsistency effect.

At first sight it may seem very odd to analyze a diamond, which looks like an enlarged square from which the corners have been removed, other than as the usual textbook square. But there is no intrinsic difficulty about carrying out the analysis of variance. A square with side k and degrees of freedom $k-1$ for each side rotates into a diamond with side $2k-1$ and degrees of freedom $2(k-1)$ for each side. The analysis may be performed by fitting binary constants to rows and columns or, equivalently, by fitting sets of polynomials (see appendix to Hope, 1971).[17]

As with the linear additive model, the dimension to be partialled out may be identified by specifying effects in a number of ways. General effects may be indicated by the use of polynomials. But if the crudity of the data is such that the analyst is willing to do no more than make pairwise contrasts between classes or levels of status axes (Simpson, 1973; Hope, 1973), then each such analysis may be made in diamond form. The 2 x 2 table for origin and destination class i and j rotates into a diamond with three rows and columns. Sets of constants may be simultaneously fitted to the columns of all the 3 x 3 diamonds derivable from a table such as that in Fig. 1 in order to test for the presence of row effects.

Four methods of partialling out the overall dimension may be distinguished, and these are listed here in ascending order of stringency and decreasing order of falsifiability. In describing the methods, terms appropriate to inconsistency analysis and terms appropriate to mobility analysis are used interchangeably.

(a) a linear ordering of the column means in the diamond, with one degree of freedom;

(b) a polynomial of order k, the rationale for which is that only k items of direct

[17] It has been objected that it is nonsense to talk of the degrees of freedom of the rows or columns of a diamond. In fact it is not nonsense. The mean of a row or column is the mean of the persons in that row or column, irrespective of the number of cells among which they are distributed. Any set of numbers may be conceived as lying in a space of a certain dimensionality, and it is nowadays a simple matter to compute the maximum possible dimensionality (if we cannot work it out analytically) as the rank of a covariance matrix calculated in the course of a design matrix regression analysis (see below). The dimensionality of a set of means is its degrees of freedom.

It is perhaps significant that Lenski stated his model in geometrical terms whereas subsequent writers have tended to work with algebra. This in itself need not have led to confusion because there is a complete logical equivalence between corresponding geometrical and algebraic formulations. However it is often very much more difficult to "see" the "structure" of a "model" presented in purely algebraic terms, whereas geometrical representation gives the analyst a clearer intuition of the general form of his model, an appreciation of the transformations to which it might be subjected, insight into its relations with other models and a grasp of its order of complexity.

information are available about the dimension to be partialled out, these being the means of the k groups of stayers;

(c) all $2(k-1)$ degrees of freedom for columns. These may incorporate some mobility effects but it may be claimed that this is inevitable because we have no empirical basis for estimating those effects: we have no theoretically-supplied criteria of interpolation which would enable us to decide where certain groups of movers ought to project on the overall dimension. It may be noted that the column constants necessarily yield a perfect fit to the means of consistent (immobile) people in the top and bottom class. Clearly no inconsistent person can project on to the general status axis at its extremes and so no comparison can be made between inconsistent people and consistent people at the extremes.

(d) the $\frac{1}{2}k(k-1)$ degrees of freedom for each pairing of cells i and j which partial out any tendency for the mean of movers c_{ij} plus c_{ji} to differ from the mean of the relevant stayers c_{ii} plus c_{jj}. In the design matrix c_{ij} and c_{ji} are allotted a +1, c_{ii} and c_{jj} are allotted a −1, and all other cells have a zero. If we at the same time fit the further $k-1$ degrees of freedom necessary to account perfectly for the means of the stayers, then the model simply tests whether the mean of the movers from i to j is the same as the mean of the movers from j to i, irrespective of where the movers lie in relation to the stayers. This is the weakest of the four models in that it makes no attempt to surmise where a group of movers would have lain on the dependent variable if they had not been mobile. Its explanatory power is limited because it contents itself with drawing very specific contrasts between groups of movers. The model does not really count as a test of the existence of mobility effects because it involves no counterfactual proposition which would indicate the non-mobile group or groups appropriate to the estimation of an effect for a particular mobile group.

Unless the data are very crude, a partialling of type (c) will probably satisfy our requirements in most analyses. In fitting $2(k-1)$ independent terms to the columns of a diamond, we are allowing for the possibility of finding both additive and interactive mobility or inconsistency effects in the diamond. The interaction term in the diamond model has an identifiable interpretation in that it reflects the presence of inconsistent inconsistency or mobility effects, that is the occurrence of different inconsistency or mobility effects at different levels of the general status dimension. An interaction term manifests itself in the diamond model only if $k \geqslant 4$. This is a consequence of the fact that, in fitting all $2(k-1)$ column constants, we are implicitly assuming that some mobile cells cannot be compared with any non-mobile cell.

Deficiencies of the Square-Additive Model

The diamond model not only serves to represent status inconsistency theory as it was originally formulated by Lenski, but its success in this respect helps us to see precisely where the square-additive model fails. The latter incorporates inconsistency (or mobility) effects in that the two linear terms of the diamond model (a linear ordering of the row means and another linear ordering of the column means) are perfectly and reciprocally related to the row and column linear terms of the square model – that is, the two submodels made up of just the linear orderings in their respective figures fit the data to exactly the same degree and yield exactly the same estimates. This explains why the square-additive model, which subsumes the linear term in the rows of the square and the linear term in the columns, cannot be used to test for a straightforward mobility effect, defined as a linear effect in the rows of the diamond. There are also other, more complicated and partial, relations between terms of the two models, which may be explored by the methods described in the following section.

It may be allowed that the square-additive model fails as a test of the existence of status inconsistency or mobility effects but it may nevertheless be claimed that it should be retained because it models some other defensible sociological theory. After all, it may be said, the model does work, in the sense that it displays an excellent fit to many different

dependent variables. There are two distinct points here and each must be dealt with on its own terms. The difficulty of assessing the force of the first claim is that no user of the model has ventured an explicit statement of either the theory which it is supposed to represent or the theory which deviations from it would represent. It is thus not clear which of its several properties are essential, and which are adventitious, to its interpretation. I have elsewhere alluded to the difficulty of interpreting the "effects" which the model fits to the rows and columns of a square mobility table such as Fig. 1a (Hope, 1971). There is something intrinsically odd about saying, for example, that the effect of origin class I may be computed from all those who started in that class, whether they stayed in it or dropped out of it, while the effect of origin class IV may be computed from all those who started in IV, whether they stayed in it or rose out of it. Part of the difficulty resides in the systematically different weightings allotted to the destination classes because of the non-orthogonality of the typical "design." But there is the further consideration that, as Sorokin (1959:509f) said, "If we want to know the characteristic attitudes of a farmer, we do not go to a man who has been a farmer for a few months, we go to one who is a farmer for life." If, as Durkheim (1970:317) alleged, it is a profound mistake to confuse the collective type of a society with the average type of its individual members, how much more mistaken is it to confuse the collective type of a class with the average type of all those who have at some time been members of that class? Since the analysis of variance has its paradigmatic application in the area of biological experiment, it would be a salutary exercise for a proponent of the square-additive model to try to describe a biometric experimental design which incorporated all the essential structural characteristics of the usual sociological applications of the model. It may be suspected that the experiment would be disowned by biologists.

Turning to the second defence of the model, namely its empirical omnicompetence, we may note that this has two aspects, one conceptual and the other an artifact of the typical distribution of cell numbers in a table such as that in Fig. 1. A biometrical analogy may suffice to illustrate the conceptual problem inherent in the interpretation of the model. When a biometrician studies consequences or concomitants of weight gain, he typically partials out initial weight and analyses the residual deviations from the regression line. With straight regression lines and homoscedasticity this means that "At each level of initial weight, the 'gainers' and 'losers' should be defined, so that the proportion of gainers at that level is equal to the proportion of losers at that level, and also equal to the proportion of gainers and losers at every other level .... When extreme groups are chosen in this way, then the embarrassing relationship between gain and initial weight disappears; in this method we are investigating the relation of other variables to gain with initial weight effectively partialled out – in other words held constant statistically." (Lord, 1962). The square-additive model is equivalent to partialling out *both* initial weight and final weight *and also* their non-linear as well as their linear components. When data are thus ground between an upper and a nether millstone, it is not surprising if little but chaff remains. Partialling out a (linear) column effect in a diamond is equivalent to partialling out the mean of initial and final weight, such as one might do if one were investigating natural, rather than experimentally-induced growth.

The second characteristic of the square-additive model which helps to explain its excellent empirical performance is that, as well as incorporating several sources of variance, it also contrives to subsume those aspects of the sources which relate to the most populous cells of the typical table, i.e., the cells on and near the principal diagonal. This may be illustrated by a comparison between two analyses which employ as "data" only the design matrices (a) of the square – additive model and (b) of the complete model which accounts for all k^2-1 degrees of freedom in the square. In the first analysis, (b) is predicted from (a) on the assumption that each cell contains the same number m observations, and the proportion of the variance of (b) which is predicted by (a) is given by the ratio of their degrees of freedom. Thus in a 4 x 4 table (a) has d.f. 6 and (b) has d.f. 15 so the proportion of variance predicted is 0.4. In the second analysis, the cell sizes are replaced by typical values n_{ij} which tend to crowd round the principal diagonal. When a comparative analysis of this sort is carried out using the cell sizes of the fertility analysis of

Fig. 1 (given in Duncan, 1966 and Hope, 1971), the percentage of total variance explained by the constants of the square-additive model increases from 40% to 53% with the introduction of weighting. I refer to an analysis of this sort as a *design matrix regression* analysis and I use it to distinguish between the *ostensible* degrees of freedom (6 out of 15 in this case) and the *effective* degrees of freedom (8.1 out of 15). Computation of the effective degrees of freedom involves weighting the R^2 for a column of the design matrix (b) by the non-standardized variance of that column. In carrying out a design matrix regression analysis, allowance must of course be made for the total loss of one or more dimensions owing to redundancy among the columns.

Although the discrepancy between ostensible and effective degrees of freedom plays a part in the empirical success of the square-additive model, it is not a sufficient condition of that success. When constants are fitted to the columns of the diamond in order to account for linear and non-linear status effects, the ostensible degrees of freedom are 6 out of 15 and the effective degrees of freedom are 8.7. The difference between the two models does not therefore reside in the extent to which they account for populous cells (the status model necessarily effects a good fit to diagonal and near-diagonal cells), but in the nature of the fit achieved.

Exploring a Model by Means of Design Matrix Regression Analysis

Schopenhauer remarked that "the solution of a problem often first looks like a paradox and later like a truism," and the critical aspects of this paper may well suffer that fate. Accordingly, we introduce now some positive recommendations for the guidance of workers in this and other fields. And we begin by asking what, in general, a model is for, what sort of task it is supposed to perform and how we may know that it is performing it properly. The grounds of these recommendations are to be found in a remark in Kant's *Critique of Judgement* (paragraph 68) to the effect that we have complete insight only into that which we have produced ourselves. It is because a model is an artifact whose principles of production and operation can be explicitly formulated that models are useful in natural science and social studies.

It is however a mistake to suppose that because the structure of a model is knowable in principle that we do in fact know it. There is more to understanding a model than seeing whether it fits. In order to gain a real acquaintance with it and in order to reap the benefits of having constructed it, we must play with it, seeing how it behaves in differing, often counterfactual circumstances.[18] If two or more models have been constructed, then it becomes important to know how they are related. In comparing the fit of different models to the data, we must be aware of the possibility that one model is more elaborate than another or that one subsumes some or all of the features of the other.

Considered from this point of view, the square-additive model possesses the desirable features of a profitable model in that it is perfectly precise, its properties are explicable without reference to its author's opinions and it is therefore corrigible. As so often happens, a meaningful debate is possible because the critic shares the basic tenets of the position he is criticizing.[19] The debate is conceptually fruitful because the proposed model has properties which become apparent only upon close

[18] The essential role of counterfactual conditionals in historical and sociological explanation is a central theme in a monograph on social mobility on which the author is now engaged.

[19] This position is totally opposed to the psychologism of critics such as Doreian and Stockman (1969). By all means let us construct means for obtaining culturally-enshrined ostensive definitions of inconsistency, whether these are normative or attributive, consensual or dissensual, conscious or unconscious (cf. the techniques employed for the investigation of shared meanings in Appendix A of Goldthorpe and Hope, 1974). But let us not employ these measures as intervening variables until we have first established that distributional inconsistency has some effects which call for explanation. Reductionism is frequently a barren creed because the reductionist, in his eagerness to degrade the higher-level analysis, deprives it of concepts and generalizations to such a degree that it is impossible to establish those isomorphisms between the two levels which would constitute a reduction. My experience as a clinical psychologist leads me to doubt whether any single psychological reaction will be found to mediate between status inconsistency and even one of its effects, if it has any. A certain school of sociology calls persistently for neglect of the structural study of attributes in favour of the investigation of perceptions and sentiments. This represents a sustained attempt to stand dialectics on its Hegelian head again.

examination, and yet these properties cannot be disputed because they are analytically present. The square-additive model has been faulted on the grounds that it is a conceptual *omnium gatherum*, but it is only because the model is formally precise that it becomes possible to argue this case by the use of the analytical techniques which will now be described.

In footnote 10 of my earlier paper, I invited data analysts to ask themselves whether the *spaces* mapped by two sets of equations are identical. The easy way to answer this question is to perform a canonical analysis on the two design matrices. If all possible canonical correlations are perfect, then one set must lie wholly within the space of the other. The technique may be extended to the case of partially overlapping design matrices by computing the proportion of the variance of each column of a design matrix which is predictable from the other design matrix. (This calculation is also necessary in the case of perfect canonical correlations since only by means of it is it possible to establish the strict equivalence of two design matrices.)

The present paper may be regarded as an invitation to sociologists to begin an analysis by ignoring their dependent variable and addressing themselves to an investigation of the formal properties of different functions of the independent variables (models). When offering one model as a means for testing another, it is desirable to show, by analysis of design matrices, that the two models are formally independent, or at least that any overlap between them may rightly be attributed to the former rather than to the latter. At the same time discrepancies between ostensible and effective degrees of freedom may be examined. Although it seems paradoxical to speak of a fraction of a dimension, there is no intrinsic oddity about a non-integral set of degrees of freedom. The ostensible number of degrees of freedom of a model is the number of dimensions into which it projects, and its effective degrees of freedom are the sum of dimensions with each dimension weighted by its variance.

It is not enough to write down a set of equations with perhaps five or ten degrees of freedom and to leave the reader to intuit both the internal properties of the model and the relations which it has to other possible models. There is no point in supplying a model if neither the writer nor the reader is aware of its formal properties. In determining its degree of fit to the dependent variable, we need to know what it is that is fitting, or failing to fit, the data. Furthermore, if the model purports to test a theory we need to be told the meaning of the terms which have not been included in the model.

I refer to this recommended task of formal analysis as a process of *exploring the model(s)*. The obvious technique to employ in accomplishing the task is that of *design matrix regression analysis. Venn diagrams* might be used to illustrate relations between models in simple cases. My preliminary work in this field suggests that it is useful to carry out the exploration both with observed frequencies in the cells and with equal frequencies. No doubt in some cases, particularly where comparative analyses are being performed, it will be desirable to employ other hypothetical patterns of frequencies. It should be noted that if sets of orthogonal polynomials are employed to specify terms in a dimension, pairs of polynomials will often be correlated even when cell frequencies are all equal, because the number of cells projecting on to each polynomial is different at different points along its length.

A design matrix regression analysis of the extent to which (a) the square-additive model overlaps with (c) the six polynomials defining all possible mobility effects (row effects in the diamond—see the table on page 1031 of Hope, 1971) illustrates the logical problem inherent in the proposition that the square-additive model may be used to test for the presence of mobility effects. Using observed frequencies in the cells, we find that 21% of the variance of (c) is accounted for by (a). This may not seem a very serious degree of overlap until we look at the distribution of explained variance among the dimensions or axes specifying mobility effects. 100% of the variance of the linear polynomial and 76% of the variance of the cubic polynomial are accounted for by the square-additive model. These two axes, which

number of ranks. All that is required is the identification of homologous points in the distribution. This may be accomplished by simple techniques such as standardization of variances (cf. Goldthorpe and Hope, 1974: Appendix A) or, if the data are adequate, by more powerful scaling procedures.

correlate 0.87 with one another, taken together are practically equivalent to a simple difference between the upwardly mobile and the downwardly mobile. It is just these two terms which account for the bulk of the variance of the dependent variable over and above that accounted for by the model which I called the halfway hypothesis (Hope, 1971). Hence, in this particular analysis, the argument for or against the existence of mobility effects turns on our interpretation of the difference between the halfway hypothesis and the square-additive hypothesis. The former ascribes one and the same effect to class *i* considered as origin and to class *i* considered as destination, whereas the latter allows *i* to have different effects according to whether it is an origin or a destination class. The halfway hypothesis may be regarded as a general factor of father-son status which is defined *a priori* by specifying that comparable points on the two axes shall have equal weights. It has three degrees of freedom because it is a non-linear rather than a linear general factor.

A set of equations is nothing in itself, even if it fits the data. In order to schematise it as something deserving the name model—that is, as a man-made analogue of social reality which we can comprehend because we know the principles of its production—we need to have insight into its properties. In the earlier paper, I argued that one means of gaining insight is to examine and compare the estimates of the dependent variable which it yields for different categories of person. The present paper has indicated how a model may be formally explored by discrete analysis of its distinct axes and by relating it to the axes of other models. The two approaches might be combined by partialling one model out of another and testing the fit of the elements which are unique to the latter.

Reconstruction of the Additive Model

Perhaps by analogy with Voltaire's famous argument "the French admiral was as far from the English admiral as the latter was from the former" (so why not hang both?), it has been objected that the confounding between the diamond model and the square-additive model is an impartial criticism of both their respective theories. This however is to misunderstand the nature of explanation and the relation between theory and empirical testing. It is by no means uncommon in the physical sciences to have two theories which are equally compatible with a wide array of phenomena because they make identical predictions in those areas. Choice between the theories then rests partly on considerations of conceptual clarity, simplicity and elegance, partly on grounds of consonance with accepted general principles of the science and partly on the search for phenomena which support one theory at the expense of the other. In sociology the appeal to general principles is indistinguishable from dogmatism and may be ruled out. The *experimentum crucis* cannot be as clear-cut in its results as one would like because relative degree of fit is often a shifting and equivocal criterion of discrimination between models, especially when much of the variance lies within cells. I have explained why clarity and simplicity appear to lie with the diamond model rather than with the square model. However, it would be desirable to have a conceptually defensible alternative to the theory represented by the diamond model which embodied the strong points of the square model. The elaboration of such an alternative will demonstrate at once the formal corrigibility and the conceptual deficiencies of the square model. It will, of course, still be a rival to the diamond model, not a test of it (the column effects in a diamond are the best possible test of the row and interaction terms which represent inconsistency and mobility effects) because there will be considerable overlap in the variance of the dependent variable explained by the two models.

The square-additive model may be criticized (a) because it has high effective dimensionality leading to empirical overkill even though the formal structure has no clear sociological interpretation and (b) because it does not adequately specify class effects. The appropriate cure for the former defect is, in Popper's (1959: section 40) terms a "formal reduction," i.e., simplification leading to greater conceptual clarity and a higher degree of falsifiability, while the second defect calls for a "material reduction," i.e., a precise identification of the bearers of class effects.

The strong point of the square model is its suggestion that movers lie halfway between those who best exemplify the effect of their origin class or aggregate and those who best

exemplify the effect of their destination class. (The proposition may be restated in terms of consistent and inconsistent aggregates. In this case, one must be careful not to imply that consistent aggregates form classes or that all classes are status consistent.) In order to advance this theory to the status of a worthy opponent of inconsistency theory, we must incorporate the latter's two-stage property, namely the distinction which it draws between the specification of a function and its empirical application. If we are to construct a theory based on class effects, this implies that we should first identify the characteristic bearers of those effects and then test the deviations of movers from the estimated halfway points. In the case of many dependent variables this means computing discriminant functions for the classes without reference to movers and then examining the deviations of the movers from the halfway positions in the discriminant space.

There is however one weakness of the square model which such a proposal would not be able to remedy, in that it does not offer a general explanation of what deviations from the model would signify. Finding evidence for an inconsistency or mobility effect, which is a function which spans several cells of the table, gives us guidance on how we should set about explaining it (by looking for characteristics which vary with strength or direction of mobility or inconsistency). Finding evidence of significant deviations from the class effects model, on the other hand, leaves us at a loose end.

An analogy may make this point clear. In the nature-nurture controversy, a strong theory—Mendelian theory—is pitted against a weak one—environmentalism. The former predicts precise values for observable statistics such as intrasibship correlation. The latter is compatible with the values predicted by Mendelian theory and also with a wide range of other values. Similarly, the idea that a person combines the influences of his class of origin with those of his destination class has little explanatory power because it is compatible with a wide range of outcomes, including significant deviations from a halfway model. There is nothing intrinsic to the basic idea of combining origin and destination effects which restricts the results to the halfway mark.

One way of making the thoery less vacuous would be to specify mechanisms which determine the relative strength of origin and destination classes. Another would be to distinguish between dependent variables which are more influenced by one class and variables which are more influenced by another. The former suggestion would give the theory an advantage over inconsistency theory in that it would enable it to explain within-cell variance by taking account of differing strengths of the mechanisms in individual cases.

It is arguable that the whole apparatus of the additive model misses the point of the basic theoretical position which it tries to formalise, namely that mobile people combine or average the characteristics of people in their class of origin and the characteristics of people in the class in which they find themselves. Conformity with the model is quite consistent with a situation in which no such combining or averaging occurs. If, for example, roughly half the people in cell c_{ij} retain the level of the dependent variable which they would have had if they had stayed in i and if the other half switch over completely to the value which they would have had if they had always been in j, then a mobility effect manifests itself, not as a mean deviation from the additive model, but by inflation of the within-cell variance of c_{ij}. Since 90% of the variance often lies within cells, any model which can explain an appreciable proportion of this variance has a head start over models which do not make the attempt. To forfend an error which only too readily insinuates itself into discussions of summed error distributions it should be pointed out that inflation of within-cell variance will typically not result in bimodality of within-cell distributions. It can be shown that bimodality manifests itself only if the distance between the means of two summed distributions exceeds two standard deviations (Hope, 1969). This is mentioned here because Berger and Fisek (1970) have proposed to use bimodality as a criterion in an experimental investigation of status inconsistency.

Do Status Inconsistency Effects Occur?

The inability of the linear additive model to test for the presence of simple status-difference effects is well-documented. This paper has demonstrated that the linear-additive

model is subsumed in the square-additive model which therefore has the same incapacity. The paper has also shown that there are other, more occult, sources of confounding between the square-additive model and the theory it purports to test. And it has been pointed out that, from the fact that status inconsistency can be defined as interaction between certain axes, we are not justified in inferring that it is correctly represented by interactions between very different axes. It has also been pointed out that measurement theory indicates that the dice are loaded against the detection of difference effects, as contrasted with sum effects.

No argument has yet been advanced for the actual occurrence of status inconsistency or mobility effects. Some evidence with this import was presented in the earlier paper (Hope, 1971). However, it is of interest to note that the literature on the square-additive model contains an empirical analysis which, if it is interpreted as a test of correctly-formulated inconsistency theory, confirms the existence of inconsistency effects. Hodge (1970) computed severally the regressions of a number of dependent variables on one and the same set of status variables and he claimed to show that the regressions differ not only in strength (measured in terms of R^2), but also in direction, that is in the relative weights allotted to the status axes. As the discussion above shows, differences of the latter kind demonstrate that no single dimension of status can be responsible for predicting position on all the dependent variables, and so it must be the case that at least one dimension orthogonal to a status dimension, however the latter is defined, is at work. Hence status inconsistency effects exist, though the analysis does not tell us how important they are.

Hodge's own interpretation of the analysis is that it shows that there cannot be such a thing as a single overall dimension of status. But this is to beg the question, since the interpretation can be sustained only if it is assumed that an overall status dimension, if it exists, must necessarily be the only source of explanatory variance. In other words, the interpretation assumes that status inconsistency effects cannot occur. This criticism is a mirror image of that which has been advanced against employment of the additive model to test for the presence of status inconsistency effects. There is a reciprocal relation between general status and status inconsistency in that any endeavour to find evidence for (or against) either one must involve measurement of the other. It is open to us to deny the usefulness or meaningfulness of both concepts, but we cannot supply evidence against either one without admitting the meaningfulness of both.

The dispute between the additive hypothesis and status inconsistency theory resembles that between general factor and group-factor theory in the study of intelligence, in that protagonist and antagonist are not always aware of the distinction between theory and model or of the distinction between empirical fit and conceptual interpretation. For example, Spearman and Garnett claimed to interpret a table of correlations as proof of Spearman's two-factor theory; Thomson (1920) pointed out that his sampling theory is equally consistent with the observed correlations and that his opponents were in the position of a man who argues that a quadrilateral must be a square because its angles sum to 360° (cf. Spearman, 1927, Appendix I.5). Garnett's (1920) claim that the two-factor theory is simpler than Thomson's no longer rings true because the latter is clearly more consonant with polygenic theories of inheritance. Nevertheless for practical purposes, we do in fact find it simpler to think in terms of a single general factor. The analogy has further relevance because it can be shown (Hope, 1972c; 1974; 1975) that a regression equation becomes immediately interpretable if it can be identified with a principal component of the set of variables to which it applies.

Status and status inconsistency, like general and bipolar factors, are concepts which sink or swim together. The sociologist may decide to investigate them jointly – let us say in the study of prestige in its strict sense of deference-entitlement – or to ignore them both – let us say in the study of variables such as ethnicity and education – but he cannot identify effects of inconsistency without first looking at effects of status. Similarly, it is not possible to conceptualise mobility effects except by formulating a counterfactual proposition specifying where a particular group of mobile people would have lain on the dependent variable if they had not been mobile.

The argument of this paper exemplifies the general proposition that an analytical concept can be deployed only in a theoretical medium, since only thus is it possible to specify the value of Y corresponding to a value of X, other things being kept constant. We are on fairly strong ground in the theoretical analysis of status inconsistency or mobility because here it is possible to employ observed values of Y for the consistent (non-mobile) in estimating what the values of Y for the inconsistent (mobile) would be if no inconsistency (mobility) effect were present. But even in this case an explicitly analytical concept (of general status) is required to direct our choice of comparison group appropriate to a particular category of inconsistency or mobility.

Sociologists who take fright at the prospect of venturing beyond the confines set by observed, descriptive, variables should, logically, forswear the use of many of the commonest concepts of their discipline, because those concepts are intrinsically analytic. In fact the typical response of sociologists who have accepted the argument of this paper has been to eschew a concept of general status. If this response were to generalize to all areas of enquiry, the sociologist would be left with descriptive concepts only and would be deprived of any pretension to explain, since explanation requires the deployment of analytic concepts. Such concepts are essential to the specification of the *ceteris paribus* clause of a counterfactual conditional proposition.

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STATUS INEQUALITY AND STRESS IN MARRIAGE

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Emotional stresses that are experienced in marriage are traced to differences in spouses' status origins. Linking status differences to such stress are a number of intervening conditions. People to whom status advancement is important and who have married mates of lower status are apt to have a sense of loss that leads, in turn, to a disruption of reciprocity, expressiveness, affection and value sharing in marital exchange. Such disruptions then act as immediate antecedents to emotional stress. It is through this process that the status order of the larger society can reach out to have a deleterious influence on the emotional states arising out of marital transactions.

This paper examines the association between differences in the status backgrounds of husbands and wives and the stresses they experience in marriage. To view social status as a force underlying marital stress is contrary to the assumption that modern marriage is not penetrated by the hierarchical arrangements of the larger society. According to this assumption, status is unitary for the nuclear family (Barber, 1957) and any status differences that existed initially in marriage give way to a common status determined by the occupation of the husband. However, even if society does not differentially accord status to husbands and wives – an assumption that should not necessarily be taken for granted (Acker, 1973) – status distinctions within marriage may still be recognized by the spouses themselves. When such distinctions are made in marriage, they will have an effect on the shaping of the relationship.

Indeed, this is suggested by prior research into marriage, some of it done several decades ago. The work of Burgess and his colleagues (Burgess and Cottrell, 1939; Burgess and Wallin, 1943) in particular indicated that the status characteristics of people are prominent factors both in mate selection and in ultimate satisfaction with marriage. More recent findings generally buttress those of earlier investigations. They show, for example, that there is less marital happiness and stability (Roth and Peck, 1951; Hicks and Platt, 1970) and less value consensus between spouses (Pearlin, 1971) in heterogamous than in homogamous marriages. In his study of divorced women, furthermore, Goode (1956: 97-103) found that there was a substantially greater proportion of this group who had been in heterogamous unions than exists in the married population.

Not all investigators agree that status inequality has an appreciable influence on marital relations. Prompted by what they consider to be unwarranted counselling against marriages between people of different class backgrounds, Glenn et al. (1974) have warned that reliable knowledge about heterogamy and its effects is far more limited than is recognized. They note, first of all, that there is less empirical substantiation of the ill effects of status heterogamy than one would expect from the emphasis given the subject in marriage texts. Secondly, they see sufficient conceptual shortcomings and enough methodological flaws to question whether there are any ill effects at all.

Viewed within the context of past work and present disputes, the findings of this study certainly support the position that heterogamy can have a disruptive impact on marriage. Nevertheless, they equally support Glenn et al. that too little is known about the phenomenon to warrant any suggestion that if one wants to avoid marital problems he might begin by avoiding heterogamous marriage. In fact, one of the first things that will be seen in this analysis is that status heterogamy by itself

is a trivial feature of marriage. Status inequalities become an important force in marital relations only when they are combined with certain status values.

A principal stance of this paper is that an understanding of what goes on in marriage is best achieved by looking at it within the context of the larger social order. Marital stress is a focus that is strategically well-suited to this position, for it provides an excellent vantage point for observing how arrangements in the larger social order merge with processes within marriage in shaping the inner lives of individuals. Although a perspective that intra-familial events are bound to conditions outside the family may seem somewhat self-evident, it is one that has been generally absent from the large body of research that regards family problems as an expression of individual psychopathology (Croog, 1970). This study, by contrast, largely ignores personality factors, emphasizing instead some of the bonds existing between stratified social organization, the structure of marital transactions and the patterns of individual stresses within marriage.

STRESS: SPECIFICATION AND MEASUREMENT

Stress is a concept claimed by many disciplines and this has contributed to the accumulation of multiple layers of meaning. While its popularity bespeaks the heuristic value of the concept, it has also led to what Scott and Howard (1970) describe as a lack of conceptual integration. Even with its problems and ambiguities, the concept of stress remains a useful one. Its major utility is in pointing the way to aspects of social structure whose impact on the lives of people would otherwise go unnoticed. Stress both reveals to us something about the psychological interior of people and sensitizes us to those aspects of their social environments that they find problematic. Much can be learned about social circumstances from the quality of responses to them, and this is a principal reason why stress is a potentially rewarding concept for sociology.

Consistent with general usage, stress is being viewed in this investigation as a condition of the organism, distinct from strains and stressors external to the individual. But, when it comes to defining precisely what this organismic condition is, general usage disappears. This is exactly as it should be, for stress is in fact manifested in many ways and at many levels of psychological, physiological and biochemical functioning. Consequently, there is not a single "correct" indicator, but potentially many of them, each pitched at a different level of analysis and each having its own problems of validity and reliability (McGrath, 1970:41-96). The selection and use of indicators, therefore, are dictated more by the substantive disciplinary interests and methodological constraints than by any intrinsic superiority of one over the others.

This study, being a large-scale household survey of a representative sample, could not feasibly employ indicators whose measurement depends on biological or chemical assays, even if this were desirable. Furthermore, since the object of the investigation is to relate stress to pervasive and enduring social circumstances, it is necessary to develop indicators that reflect chronic – albeit low-keyed – stress rather than ephemeral eruptions of stress in which the organism is in extreme imbalance. The measures employed by the study – which are based on respondents' reports of their feelings of distress in such areas of their lives as job, marriage, parenthood and economic livelihood – are consistent with the needs and goals of the present research. The actual measures are described in a later section.

There is one respect in which the conceptualization of stress here differs from that found even in other sociological investigations. Stress is treated in this study as a specific response to specific circumstances rather than as a general condition of personality or a fixed characteristic of the individual that cross-cuts the situations in which he finds himself. As a specific condition, it is viewed as something aroused under particular circumstances in particular situations segregated in time and space from other situations. The few survey studies of social stress tend to treat stress as a general phenomenon and have used global measures of anxiety, depression or other psychological impairments to indicate the presence of stress (Langer and Michael, 1963; Dohrenwend and Dohrenwend, 1969:61-76). Here, however, it is viewed as a condition specific to a given set of circumstances and not as an over-arching pattern of psychological malfunctioning.

The present study, then, regards stress as

(1) involving emotional disturbance; (2) being a response to the specific circumstances of specific role areas rather than a generalized state manifested in all roles and (3) being aroused not only by crises but also by continuous circumstances woven into daily life, which is consistent with the kinds of antecedents that will be emphasized here.

BACKGROUND AND METHODS

The data reported in this paper are a part of a larger inquiry into the social origins of personal stresses. Most of the inquiry is organized around the major social role areas of economic maintenance, occupation, child rearing and marriage, including as well an examination of some of the stresses arising among the single, the retired, full-time homemakers and employed women who are wives and/or mothers. The interview schedule used to collect the data provides several types of information: first, an inventory of some of the potential strains in the different institutional areas that earlier exploratory interviews had revealed to be commonly experienced; next, an overview of the coping devices and resources people employ in dealing with these strains and third, measures of the stresses that individuals feel. The present report is concerned only with the area of marriage.

The sample is drawn from the urbanized area of Chicago (U. S. Bureau of the Census, 1972), which includes sections of Northwestern Indiana as well as some of the suburban areas of Chicago. A cluster sample of 2300 respondents was interviewed, each cluster consisting of four households per block.[1]

STATUS RELATIONS AND STRESS

The analysis of status relations between spouses will primarily rely on their relative status origins, and this is determined by comparing the occupational statuses of their respective fathers prior to the marriage. The same comparison has on occasion been employed as an indicator of mobility of women through marriage (Rubin, 1968; Elder, 1969). Comparisons of spouses' fathers for this purpose has come under criticism on the grounds that a more meaningful measure of women's mobility is the comparison of their husbands' current status with their fathers' standing at the time of marriage (Scott, 1969; Martin, 1970). The criticism is based, of course, on the assumption that the status of a woman is tied to her husband's career achievements, not to his origins. But for two reasons this debate is beside the point of the present analysis. First, the comparison of the relative status backgrounds of couples here is not to identify the occurrence of mobility, but to observe the effect of status differences on aspects of marital relations that have the potential to evoke stress. Second, this analysis is interested in the effects of hypergamy and hypogamy on men no less than on women, an interest best served by comparing spouses' backgrounds. In this connection, it will be seen that the status a wife initially brings to marriage is likely to be as important to her husband as his is to her. The comparison of spouses' status backgrounds is being employed, then, not because this is the only way

[1] Since the total sample was 2300, the interviewing was done in 575 blocks, one-fourth the number of interviews. The 1970 census reports that there are 2,137,185 households in the Chicago urbanized area (U. S. Bureau of the Census, 1972) and when this total is divided by the total number of blocks in which households are to be chosen (575), the result, 3716, is the skip-factor for the selection of households; that is, the block in which every 3716th household is located was selected in this fashion. The additional three households in the cluster were chosen by simply dividing the total number of households on each block by four and using the result as the factor for counting from the initially selected address. The households selected in this fashion were pre-listed, thus separating the task of household selection from the interviewing. Two criteria were used in selecting respondents within sampled households. First, anyone younger than 18 or older than 65 did not qualify, for it was desired to have a sample weighted in favor of those still actively engaged in occupational life. As it was, some of the subjects were already retired and, with foreknowledge of this contingency, questions were developed to probe some of the stresses attendant upon retirement. Where more than one person in a household qualified as a respondent under the age limitations, the older candidate was systematically chosen. A second restriction concerned respondents' sex. The sex of the person to be interviewed in each household was predesignated so that the final sample would have as equal a number of males and females as possible. This restriction could be ignored only in instances where all age-qualified respondents were of the same sex. Because females typically head such households, the final sample contained more women than men. This report deals only with those who are currently married, a total of 721 men and 870 women.

to identify status differences, but because it is best suited to the goals of the analysis.

Respondents were questioned about the occupations of both their fathers and fathers-in-law and each was grouped into seven ranks: (1) professional, higher executives, proprietors of large concerns; (2) business managers in large concerns, proprietors of medium business and lesser professionals; (3) administrative personnel, small independent business proprietors and minor professionals; (4) clerical and sales workers, technicians and owners of limited, one-man businesses; (5) foremen and skilled manual employees; (6) machine operators and semi-skilled employees and (7) unskilled employees. People coming from farm families are not included in this particular occupational array because of the difficulty in judging the status of such families. The relative status origins of spouses are ascertained simply by observing whether respondents married a person whose father was lower, higher or equal to their own father's status.

The first question to be asked of the data is also the simplest and most direct: is there anything about status inequality that might result in stress? In keeping with the conceptualization of stress as a specific reaction to specific circumstances, several measures of stress were provided for in the interviews, each one focusing on the emotional reactions to recurring, day-to-day problematic experiences within different role areas. Here only stresses aroused in response to the strains of marriage are relevant. Following the series of questions about married life, respondents were told, "Now think of all the pleasures and problems that go into daily life with your (husband/wife). When you think of these things, how_____do you feel?" Seven adjectives describing negative emotions were serially presented in this form: bothered, tense, bored, frustrated, unhappy, worried and neglected. Respondents were instructed in each instance to indicate whether they felt the emotion very much, somewhat, only a little or not at all. Depending on the intensity of the negative emotions, a score of from one to four was given each item. Thus, for the seven items the raw scores ranged from seven to twenty-eight.

The relationship between status inequality and stress is presented in Table 1 and is tabulated separately for men and women simply to demonstrate that it is parallel for the sexes, although somewhat smaller for men. Beyond this, attention should be directed to the fact that inequality *per se* is not related to intense stress (a category made up of the approximately 20 percent scoring highest on the measure). What is pertinent is the particular pattern of inequality; when the subject is hypergamous, coming from a status background lower than his partner's and thus marrying up, he is not disposed to stress. When he is hypogamous by virtue of having married down, he is especially vulnerable to

Table 1. Relative Status Background of Spouses and Marital Stress by Sex of Respondent (percent)

	Women Whose Husbands' Status Backgrounds Are:			Men Whose Wives' Status Backgrounds Are:		
Marital Stress	Lower	Equal	Higher	Lower	Equal	Higher
Intense	27	22	14	22	16	12
Moderate	22	17	26	12	14	17
Low	51	61	60	66	70	70
N[A] =	(175)	(259)	(154)	(176)	(215)	(154)

For women and men combined $\lambda^2 = 16.7$; 4 d. f.; $p < .05$.

[A]This table includes only respondents for whom there is information about father's and father's-in-law occupational statuses. The total N is thus smaller than the number of married subjects in the sample.

stress. Although both hypergamy and hypogamy indicate inequalities of status in the marriage, their consequences for stress are quite different. A similar association was found by Goode (1956:100-2) in examining the status differences that existed in the marriages of divorced women.

The relationship of these patterns of inequality to marital stress are central to this analysis, and much of the remainder of the paper is aimed at elaborating and explaining how individual stress is influenced by the status structure of marriage. Before this, it is useful to consider some issues that might have a bearing on the findings of Table 1. One such issue concerns the criteria for judging status. Table 1 reflects differences in ascribed statuses and is based on the relative occupational standing of spouses' fathers. But this is not the only condition shaping the status structure of marriage. Also potentially important in this respect are spouses' own education and occupational achievements. This being the case, the question understandably arises as to whether these achieved statuses when considered in conjunction with status backgrounds, reduce or intensify the relationships shown in Table 1. It is important to know, for example, if the likelihood of stress is reduced among women who are hypogamous by status background but who married men whose eventual occupational achievements equal or exceed those of their fathers. In fact, the disposition to stress is no less for this group than for women whose husbands' occupational statuses are below their fathers'. Overall, current family status, based on husbands' occupations, does not alter the bearing of status backgrounds on stress.

One other factor that might reasonably be expected to affect the connections between status inequality and stress is the length of the marriage. It is apparent from Table 1 that the way status origins are combined in marriage does have some impact on feelings of stress, but does not this impact wear thin with time? Marriage is a primary relationship whose interactions span much life-space, and which is typically more intimate than any other a person is likely to have. All this suggests that social differences carried into the marriage from spouses' backgrounds would erode as interactions are extended through time. Somewhat surprisingly, this is not the case, for we find that patterns of status inequality are related to marital stress in essentially the same manner regardless of the years of marriage. These initial divisions clearly have a tenacious quality and, consequently, the length of marriage, like achieved status, is not relevant to the findings of Table 1. The remainder of the paper considers factors that do contribute to the linkage of status inequalities and stress.

STATUS INEQUALITY AND EXCHANGE IN MARRIAGE

What does marrying a person of lower status rank represent, what meaning can it have, what processes does it trigger in marital relations that produce the relationship between inequality and stress? These are the questions that we shall be considering. In attempting to answer them, the theme of exchange will be appearing throughout the analysis. Exchange simply refers to the giving and receiving of rewards in interpersonal transactions (Homans, 1958; Blau, 1964). This is a theme that ordinarily is associated with relations that are more clearly contractual than is marriage, which, by contrast, is a relation often formed on the basis of romantic attraction and having strong affective components. Yet, love notwithstanding, the results of Table 1 suggest that some couples do make invidious status comparisons between themselves and their spouses and, as much of the following discussion will show, vital elements of marital exchange are influenced by whether one must, as he regards his mate's origins, cast his gaze upwards or downwards. When one feels himself to be a loser in a marriage marked by inequality, there are likely to be problems in interaction and exchange. It will be seen that whether or not one experiences loss depends on the importance he attaches to status. But when a sense of loss does occur, there is also a very good chance that stress will occur, too. Thus, status inequality and stress become linked, as in Table 1, through status values and intervening disruptions in marital relations.

We begin assembling the evidence for these statements by examining four aspects of interaction and exchange in relationship to status inequality. They are: (1) the reciprocity of marital exchange; (2) the exchange of expressiveness; (3) the exchange of affection and (4) the sharing of values. First, we shall

see how each of these is affected by status inequalities in marriage and then we shall see how each becomes an intervening condition leading to individual stress.

Reciprocity. The present concern focuses less on what is exchanged in marital transactions than on the general equitableness of the exchange. At issue is not what is conferred or withheld in interaction, but whether there is an equal share of rewards – an overall reciprocity in what partners give to and take from their marriages. In marriage, as in other relations, people want the rewards they receive to be equal in value to those they see themselves conferring (Scanzoni, 1972). The judgment of value, and ultimately of reciprocity, depends largely on the priorities of people engaged in the exchange. Reciprocity does not only obtain by exchanging identical rewards, but more often by a mutual appraisal of different kinds of rewards as being of equivalent value (Gouldner, 1960).

Reciprocity is measured here by three questions to which people indicated either strong agreement, agreement, disagreement or strong disagreement:

1. My (husband/wife) usually expects more from me than (he/she) is willing to give back.
2. My (husband/wife) usually acts as if (he/she) were the only important person in the family.
3. Generally, I give in more to my (husband's/wife's) wishes than (he/she) gives in to mine.

These three items tap the general equality that respondents experience in the give and take of their marriages – how they see themselves as objects of fairness or unfairness in their prevailing daily marital exchanges. Each item is scored from one to four depending on the intensity of response, and the sum of the three items ranges from three to twelve. Based on their scores, respondents are then categorized as seeing exchanges with their spouses as being non-reciprocal (scores 3 to 8), partially reciprocal (9 and 10) or reciprocal (11 and 12).

It can be reported that by itself status inequality makes little difference to reciprocity. That is, in looking at whether the reciprocity that one experiences is influenced by being of higher, lower or equal status to his spouse, only a small and insignificant relationship is found. As suggested earlier, the disruptive influence of status inequality on reciprocity, and on the other aspects of exchange, depends on the status values that people hold in unequal marriages. Specifically, when status inequality exists conjointly with status striving, the likelihood of there being disruptive consequences increases considerably. Striving, the importance people ascribe to status aggrandizement, provides the psychological context by which status realities acquire much of their meaning. If status advancement has a prominent place in the priorities of a person, marrying down may be experienced by him as a keen loss; the same status striving, it will be evident, can turn an upward marriage into a symbol of personal gain and success. Because status values are so instrumental in shaping the consequences of status events, they will be taken into account when trying to assess the full significance of inequality for each aspect of marital relations.

A number of questions were presented that are indicative of status values. One item asked subjects with which social class they identify themselves when they think of the prestige of their (or their husbands') occupations. Immediately following this query was the question on which we are relying as an indicator of priorities people attach to status advancement. This question asks: "How important is it to you to move to a higher prestige class than the one you're now in?" In Table 2[2] respondents are dichotomized according to whether they reply that this is of great, of some, or of little or no importance. They are further divided by the directions of their marriages; that is whether the respondent married down by choosing someone of lower status, chose a mate of equal status or married up to a person of higher status. The results are quite evident: when status is important to the individual, its unequal distribution influences his appraisal of fairness and reciprocity in marital exchange. Concretely, among strivers the person bringing the greater status to the marriage is quite likely to judge his partner as unreciprocating. By contrast, people holding

[2] Variations in the total N between Tables 1 and 2 – and all subsequent tables – are due to differences in the numbers responding to the various items.

Table 2. Importance of Status Advancement, Relative Status Backgrounds of Respondents and Spouses, and Reciprocity of Marital Exchange (percent)

	Respondents for Whom Status Advancement Is:					
	Of Great or Some Importance and with Spouse of:			Of Little or No Importance and with Spouse of:		
Reciprocity of Exchange	Lower Status	Equal Status	Higher Status	Lower Status	Equal Status	Higher Status
Nonreciprocal	40	34	16	17	25	19
Partially Reciprocal	37	30	30	37	34	42
Fully Reciprocal	23	36	54	46	41	39
N =	(78)	(96)	(75)	(248)	(352)	(213)
	$\chi^2 = 13.9$; 4 d. f.; $p < .01$.				N. S.	

identical status striving values, but who have married up, are, more than any other group, likely to possess a sense of reciprocity in their marriages. Still by contrast, when people place little value on status advancement, there is a good chance that they will see their marital relations as being reciprocal, regardless of the actual status relations in marriage.

It is apparent, then, that when one is both a status striver and wed to a person whose origins are of lower status, one is apt to feel that he contributes more to the partnership than he receives in return. His status values probably so inflate his judgments of his own input that, regardless of what his partner gives, he is likely to feel that it does not measure up to what he has brought to the marriage. Special note should be made of the fact that reciprocity is influenced by hypergamy as well as by hypogamy among status strivers. What differs is the direction of the influence: unlike those who have married down, status conscious people who have married up are disposed to see themselves as enjoying a reciprocal marriage. From the perspective of exchange, the hypergamous strivers are the beneficiaries of something they value, and their estimates of reciprocity reflect their satisfaction with the rewards they have received. Thus, it is that the status order of the larger society may structure marital relations in a manner that stimulates a sense of gain or one of loss. And, as will be shown later, when one sees himself a loser in this exchange, he becomes vulnerable to intense stress in marriage.

Expressive communication. Our focus shifts now from the way that conditions of status affect the balance of give and take in marital exchange to a consideration of how these same conditions influence what is given and what is witheld in the exchange. Of central importance in this regard is expressive communication, the ability to exchange ideas, experiences and feelings that are of deep significance to one or both of the partners. Marriage, as it is usually thought of, is a primary relation in which intimate communications can go on freely and without hindrance. In modern societies, where social relations are so commonly of a secondary nature, one of the major functions of marriage is to provide an opportunity for communicating about important feelings that are usually not revealed in ordinary social relations. The question that is raised here is whether this function is hindered by status inequalities between husbands and wives.

There are three pieces of information that reflect on the flow of expressive communications in marriage. One comes from this statement with which respondents were asked to indicate strong agreement, agreement, disagreement or strong disagreement: "My (husband/wife) is someone I can really talk with about things that are important to me."

The next is from a question asking respondents to whom they turn first "to talk about very personal problems" and the interest here concerns only whether they indicate their spouses or another person. The third query is in the same format as the second, this one asking whom the respondents choose to be with when they are feeling "depressed or down." Table 3 again organizes respondents both according to the importance they attribute to status advancement and by the actual status arrangements of their marriages. These conditions are then presented in relationship to the proportions engaging in expressive communications with their spouses. Because of differences in response categories the three items are not combined into a single index of expressiveness, but are presented separately.

It is readily evident that such expressive communication is least likely to occur among status strivers who have married down and most among those who have married up. For example, only 58 percent of the former but 81 percent of the latter strongly assert that their spouses are persons they can talk to "about things really important." The same status differences, however, are seen to have no effect on the communications of people to whom status advancement is of little or no importance. These results, then, are completely consistent with what we learned about reciprocity.

Exchange of affection. A third element in marital relations, different from but akin to the expressiveness of husband-wife communications, concerns the exchange of affection. Two items bear directly on this issue, both of them statements with which respondents were asked to indicate the strength of their agreement or disagreement: "My (husband/wife) is affectionate toward me"; and "My (husband/wife) is someone who is a good sexual partner." The results here parallel those found with regard to communication, for once more it is the hypogamous mates who are least likely to experience affectionateness and the hypergamous most likely to do so. But again, these relationships are found only among those to whom status advancement is important, for the status structure of the marriage has no influence on affectionateness among people to whom status makes little or no difference. It is apparent, then, that status, which separates and divides people in the society at large, may penetrate to the most intimate levels of marital relations, especially so when the achievement of status is a goal of high priority.

Sharing of values. A final critical aspect of marital transactions, one also affected by status inequalities between husbands and wives, concerns the sharing of values. The importance of value consensus in marriage is self-evident, especially so if it is assumed that some measure of agreement on basic issues is necessary, albeit not sufficient, to avoid falling into a consuming conflict. In the normal course of interaction, there is a very broad range of values that enters into the decisions and actions of couples. Here only two are considered, an unavoidably small segment of the rich array of values around which conflict and consensus exist in marriage. Nevertheless, each is important to the functioning of many households: the expenditure of money and the handling of children. Consensus on these issues was assessed by questions that asked: "How do you and your (husband/wife) compare in your opinions about the following things? Are your views about the same, somewhat different or very different on (1) the way money should be spent; (2) the way children ought to be raised?" The relationship of status inequalities and of status striving to consensus on these value areas is shown in Table 5. Once more the status oriented hypogamous group manifests least consensus and, on the spending of money, the hypergamous strivers the most consensus (there is virtually no difference between those marrying across and those marrying up on child-rearing).

Taking a broad view of marital transactions, then, it is clear that status conditions and status values have a very real presence. The equitableness spouses perceive in the general give and take of their marriages, the nature of the communications they engage in, the affection they experience from one another and the values they share are all influenced by their status characteristics and the way these characteristics are combined to give form and structure to the marriage itself. Status inequalities are remarkably persistent influences on marital transactions, resisting any changes brought about by mobility that occurs after the marriage, and resisting also what would seem to be the powerful levelling effects of continued and intense interaction

Table 3. Importance of Status Advancement, Relative Status Backgrounds of Respondents and Spouses, and Expressive Communication (percent)

Indicators of Expressive Communication	Respondents for Whom Status Advancement Is: Of Great or Some Importance and with Spouse of: Lower Status	Equal Status	Higher Status	Of Little or No Importance and with Spouse of: Lower Status	Equal Status	Higher Status
A. Spouse someone "I can really talk with about things important to me."						
Strongly agree	58	61	81	74	75	76
Agree	29	35	17	20	19	19
Disagree and Strongly disagree	13	4	2	6	6	5
N =	(78)	(95)	(75)	(249)	(352)	(212)
	$\lambda^2 = 18.3$; 2 d. f.; $p < .01$.				N. S.	
B. Percent saying they speak to spouse before speaking to others about "very personal problems."	72 (77)	73 (95)	89 (74)	79 (253)	81 (355)	81 (215)
	$\lambda^2 = 8.1$; 2 d. f.; $p < .02$.				N. S.	
C. Percent choosing spouse as person they want to be with when feeling "generally depressed or down."	59 (78)	67 (95)	75 (75)	59 (253)	61 (354)	63 (216)
	$\lambda^2 = 4.3$; 2 d. f.; $p < .12$.				N. S.	

Table 4. Importance of Status Advancement, Relative Status Backgrounds of Respondents and Spouses, and Affective Exchange (percent)

Indicators of Affective Exchange	Respondents for Whom Status Advancement Is: Of Great or Some Importance and with Spouse of: Lower Status	Equal Status	Higher Status	Of Little or No Importance and with Spouse of: Lower Status	Equal Status	Higher Status
A. My (husband/wife) is someone who is affectionate toward me.						
Strongly agree	69	75	83	77	76	77
Agree	22	21	17	19	20	20
Disagree and Strongly disagree	9	4	--	4	4	3
N =	(78)	(95)	(75)	(250)	(352)	(212)
	λ^2 = 13.1; 4 d. f.; $p < .05$.				N. S.	
B. My (husband/wife) is someone who is a good sexual partner.						
Strongly agree	64	69	83	74	74	79
Agree	28	20	16	22	24	16
Disagree and Strongly disagree	8	1	1	4	2	5
N =	(78)	(94)	(75)	(245)	(342)	(207)
	λ^2 = 13.5; 4 d. f.; $p < .05$.				N. S.	

that takes place for a large number of couples in the sample over a period of many years.

It might appear that the penetrating and durable influence exerted by status inequalities on the sorts of transactions we have been looking at is a reflection of the different and, perhaps, clashing norms and standards that people of different social backgrounds bring to marriage. This cannot explain the relationships we have seen, however, since those who have married up have marital experiences very different from those who have married down, although each groups has a disparate status relative to their respective mates. The marital disruptions that have been examined are not, therefore, a simple result of differences in custom and attitude. The explanation lies, instead, with status values and the meaning such values give to inequalities. Thus, it is not marrying a person of lower status background that matters; it is having married down while striving to move up that is most apt to bring disruption to marital exchange. The sense of loss is keenest among these people, for it involves the sacrifice of something they prize – status. Such loss and deprivation then colors the appraisal of reciprocities and further separates and divides couples in ways that hinder marital exchanges. But, whereas the hypogamous strivers may experience detachment from their spouses and disruption in marital relations, people with the same status goals who have married up are outstandingly likely to experience marriage as involving equitable and gratifying exchange. Thus, social status becomes part of the fabric of husband-wife interaction, one of the conditions that shapes the appraisals and defines the value of elements exchange in the marriage.

Table 5. Importance of Status Advancement, Relative Status Backgrounds of Respondents and Spouses, and Value Consensus (percent)

	Respondents for Whom Status Advancement Is:					
	Of Great or Some Importance and with Spouse of:			Of Little or No Importance and with Spouse of:		
Indicators of Value Consensus	Lower Status	Equal Status	Higher Status	Lower Status	Equal Status	Higher Status
A. Comparison or respondent's and spouse's views about the way money should be spent.						
Same	60	76	80	70	76	76
Somewhat different	23	21	19	28	20	20
Very different	17	3	1	2	4	4
N =	(78)	(95)	(75)	(251)	(351)	(213)
	λ^2 = 18.9; 4 d. f.; p < .001.				N. S.	
B. Comparison of respondent's and spouse's views about the way children ought to be raised.						
Same	69	86	84	83	80	80
Somewhat different	24	13	16	15	18	14
Very different	7	1	--	2	2	3
N =	(62)	(80)	(62)	(212)	(302)	(185)
	λ^2 = 10.3; 4 d. f.; p < .05.				N. S.	

INEQUALITY, EXCHANGE AND STRESS

It has been argued that the status arrangements existing in the society converge in marriage in ways that may ultimately have adverse consequences for the emotional well-being of the partners. Such consequences are created when status inequalities, coupled with status striving, disrupt exchange; this sort of disruption, we submit, then becomes a condition eventuating in stress. It has already been demonstrated that marriage between strivers of unequal status origins is related to the disruption of crucial elements in marital exchange. A rounding out of the empirical evidence for our argument should now show directly that status inequality results in individual stress through its disruptive effects on exchange. In order for disrupted exchange to stand as an intervening condition connecting status and stress, however, such exchange must itself be related to stress (Rosenberg, 1968: 54-83). This is what will be ascertained first.

In Table 6 each of the several aspects of marital exchange that was earlier presented as a consequence of the status of marriage is reassembled and shown in relationship to stress. It is clearly evident that the chance of intense stress being present is greatest when little reciprocity is experienced in the marriage, when expressive communications are attenuated, when the signs of affection are weak and when the sharing of values is limited. Where there is a disruption in these crucial elements of exchange, it is most unlikely that spouses can participate in the marriage with a sense of well-being.

A brief caveat needs to be injected here concerning the difficulty in establishing the boundary separating problems residing in

Table 6. Disruptions in Marital Exchange and Proportions Experiencing Intense Emotional Stress

Marital Exchanges	Proportions Experiencing Intense Stress*	
A. Reciprocity of exchange	Partially and fully reciprocal	Nonreciprocal
	14 (784)	40 (230)
B. Expressiveness	Strongly agree	Less than strongly agree
Can talk with spouse "about things really important to me."	13 (770)	38 (290)
	Spouse	Others
To whom do you first speak about personal problems?	16 (849)	36 (212)
Whom do you choose to be with when feeling depressed or down?	14 (663)	29 (399)
C. Affection	Strongly agree	Less than strongly agree
Spouse is "affectionate toward me."	15 (809)	35 (252)
Spouse is "good sexual partner."	15 (772)	32 (268)
D. Values	Same	Different
How do your views compare with your spouse's on "way money should be spent?"	14 (781)	36 (280)
How do your views compare with your spouse's on "way children ought to be raised?"	16 (734)	35 (167)

*Significance of Chi-Square .001 or better for all relationships.

marital transactions from stresses residing in individuals. It has to be recognized that the way a person appraises the transactions of which he is a part may be influenced by stresses already existing within him (Lazarus, 1967). Although stress is treated in Table 6 as being aroused by disruptive circumstances in marital transactions, stress may at the same time be helping to define these circumstances as disruptive. But whatever effects stress itself may have in interfering with reciprocity, communications, affection and value sharing, the fact should not be obscured that these features of marriage, as we have seen, also stem substantially from the status structure of the marriage, a structure formed at the very initiation of the union.

We can now bring the discussion around to the final goal of the analysis, which is to show empirically that the connection between status inequality and stress comes about through the problems and strains that arise in daily marital exchange. This can be done by asking, statistically, what the relationship of status inequality to stress would be if problems in marital exchange were not to intervene in the process. This is the question around which Table 7 is constructed. Using correlations rather than distributions because of the large number of variables involved, this table shows the relationship between status inequality and stress separately for status strivers and for those to whom status is relatively unimportant. The coefficient for the strivers is .18 ($p < .001$), indicating a positive association between marrying down and stress; for the nonstrivers the correlation is smaller ($r = .06$, n.s.). However, when all the

Table 7. Correlations between Status Backgrounds and Stress by Importance of Status Advancement, Uncontrolled and Controlling on Marital Exchange

	Original Correlation between Status Inequality and Stress	Controlled Correlation between Status Inequality and Stress
Respondents for whom advancement is of great or some importance	.18 (200)	.01 (200)
Respondents for whom advancement is of little or no importance	.06 (735)	.07 (735)

elements of husband-wife transactions that are arrayed in Table 6 are eliminated from this relationship through the use of partial correlation, the original correlation among the strivers is now reduced to .01, virtually disappearing. Thus, people who marry partners of lower status while valuing status advancement are relatively likely to form daily marital exchanges marked by disaffection and a sense of loss and these conditions, in turn, contribute to stress.

DISCUSSION

Status inequality and status striving, conditions rooted in the organization of social relations in the surrounding society, reach out to affect the feelings of distress in individuals. Hierarchical arrangements that exist in the larger society may be brought into marriage in heterogamous unions; when such arrangements are incorporated into marriage, they then come to organize the flow of daily marital interactions, having deleterious effects on reciprocity, communication, affection and value consensus. When there is a disruption in these important aspects of husband-wife transactions, the chances of there being personal stress are greatly increased. It is by this kind of process that features of social structure may come to have an impact on the inner lives of people.

The findings unravelled here are quite consistent with earlier work showing an association between status heterogamy and problems in marriage. But this analysis is less concerned with whether the association exists than how it comes about. There has been an assumption that marital problems in heterogamous marriages reflect conflicts in the cultural backgrounds of people coming from different classes. This interpretation, however, is not consistent with the fact that those marrying up are much less likely to have marital disruptions than those marrying down, although cultural differences are equally present in each group. A culture-conflict interpretation is similarly inconsistent with the fact that the length of time people have been married does not lessen the consequences of status inequality. If cultural differences accounted for problems in heterogamous marriages, we could reasonably expect that both the differences and the problems would fade with time. To whatever extent cultural conflicts do characterize heterogamous marriages, they cannot explain the stresses that were observed here.

To account for marital problems and marital stress it needs to be recognized, first of all, that status inequality by itself is of little or no consequence. The importance of such inequality to marital problems and marital stress depends on the meaning and value that one attaches to it. Glenn and his colleagues (1974) were correct in objecting to the kind of counselling being given about mate selection on the basis of what we know. If such counselling is to have any validity, it should focus not on status differences, but on status consciousness. It is when differences existing in the larger society retain their importance within the context of marriage that relations sour. When status consciousness invades marriage, apparently, exchange equivalence is difficult to maintain.

It should be underscored that the purpose of this paper was not to account for marital stress to as great an extent as possible, but to learn in particular how status factors contribute to such stress. Indeed, the emphasis given status inequality and status striving cannot be

construed as meaning that they are of preeminent importance to stress in marriage. First of all, their prevalence must be recognized as somewhat limited because these two status conditions come together only in a minority of marriages. Nor is their importance evident from their direct impact on stress, for most of their influence is indirectly exerted through intervening conditions. But what is notable is not that status conditions have a limited part in marital stress, but that they have any part at all. In order for these conditions to be among the circumstances that contribute to emotional stress, they must first circumvent whatever barriers primary relations pose to hierarchical divisions, they must be persistently maintained over time and their influence must fan out to a number of crucial transactions which, in turn, reach the emotional interior of individuals. All this stands as further testimony of the ubiquitous importance of status stratification to behavior and feeling.

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GENERATION AND FAMILY EFFECTS IN VALUE SOCIALIZATION*

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To what degree is the socialization of "core" values a function of family, as opposed to generational influences? Data from 2,044 individuals, subsequently reduced to 256 grandparent-parent-youth lineages, are used to explore this issue. Multivariate analysis resulted in the identification of two value dimensions (Humanism/Materialism and Collectivism/Individualism) that characterized the range of choices from which respondents made value judgments. Generational differences in Collectivism/Individualism values were apparent, and on this dimension low to moderate family transmission effects did emerge. But the capacity to explain value choices on the basis of either of these sources of influence was minimal, especially on Humanism/Materialism.

Of concern to much social theory is the role of value socialization in social stability and change. In the face of continuous replacement of individuals through birth and death, conceptions of desirable behavior must be transmitted from one generation to the next if cultural continuity is to be maintained. The family is usually considered the most important mechanism in value socialization. But the influence of age-peers, of age-graded institutions outside the family such as the school, and of socio-historical events which impinge in special ways on youth, have increasingly been recognized in socialization theory as important—and alternative—determinants of value orientation (see Mannheim, 1952; Eisenstadt, 1955; Coleman, 1961; Kandel and Lesser, 1972).

Such age-related factors, which often are collectively considered as "generation effects," have attracted considerable attention recently in the social sciences. The emergence of alternative value orientations among youth has frequently been noted in analyses of the student movement and counterculture in the past decade. In a broader context, contrasts between birth cohorts or age strata have been identified as an important mechanism for social change (Ryder, 1965; Flacks, 1971; Riley et al., 1972; Bengtson and Black, 1973).

However, there is little confirmed theory in the socialization literature concerning value transmission or contrast, in terms either of within-family or between-generation effects. For example, the evidence concerning parent-child transmission is contradictory. Some studies support the proposition of considerable lineage similarity in orientations (see Hyman, 1959; Flacks, 1967; Hill et al., 1970; Troll et al., 1969; Thomas, 1971; Kalish and Johnson, 1972) while other analyses question or qualify the efficacy of family transmission (Jennings and Niemi, 1968; Hill et al., 1970; Furstenberg, 1971; Kandel and Lesser, 1972; Connell, 1972).

Nor is there agreement concerning the nature or magnitude of differences between larger aggregates defined by age or generation. Several observers have suggested a dominant characteristic of American society in the past decade was an ever-widening "gap" in values between groups defined by age (Roszak,

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1969; Mead, 1970; Reich, 1970; Slater, 1970). Some empirically-based analyses appeared to support the proposition of significant age contrasts in values (Ingelhart, 1971; Payne et al., 1973; Youmans, 1973). But others have questioned the degree of generation differences in value orientations (Douvan and Adelson, 1966; Thurner et al., 1974) or pointed to the greater importance of period or historical effects in examining trends over time (Cutler and Bengtson, 1974). Still others have noted extensive within-generation variability (Yankelovich, 1972; Kasschau et al., 1974) which question simple attribution of between-generation contrast.

Several reasons account for the lack of clear theory concerning inter-generational value transmission. The first and most obvious is the lack of adequate longitudinal data and the necessity, in its absence, to infer transmission from manifestations of similarity at one point in time (for an exception see Elder, 1974). A second reason involves other problems of method in many studies to date (reviewed in Bengtson and Starr, 1975): samples restricted to college students and their parents, a tendency to treat the younger generation as homogeneous, inadequate conceptualization and measurement of "values"—a construct which, in many studies, has been left virtually undefined.

But a third problem, reflected in much of the research cited above, can be traced to ignoring the distinction between family and generation effects and the lack of adequate models for assessing these effects. Differences between age cohorts do not necessarily reflect incongruence between parents and children; nor does within-family concordance necessarily imply between-generation similarity at a broader societal level (see Connell, 1972; Kandel and Lesser, 1972). Thus, a key issue in any research concerning inter-generational transmission is the disentanglement of these two socialization effects, which often appear to be in opposition, and the assessment of their relative influence on the formation of values.

The lack of consistent definition of relevant constructs represents a fourth difficulty. The terms "generation" and "generation effects" have been used variously in the literature of sociology, developmental psychology and political science in past decades (see Troll, 1970); often in research reports it is undefined. There appear to be three major perspectives on generational analysis: generation as birth-cohort, generation as ranked descent within family lineage and generation as an activist socio-political or intellectual subgroup in Mannheim's (1952) sense. Each perspective involves contrasting implications of the construct, especially in analysis of social change (Bengtson et al., 1974).

In this paper the term is used in a generic sense. In terms of sample definition, the "generations" are lineage members—grandparents, parents and young adult grandchildren. As employed in interpretation, however, "generation" will be used to refer to a broad age group or cohort born during roughly similar historical periods and who thus are at similar levels of life-cycle development. This is similar to Mannheim's (1952) use of the term "actual generation," Ryder's (1965) "cohort," and "age stratum" (Riley et al., 1972). Such usage, of course, blurs possible distinctions due to *maturation* (aging) and *cohort-historical* factors operating to produce "generation effects" (see Schaie, 1967; Riley et al., 1972; Mason et al., 1973; Cutler and Bengtson, 1974) to say nothing of possible socio-political or ideological groupings implied in Mannheim's "generation units" (Laufer and Bengtson, 1974). But in the research design to be employed, the broad definition used here allows all such aggregate *age group* factors to be considered in contrast to *family* effects (biological or social lineage) in assessing influences on values. This provides necessary baseline data from which the effects of subsequent controls (influence of age-peers, education, identification with parents or cohorts) can be employed. There is also empirical justification for grouping age cohorts into the three generations, as will be discussed later.

This research attempts to identify family and generation effects in value socialization, to determine their relative efficacy and to assess their joint contribution in predicting value orientations of youth. The specific research issues concern: (a) identifying the underlying dimensions which characterize values in the three age-cohort groups; (b) assessing the extent of age group differences on the relevant value dimensions; (c) examining the extent of within-family similarity; (d) analyzing the relative effect of generational and familial factors in predicting value orientations.

METHODS AND PROCEDURES

Population and Sample

The population sampled were members of a major medical health plan (840,000 subscribers) representing a broad range of variation in socio-economic status. Each sampled family was to include a grandfather with a dependent enrolled in the health plan (usually the spouse), and at least one living child who in turn was the parent of a youth between the age of 16 and 26. Of 7,112 potential grandparents randomly drawn from the population, only 595 (8%) were members of a family with these characteristics and were able to furnish addresses of the other generations.

Each eligible member of the families thus defined was mailed a lengthy questionnaire requiring over an hour to complete. Useable questionnaires numbered 2,044 (reflecting a 65% response rate).

Although three-generation families including young adults are rare, as reflected in our sampling experience, the study group that resulted represents a wide range of social characteristics. The age range of respondents was 16 to 91 years. Grandparents (hereafter termed "G1") numbered 516 (average age 67), with 701 parents ("G2"; average age 44) and 837 grandchildren ("G3"; average age 19.7 years). Fifty-five percent of the total sample is female. Protestants predominate (54%; 18% Catholic and 12% Jewish) as does Democratic party preference (53% to 32% Republican). About 20% of household heads in the sample held low-status, blue-collar jobs. But these were largely in the older age brackets; over half of the 35-44 year old males had high-status white-collar occupations. In this sample, each generation has tended to surpass the occupational achievements of the previous generation (see Aldous and Hill, 1965; Hill, 1970 for similar findings).

Operationalization of Value Orientations

The concept "value" has been defined and operationalized in many different ways in sociology (see discussion by Kluckhohn, 1951; Scott, 1965; Rokeach, 1968; Kohn, 1969; Williams, 1971). Because of such variability, an attempt was made to develop explicitly a nominal definition of the construct, to develop a multidimensional operationalization and to test the adequacy of the dimensions defined using factor analysis procedures.

	Expansion	Restriction
Collectivity	A. Values reflecting Humanism	B. Values reflecting Collectivism
Egoistic	C. Values reflecting Materialism	D. Values reflecting Individualism

Figure 1. Value Typology Employed in This Research

In this research, values are considered *conceptions of desirable ends which serve as guides to action.* The components of this definition, which relies on prior definitions by a number of scholars, are discussed at greater length in another publication (Bengtson and Lovejoy, 1973). What is most important to note is that, while there are by this definition a variety of discrete values, they are assumed to be patterned and organized according to superordinate cognitive categories. In defining some dimensions which reflect such groupings, this research follows a suggestion adapted from Fallding (1965) to the effect that specific value orientations reflect intersections of two conceptual axes: *egoistic-collectivity* and *expansion-restriction* (see Figure 1). For example, an individual may define desirable ends in terms of direct self-enhancement (egoistic) and in terms of an increase in intensity through narrowing (restriction). He or she would thus highly value "Individualism" (the term applied in the present research). Another person may perceive desirable ends in terms of a frame of reference more inclusive than himself (collectivity) and by an increase in breadth (expansion). He or she would highly value "Humanism." The result from this classification is a set of four ideal-types of values (see Figure 1). It is postulated that individuals and groups will vary according to their relative positions in these four categories of values.

[1] Rokeach has made a distinction between "instrumental" and "terminal" values, corresponding to preferable modes of conduct as contrasted with preferable end-states of existence. Such a distinction between means and ends appears difficult to maintain; we chose to define values here simply in terms of end-states ("terminal" orientations).

Table 1. Loadings of the 16 Values Items on Two Factors for Total Sample (n = 1996)

	Factor I Humanism/Materialism		Factor II Collectivism/Individualism	
Variable	Loading	Integral Weight*	Loading	Integral Weight*
Service	-.59	-6	-.20	0
Equality	-.52	-5	-.07	0
A world at peace	-.45	-4	-.22	0
Ethical life	-.43	-4	-.16	0
Financial comfort	.76	8	-.06	0
Possessions	.58	6	.09	0
Attractive appearance	.38	4	.20	0
Respect or recognition	.27	3	-.02	0
Religious participation	-.15	0	-.54	-5
Loyalty	.09	0	-.51	-5
Patriotism	-.17	0	-.46	-5
Friendship	-.12	0	-.15	0
Skill	.18	0	.54	5
An exciting life	.19	0	.54	5
Personal freedom	-.05	0	.52	5
Sense of accomplishment	.15	0	.26	3

*This weighting was assigned to each value as ranked by the respondent in the calculation of factor scores for each case. Only items greater than .25 were used in defining such factor scores; thus 8 items entered into Factor I scores, 7 items in Factor II scores. (Friendship was not included on either factor because of its low and ambiguous loading).

The construct of values was operationalized in this research by asking respondents to rank-order a list of 16 value items that "people might find important in their lives." This procedure is similar to that developed by Rokeach (1968),[1] but the 16 items presented were chosen to reflect the four superordinate dimensions described above. Accordingly, respondents rank-ordered the following 16 items, presented in a random format.

(1) Equality; (2) Service (devotion to bettering mankind); (3) A world at peace; (4) An ethical life (responsible living toward all mankind). (These items were chosen to reflect *Humanism* value types.)

(5) Financial comfort; (6) Respect or recognition from other people; (7) Possessions (enough things so you can do what you really enjoy doing); (8) An attractive appearance (knowing others admire the way you look). (*Materialism* values.)

(9) Religious participation; (10) True friendship; (11) Loyalty to your own (family and loved ones, church or group); (12) Patriotism. (*Collectivism* or "traditional-institutional" values.)

(13) An exciting life (novelty, adventure); (14) A sense of accomplishment (achievement); (15) Personal freedom (independence, autonomy); (16) Skill (being good at something you enjoy doing). (*Individualism* values.)

A subsequent step in operationalization involved assessing construct validity.[2] Appropriateness of the conceptual dimensions underlying the 16 item set was tested by subjecting responses to principal component factor analysis. Two independent bipolar dimensions emerged from a varimax rotation (see Table 1). Factor 1 accurately reflected the postulated Humanism/Materialism axis (for example, high negative loadings on items concerning service, equality, peace and ethical life; high positive loadings on financial comfort, possessions, attractive appearance). Factor 2 reflected a Collectivism/Individualism dimension, contrasting traditional-institu-

[2]Test-retest reliability (in a sample of 58 students) was .78 (Spearman rank-order correlation) after a four week interval.

tional values with a "do your own thing" orientation.

The factor analytic procedure resulted in dimensions that are, in fact, similar to categorizations of "values" in many other studies (see, for example, Scott, 1965; Flacks, 1967; Payne et al., 1973). The analysis provides support for the *a priori* theoretical categorization initially proposed and allows the use of factor scores reflecting multivariate portrayal of individual responses in the analyses to follow. The factor scores were constructed using the integral weight indicators reported in Table 1 (for procedures employed, see Guilford, 1954: 485, 524-5). The range of value scores is from -120 to +120.

RESULTS

Four major questions guided the analysis of data. The first concerns possible generational contrasts in the *dimensions* underlying value items as ranked by the respondents: do members of different generations[3] display unique configurations of the 16 value items or is there factorial invariance from generation to generation. A second question involves the extent of age-group differences in *location* on the value dimensions. Are there clear contrasts between youth and the older generations? Between the middle-aged and the elderly? Third is the issue of *within-family similarities* in values—the extent to which there appears continuity between parents and children, grandparents and youth, in the ordering of desirable ends. A fourth question concerns the *relative efficacy of lineage as opposed to cohort effects* in accounting for variation in values. When predicting value orientation, to what extent is there similarity or contrast with (a) older or younger members of the individual's family versus (b) members of differing age groups, irrespective of family relatedness?

Factor Invariance across Generations

In exploring the nature and extent of age-group differences in values, the first issue concerns whether members of contrasting generations conceptualize desirable ends in such different ways that the underlying dimensions of value orientation vary from one group to the next. This is the implication of those who postulate a "great gap" between contemporary generations—for example, Reich (1970) in his portrayal of three levels of consciousness related to age-groups, Mead (1970) who has suggested the emergence of a "prefigurative" culture and Roszak (1969).

Although testing the factorial invariance among generations is treated here as a substantive issue, it should be noted that this is also an analytic procedure necessary to subsequent age-group comparisons using factor scores. Unless factorial invariance is established, one runs the risk of detecting changes in the operation of the measuring instrument across generations, rather than actual changes in values (see Baltes and Nesselroade, 1972). The terms "personal freedom" and "an ethical life" may have quite different meaning to a 60 year-old respondent as contrasted with a 17 year-old. Under these circumstances, detected differences in factor scores could merely represent contrasts in instrument by generation interaction, rather than substantively interpreted age-group differences in values. This possibility has not been adequately considered in much previous research contrasting age strata.

The factor analytic procedures described earlier were repeated for each age group separately; then tests were developed to assess the factor invariance between generations.[4]

[3] Throughout the analyses that follow, "generation" rather than chronological age or arbitrary cohort groupings constitutes the independent variable. Despite problems attendant to current usage of the term "generation," there are two reasons for employing categorization of the sample into three generations in the research reported here. The first is a reflection of the sampling design (as well as the statistical models) employed—potential respondents were identified in terms of ranked descent within families, and analytic models (to be described later) required identification of three generational groups in order to ascertain within-family and between-age effects. The second reason is empirical. In another analysis of these data (Bengtson and Lovejoy, 1973) employing Automatic Interaction Detection (AID) to test the degree to which value orientations are predictable from position in social structure, age-status was found to be the most substantively meaningful predictor. Moreover, three age groups emerged in successive splits: the first reflecting G3 *versus* the older age groups and the second distinguishing between G2 and G1; and these did, in fact, reflect the G1, G2, G3 classification employed here.

[4] I am indebted to Walter Cunningham for suggesting the procedure. Tables reporting factor loadings for each generation are available from the author.

Table 2. Analysis of Variance Summary and Mean Scores for Each Value Dimension (256 Three-Generation Triads)

Factor I: Humanism/Materialism

Source	Sum of Squares	df	Mean Square	F	P	Explained Variance Estimate
Family	664,010	225	2,603	1.12	NS	.038
Generation	11,759	2	5,879	2.53	NS	.003
Residual	1,186,167	510	2,326			
Total	1,861,937	767				

Factor II: Collectivism/Individualism

Source	Sum of Squares	df	Mean Square	F	P	Explained Variance Estimate
Family	436,328	255	1,711	1.77	<.001	.159
Generation	260,732	2	130,366	134.47	<.001	.217
Residual	494,427	510	969			
Total	1,191,488	767				

Three independent varimax solutions resulted in 16 item loadings for each item across generations. Similarity of the loadings was then assessed by computing Spearman rank-order correlations between each pair of generations.

The results indicate a high degree of invariance across generations (lowest $r_S = .94$, highest = .99).[5] While in general it is preferable to rotate factors to similarity (Meredith, 1964), under the present circumstances a sufficiently high degree of similarity has been achieved by the normalized varimax criterion (Kaiser, 1958) to permit a conclusion of factorial invariance without recourse to any further procedures (see Jöreskog, 1971, for a discussion). Thus, these procedures suggest that the factor dimensions, commonly defined, can be examined with confidence to determine age-stratum differences in the distribution across the three generational groups; and that factor scores can be validly employed in the age-group analyses to follow. More importantly in terms of theory, the fact that factorial structure is the same in each generation questions many assumptions concerning dramatic contrasts between generations in values or world-view. It suggests the necessity to look for more narrowly defined generational units (Mannheim, 1952; Laufer and Bengtson, 1974) and implies many commonly-held conceptions about fundamental differences between age groups may be exaggerated.

Contrasts in the Distribution of Values across Generations

Although the demonstration of factorial invariance suggests these generational groups do not differ in the *dimensionality* of values examined, they may still differ in their respective *distribution* or location on those common dimensions. To test this, generational differences were examined in an analysis of variance design.

Because of family relatedness characterizing the sample of 2,044 respondents, there is a possibility of intrafamilial correlation which might bias a one-way ANOVA comparison of distributions by age category. Thus the main effect for generation was examined in a two-way, blocked design (sometimes called replication, related or dependent ANOVA to distinguish it from a factorial model) with family membership or lineage as the second dimension. This required drawing a subsample of 768 individuals: 256 distinct family lineages, each represented by one grandparent,

[5] The actual correlations are as follows: on Humanism/Materialism rho = .979 (G1-G2), .996 (G2-G3), .982 (G1-G3); on Collectivism/Individualism rho = .945 (G1-G2), .967 (G2-G3), .959 (G1-G3).

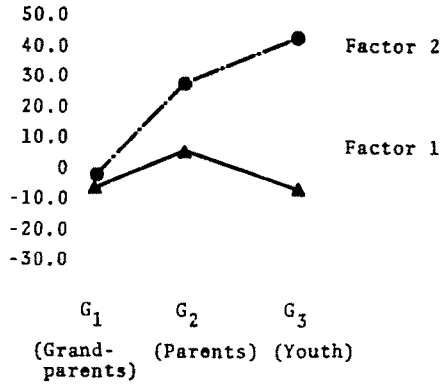

Figure 2. Means of Each Factor Score (Representing Two Dimensions of Values) by Generation.

one parent and one young adult grandchild.[6] In the design, families are treated as replications. As will be discussed later, this allows direct assessment of the relative effects of generation or family lineage on variation in value orientations. For now, our concern is with the main effect of generation (see Table 2). The means of each generation group are plotted in Figure 2.

The results suggest some substantial differences between generations in the distribution of value orientations – but also some unanticipated similarities. On Factor 2, the Collectivism/Individualism dimension, there is a clear progression by age: the grandparents ($\bar{x}$ = -4.06) are lowest (the negative sign indicating a mean towards the Collectivism pole of this dimension) and the grandchildren ($\bar{x}$ = 40.28) are highest (toward the Individualism pole). The generation main effect is statistically significant (beyond .001) and accounts for 22 per cent of the variance. On Factor 1, however, the grandparents and grandchildren are quite similar, with means of -7.18 and -5.69, respectively (negative sign indicating distribution toward the Humanitarian pole of the dimension). The parents, in contrast, appear to place more importance on Materialistic values ($\bar{x}$= 1.76). On this dimension there appears a curvilinear relationship between value orientation and generational membership, with the grandparents even more inclined toward Humanism than the youth.

Any interpretation of between-generation differences on these value dimensions, however, must be tempered by the substantial degree of within-generation variation. The standard deviations for each group are large (for example, 36.12 compared to a group mean of 40.28 for G3 on Collectivism/Individualism). Direct indication of within-generation variation can be seen when the factor score distributions are plotted for each generation and superimposed on the same graph (see Figure 3). Here differences in central tendency can be observed, but the considerable overlap between generations and the high degree of within-group variability is equally obvious. For example, though grandchildren as a group are higher than parents on Individualism (items concerning skill, an exciting life, personal freedom, accomplishment), many parents and grandparents rank these values highly, while many youth rank highly the values of religious participation, loyalty to your own and patriotism (Collectivism).

Thus, although there are statistically significant contrasts between generations on the Collectivism/Individualism value dimension, there is little group difference on Materialism/Humanism. Moreover, the magnitude of within-generation variation appears so substantial as to question a simple generational interpretation of value differences.

Intra-Family Transmission of Values

The degree of transmission within families can be assessed in cross-sectional data only by examining the extent of parent-child similarity at one point in time. With a three-generational design, however, one can trace the path of similarity from grandparent to grandchild and examine the relative effects of the two older generations on the orientations of the youth. In this procedure, it is necessary to keep in mind something frequently ignored in assessment of parent-child similarity: that

[6] The only restriction in this subsample selection was that a given three-generation lineage could be represented only once. For example, if both grandmother and grandfather (G1) responded, one was chosen randomly and the other omitted from this analysis; if that G1 had two or more children (G2) represented in the sample, one was randomly selected; similarly for the G3. This accounts for the sample reduction from 2,044 individuals to 768. Since selection of individuals was random, there are a variety of sex-lineage combinations with approximately equal representation among sex-lineage types–for example, as many grandmother-mother-grandson triads as grandfather-father-granddaughter.

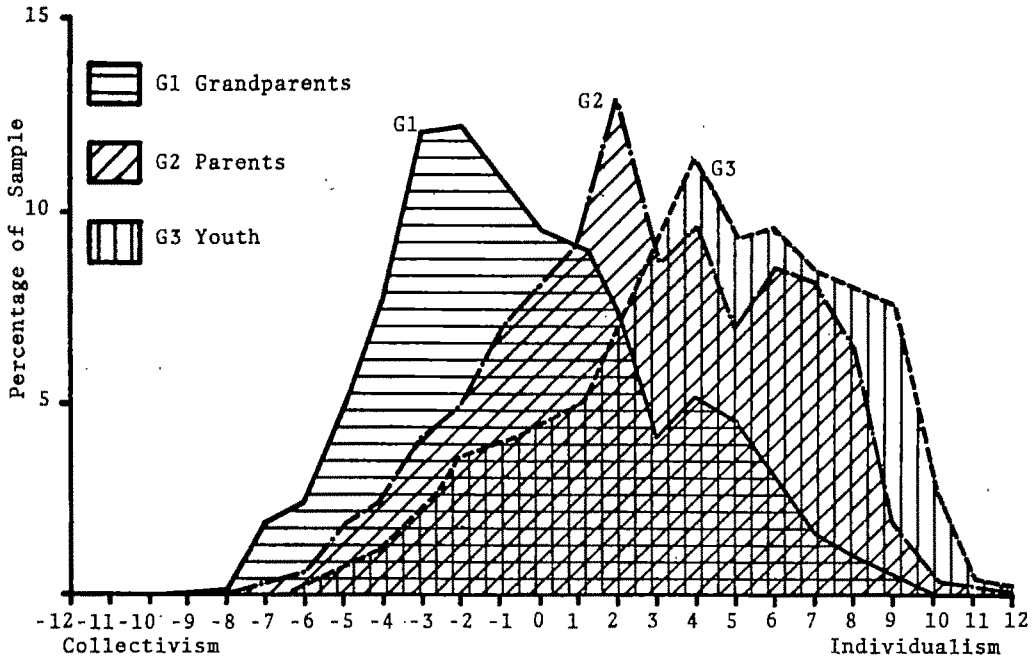

Figure 3.
Frequency Distribution of Factor Scores, by Generation, on Collectivism/Individualism Value Dimension.

covariation is not the same as *agreement* (see Robinson, 1957). The first (usually assessed by regression or correlation techniques) involves predicting the score of one group from another (with the intercept being $\neq 0$), while the second (requiring assessment of absolute agreement on a given dimension) involves a more stringent test of similarity (as in Robinson's A, intra-class correlation, or analysis of variance designs). Both approaches will be taken here.

To test the degree of covariation or prediction in value orientations among lineage members, a regression analysis was performed relating scores of grandparents and parents to grandchildren (Figure 4). The 256 triads described earlier were decomposed into three sets of two-generation dyads. In this model, G3 score is defined by two independent variables (G1 and G2) plus the residual. Examination of direct and indirect effects can be made by comparing the path coefficients with the zero-order correlations.[7]

The results suggest relatively low levels of parent-child prediction and a high degree of residual variation. On Humanism/Materialism, the regression coefficients are extremely low, as are the zero-order *r* between each generation pair. The residual is in excess of .99. On Collectivism/Individualism, the prediction is a bit higher (regression coefficients .08 and .29), but the residual is still large (.91). Comparison with the zero-order correlations reveals a Markovian chain of influence among generations, with little direct G1-G3 similarity.[8]

This analysis suggests three things: (1) virtually no transmission is indicated in Humanism/Materialism (where, as reported in the preceding analysis, there was little group difference by generation); (2) higher family transmission is evidenced on Humanism/

[7] The zero-order correlations between generations on the value factor scores are as follows: on Humanism/Materialism, r = .09 (G1-G2), .05 (G2-G3), -.02 (G1-G3); on Collectivism/Individualism, r = .18 (G1-G2), .30 (G2-G3), .13 (G1-G3).

[8] Many studies of parent-child agreement, particularly in the political socialization literature, have reported covariation coefficients using Tau-b. For purposes of comparison with these studies, value scores were grouped and the following statistics are reported: on Humanism/Materialism, Tau-b is .06 (G1-G2), .02 (G2-G3), -.01 (G1-G3); on Collectivism/Individualism, .13 (G1-G2), .19 (G2-G3), .09 (G1-G3).

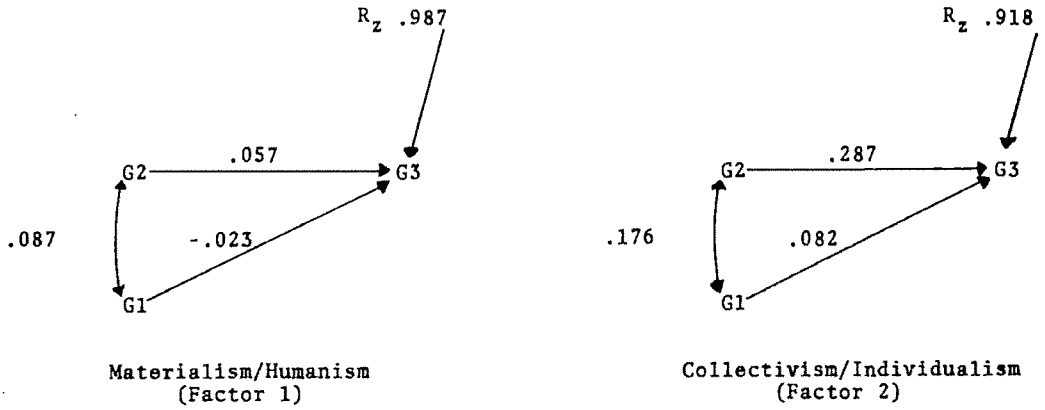

Figure 4. Path Model for Three-Generation Lineages Relating Value Scores of Grandparents (G1) and Parents (G2) to Grandchildren (G3).

Materialism (the dimension on which the previous analysis indicated significant generation group contrasts); (3) whatever G1-G3 transmission exists is mediated through the G2-G3 dyad. In short, these data imply that little in the way of "family traditions" is passed through three generations in terms of abstract value orientations. Moreover, the Collectivism/Individualism value dimension is of interest because it indicates both generation group differences *and* within-family transmission.

The Relative Effects of Generation and Family

The analysis thus far has demonstrated factorial invariance of the value dimensions defined between generations. Further, there are some differences among the three generations in location on these dimensions, particularly on Collectivism/Individualism, but there is also substantial within-generation heterogeneity. Finally, there is slight evidence of within-family covariation. We now turn to a more direct assessment of the generation and family antecedents of value orientations. This will be examined in a comparative context: how much within-family influence, relative to inter-generational factors, can be seen in predicting global values? This question is of special interest with respect to Collectivism/Individualism orientations where both effects appear significant.

To explore this issue, the blocked analysis-of-variance design described earlier (see Table 2) was employed. But to further assess the degree of influence of the two effects on each parent-child grouping, the 256 triads were again decomposed into three two-generation dyads (as in the regression analysis) and three two-way ANOVAs were performed. In this blocked design, large net differences between families may be interpreted as high lineage similarity. Moreover, this represents a method of assessing the intra-family agreement issue described earlier (as opposed to covariation or prediction in the regression model), since the variance estimate is an analog to intra-class correlation (for a discussion of measures of agreement see Robinson, 1957). The question to be asked is: of the variance in value orientations for the sample, how much is attributable to the fact that individuals are members of different lineages (the family effect) and how much to the fact they are members of different age groups (the generation effect)?[9]

Significance levels of main effects are the usual method for reporting results of data analyzed in analysis-of-variance designs. However, a more direct and potentially informative method of interpretation involves the use of variance components estimates (Vaughan and Corballis, 1969; Kirk, 1972). In the present case, explained variance estimates are obtained for family (lineage), generation (age group) and for the residual (unexplained)

[9] I am indebted to Alan Acock for suggesting this procedure.

variance. These, of course, are additive. The estimates of these variance components treat the family component as a random effect (as in intra-class correlation) and the generation component as a fixed effect (as in omega squared). Computations are based on formulae found in Vaughan and Corballis (1969: 266-7).

Results of this procedure are presented in Table 3. The values in this table are similar to the analysis of variance summary in Table 2 except that (a) the unit of analysis is three sets of parent-child pairs for each value, rather than one set of three-generation dyads and (b) the complete ANOVA summary table of the six comparisons has been omitted. Table 3 simply presents the variance components estimates with indication of which main effects are statistically significant.

First, note the small to moderate percentage of total variance accounted by *either* main effect of cohort or lineage. The last column of Table 3 indicates the residual range is from 62 to 100 per cent in the various dyads. There is clearly a high degree of individual variance; neither inter-generation differences nor intra-family similarity are factors which consistently account for much variance on these dimensions of values over the six comparisons made.

A second finding concerns contrasts among the lineage dyads and between the two types of values in comparing the two effects. Of particular interest in the G2-G3 dyad is the greater importance of the family as contrasted with the generation effect. Comparison of the left and center columns indicates the estimate of variance for the family effect is 5% and 29% for the two value dimensions, while that attributable to generation is .4% and 4%, respectively. This suggests, particularly with regard to Collectivism/Individualism, relatively stronger family socialization effects than generational effects for youth and parents. The G1-G3 dyad, however, reflects a different pattern on this factor – 9% family effect, 29% generation effect. Again there is some suggestion of family transmission but in this instance substantially more generation influence.

Third, although the young and the old are as a group similar on Humanism/Materialism (see Figure 2), the similarity is not attributable to direct G1-G3 influence. And, although G1 and G3 as a group are markedly dissimilar on Collectivism/Individualism, there is still indication of a family effect. Finally, in the G1-G2 dyad, a moderate family effect can be noted on both dimensions (9% and 15%) in the presence of a moderate generation effect on Collectivism/Individualism (15%) but none on Humanism/Materialism (0.7%).

Thus although neither family nor generation effects consistently explain much variance in value orientations, it is on the

Table 3. Summary of Variance Estimates of Main Effects, Family and Generation Differences, from Analysis of Variance (256 Three-Generation Triads)

	Variance Attributable to Family (Per cent)	Variance Attributable to Generation (Per cent)	Unaccounted Variance (Individual or Residual) (Per cent)
1. G1-G2 dyad (grandparent-parent)			
Humanism/materialism	9.0	0.7*	90.3
Collectivism/individualism	15.0**	15.2**	69.8
2. G2-G3 dyad (parent-youth)			
Humanism/materialism	4.8	0.4	94.8
Collectivism/individualism	28.7***	4.1***	67.1
3. G1-G3 dyad (grandparent-youth)			
Humanism/materialism	0	0	100.0
Collectivism/individualism	9.3*	28.8***	61.9

* p from F-ratio < .05
** p from F-ratio < .01
*** p from F-ratio < .001

Collectivism/Individualism dimension where both the "gap" and the "transmission" most commonly is seen. As might be expected, the highest family similarity occurs between youth and their parents, and the largest generational dissimilarity is seen between youth and grandparents. On Humanism/Materialism, however, both family and generational influences appear slight.

CONCLUSIONS

The purpose of this analysis has been to test several implicit propositions in current socialization literature concerning generational contrasts and within-family similarities in global value orientations. The first is that there is a substantial "gap" in value dimensions between broadly-defined age groups. The second is that there is considerable transmission between parents and children in such orientations. A third and hitherto untested proposition concerns the relatively greater influence of family versus generational factors in accounting for variance in value orientations. By examining data from a three-generation sample and employing three statistical techniques (factor analysis, regression and analysis-of-variance), four conclusions can be drawn.

1. The evidence in general points to considerable value similarity between generations. Most important in terms of theory is the fact that each of the three generational groups demonstrated the same underlying dimensions in the operationalization of values (seen in the factorial invariance across generations). Second, although there was a statistically significant difference in the location of the generational groups along these dimensions, the variance explained by generation membership was not large. Third, within-cohort variability was substantial; the distribution of each generation overlaps the others. The proposition of a "great gap" in values between generations cannot, therefore, be supported.

2. Where there is indication of between-group differences in values, the pattern of those contrasts is somewhat unexpected in the light of previous research. For example, the youth and the elderly generations are at similar levels on the Humanism/Materialism dimension, with the middle-aged more Materialistic, suggesting a curvilinear trend by age-group. More expectable is the pronounced age progression on the Collectivism/Individualism dimension, where the youth are most Individualistic and the elderly most Collectivistic (traditional group oriented). Whether these differences can be interpreted as indicative of maturation (aging) or cohort (historical) effects is, in these cross-sectional data, open to question (Schaie, 1967; Riley et al., 1972). The more plausible explanation concerns life-cycle contrasts (see the discussion in Bengtson and Lovejoy, 1973). Certainly the middle-aged have greater financial responsibilities, while youth are more open to concerns reflected by individualism. But the same findings can be interpreted in terms of socio-historical changes experienced in young adulthood which may have lingering effects on these cohorts as they progress through adulthood (see, for example, Fendrich, 1974; Laufer and Bengtson, 1974; Bengtson and Starr, 1975). These data may reflect the romanticism of individualism, increasingly evident in each successive cohort (G2 as well as G3) which portend changes in broader cultural values in the next several decades. Of course, the design of the present study does not allow any direct test of "maturation" *vs* "cohort-historical" effects. However, the greater similarity of G2-G3 than G1-G2 lends credence to a cohort-historical interpretation of contrasts on the Collectivism/Individualism value dimension. (For similar findings see Hill et al., 1970.)

3. A proposition asserting direct within-family transmission of global values is at best only weakly supported, either in terms of covariation or agreement. On the one hand, the family effects generally are higher than the generational effects, implying that lineage influences may be greater than age influences in defining desirable ends. On the other hand, the variance explained is quite small. Moreover, on those comparisons where there are group similarities the parent-child similarity is also low. G1-G3 predictability is slight; little evidence exists in favor of family transmission through three generations in global value orientations.

4. The analysis demonstrated unanticipated contrasts between the two value orientations which have implications for characterizations of contemporary mass culture. On Humanism/Materialism (Factor 1), there was little group difference by generation and very little parent-child transmission indicated. On

Collectivism/Individualism, by contrast, there was both substantial generational contrast and greater within-family predictability. This emphasizes the importance of isolating specific value dimensions in inter-group comparisons. Of greater theoretical interest is the possibility that issues regarding Collectivism/Individualism have considerable salience with respect to both family socialization and cross-generation influences at this point in time. Put differently, these data may reflect *where* today's "generation gap" lies: on values relating to Collectivism/Individualism. This is in contrast to many portrayals of contemporary age-group cleavage (see Feuer, 1969; Roszak, 1969; Mead, 1970; Slater, 1970).

Age-group influences, defined either in terms of family or generational effects, appear in these data as minimal socialization factors in explaining value orientations. One general implication from the study, therefore, concerns the *substantial degree of individual variability* in value orientations. Such individuation suggests that many existing theories of value origins may have been cast in "over-socialized" perspectives. Wrong (1961:193) has pointed out that socialization means both the transmission of culture and the process of acquiring unique attributes. Core values, abstract desirable ends which orient an individual's conduct, appear highly variable from individual to individual depending on his or her unique personal biography. Although both generation and family influences are partial predictors, these data imply that neither predominates in the manner which deterministic models of various persuasions, from Parsonian to Marxist, have traditionally implied.[10]

But a second general interpretation concerns possible *convergence of family and generational influences* in the development of value orientations. Although direct family transmission of values may be slight, there is even less indication of overarching youth-adult contrasts or conflict in world-view. Such indications question the assumption characteristic of much "youth subculture" literature suggesting competing parent-peer influences on values (Feuer, 1969). In contrast to Coleman's argument that youth "... maintain only a few threads of connection with the outside society ... (and) the home has less and less ability to mold him" (Coleman, 1961:312), this research appears to support the argument of Kandel and Lesser (1972) regarding the mutual reinforcement of value orientations by parents and age-peers. The family serves as an important mediating link in selecting or orienting the child to the multiple reference groups to which he or she can turn for value development in a pluralistic society. Such similarity in social location may be more important than direct transmission (by tuition or by imitation) in achieving inter-cohort, rather than within-family, value congruence. It may well be that the younger and older generations develop their orientations "in parallel rather than series, by similar experiences in a common way of life" (Connell, 1972:330; see also Jennings and Niemi, 1975). Neither the family similarity nor the generational similarity indicated in this study *negate* the prospect of broader social change. For socialization must, after all, be regarded as a bilateral process: parents and children learn from each other, in the context of a continuously-changing society (Bengtson and Black, 1973).

In short, the findings of this study suggest neither (a) marked generational differences nor (b) strong familial similarity in value orientations. Thus generalizations concerning homogeneity within generational groups and their contrast to other age aggregates should be made most carefully, since empirical evidence often reveals the "generation gap" to be subtle if indeed it exists at all. By the same token, generalizations concerning family influences on the development of values should be made with caution: similarity between parents and youth reflects their commonality of social location rather than direct transmission. In any event, it is well to avoid the fallacy of an "over-socialized" perspective on the development of values. Global orientations may be more reflective of the individual's unique personal biography, or of his or her response to socio-historical events, than of effects attributable either to family or generational factors.

[10] This interpretation is supported by other analyses of these data in which neither social class, nor sex, nor personality variables predicted value orientation to any significant extent (Bengtson and Lovejoy, 1973). Such factors may, of course, account for some variation in value orientations, but not to the extent commonly postulated.

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EXPECTATIONS, EVALUATIONS AND EQUITY*

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One important reason for the inconsistent findings of the research on inequitably over-rewarded individuals is the failure to understand and control the process by which rewards come to be related to the formation of expectations about rank or position on the dimensions of evaluation which are the basis for the allocation of rewards in social systems. If individuals have well-defined expectations concerning position or rank on the dimension of evaluation which serves as the basis for allocating rewards, when reward-expectations are violated the individual will define the situation as unjust and will attempt to act equitably when allowed to distribute rewards. If, on the other hand, an individual's rank expectations are not clearly defined, rank expectations emerge which tend to be consistent with the actual distribution of rewards. In this case, individuals come to define the initial distribution by some third party as equitable, and when given the opportunity to allocate rewards they maintain the existing reward distribution, continuing to allocate more pay to themselves.

INTRODUCTION

The dominant concern of equity research over the past fourteen years has been to identify different reactions to inequity. The findings, however, have been inconsistent, especially in the case of reactions by over-rewarded individuals.[1] Several factors can be identified which contribute to this confusion: (1) methodological shortcomings in many of the tests of equity theory; (2) lack of comparability of findings and (3) incomplete formulations of the theory of inequity. This paper will show that the problematic aspects of both theory and method are due principally to difficulties in understanding the conditions that produce the equity process. Our task is to explicate these conditions and to incorporate them into the theory of inequity. Once these conditions are specified, it is possible to deal with many of the methodological shortcomings that have plagued equity research, to increase the comparability of the findings and to complete the theoretical formulation of the process.

In the Adams' experimental situation individuals typically increase their work output when overpaid on an hourly basis (cf. Adams and Rosenbaum, 1962), a result which has been interpreted as a reaction to inequity. The principal objection to this interpretation has been that the experimental manipulation used by Adams threatens the self-esteem of the subjects. Therefore, the result is due not to inequity but to a defense of self-esteem (Andrews and Valenzi, 1970), or to attempts to maintain prior perceptions of self-qualifications (Friedman and Goodman, 1967). In other words, some sort of self-evaluation process appears to operate in the Adams' situation, but it is unclear when the self-evaluation process occurs and how it is related to equity processes. Lack of comparability of findings, too, is largely a result of the failure to explicate precisely the nature of the equity process and to differentiate it from other

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[1] For a review of these findings see Pritchard, 1969; Goodman and Friedman, 1971 or Walster et al., 1973.

social processes. In addition, theories of inequity are incomplete primarily with respect to the conditions of the process and its relation to self-evaluation.

The two major theoretical formulations of the equity process are the exchange formulation (Homans, 1961; 1974; Adams, 1965) and the status-value formulation[2] (Berger, Zelditch, Anderson and Cohen, 1972). Both theories are concerned with the reactions of individuals to the ways in which valued outcomes or rewards are distributed in social systems, and the two theories have a number of common elements: (1) Rewards or outcomes are allocated on the basis of the possession of particular social characteristics (Berger, Zelditch, Anderson and Cohen, 1972), investments (Homans, 1961; 1974) or inputs to exchanges (Adams, 1965). (2) A justice rule (Homans, 1961; 1974; Adams, 1965) or referential structure (Berger, Zelditch, Anderson and Cohen, 1972) defines what is just or equitable. (3) A comparison process based upon the justice rule or referential structure determines the rewards individuals can legitimately expect to receive (Homans, 1961; 1974; Berger, Zelditch, Anderson and Cohen, 1972). (4) Violations of legitimate expectations result in psychological reactions (e.g., anger, dissatisfaction, guilt) and/or behavioral reactions that restore equity or justice.

The theoretical formulation presented in this paper incorporates all of these elements and adds one further important condition to the process: there must be not only a well-defined distribution rule which specifies how rewards should be allocated, but there must also be well-defined expectation states (cf. Berger et al., 1974). That is, individuals must know which states of the relevant dimensions of evaluation they possess in order to determine what level of reward to expect. (The importance of knowledge of individuals' ranks for determining the outcome of the justice process has been discussed recently from a different theoretical perspective by McCranie and Kimberly, 1973.) If this condition is not met, that is, if rank expectations are not well–defined, individuals do not react to the violation of expected rewards; instead they form expectations for self and others concerning the ranks they have on the relevant dimensions of evaluation. In contrast, if well-defined rank expectations exist and legitimate reward expectations are violated, inequity exists and psychological and/or behavioral reactions are predicted to occur. Only in the latter case can one expect to find consistent equity results.

Traditionally, equity research has focused upon situations in which distribution rules have been taken for granted as legitimate, and it is only their application that has been judged to be inequitable. It would be interesting, of course, to address questions concerning the legitimacy of the distribution rules (e.g., that workers with more seniority should receive higher salaries). However, in the present formulation we maintain the emphasis upon the inequitable application of accepted rules in order to specify more clearly the conditions under which the equity process emerges. The failure to distinguish between these two types of justice evaluations has obscured much that is important about equity processes. We hope eventually to extend the theoretical formulation presented here to include an examination of both types of justice problems. At the moment, the more important problem is that given the experimental findings to date the existence of an equity process or equity effect is still in doubt.

A Theory of the Process

The distribution of valued outcomes in social systems is based upon dimensions of evaluation which include achieved or ascribed traits such as seniority, skill, level of education, level of need, age, sex, etc. The specific relationship between a dimension of evaluation and the outcome or set of outcomes to be allocated in a particular system will be referred to as a distribution rule.[3] For example, the allocation of welfare checks in a community may be based solely upon an individual's level of demonstrated need. The

[2] In this formulation the status-significance of rewards is emphasized rather than the consummatory value of the rewards in an exchange process.

[3] This concept is more general than the "justice rule" as formulated by Homans and later adopted by Adams since the rule which states that inputs or investments must be *proportional* to profits for each party to a social exchange is only one possible distribution rule (cf. discussion of canons of distributive justice, Rescher, 1966:73).

"rule" specifies what levels of need are associated with particular levels or magnitudes of welfare payments. This information not only serves as the basis for reward expectations, but also, by specifying the appropriate dimensions of evaluation, defines which justice comparisons are relevant in that social system.[4] Distribution rules exist in every social system although they vary extensively with respect to their degree of formalization and their degree of perceived legitimacy.[5]

The "justness" of any particular distribution is determined by the comparison of the level of outcome one is allocated to the level of outcome one expects to receive based upon the distribution rule perceived to obtain in the situation. If the actual allocation is not in accord with these expectations, then the allocation is likely to be evaluated as unjust. In order to understand equity processes, it is important to understand precisely the nature of these expectations.

Scope Conditions of the Theory[6]

The theory applies to social situations which fulfill the following scope conditions. First, one or more socially *valued* outcomes[7] are allocated to individuals in the situation. Outcomes can be positively or negatively valued; that is, the outcome may be something which a person (p) desires or something which p wishes to avoid. If individuals are indifferent towards the outcome, it is unlikely that they will care about "just" or equitable distributions[8]. This condition implies that magnitude levels of the outcome must be differentially evaluated. For example, if the outcome is salary, we assume that a high salary is preferred over a low one.

The second condition specifies that there is a distribution rule which identifies the nature of the association between the levels of the dimension of evaluation and the levels of the outcome to be distributed in the situation.[9] The distribution rule identifies the relevant dimension of evaluation. Initially, consideration is limited to situations in which only one dimension of evaluation is relevant and one outcome is being distributed. The theoretical formulation deals with situations in which performance characteristics (e.g., skill level) serve as the dimension of evaluation. As Kimberly (1966; 1972) suggests, skill is not only a "prime determinant of behavior," but knowledge of skill rank is essential in determining whether a status consistency or a justice process is produced (McCranie and Kimberly, 1973). The theory presented here explains why knowledge of rank is an important determinant of equity processes.

The third scope condition is that the distribution rule which applies in the situation is perceived to be legitimate. Here, legitimacy refers to the extent to which the distribution rule is supported normatively in the social system. If the rule is not normatively supported, the reaction to the violation of the rule may result in different social processes.[10] The class of social situations which meet these conditions is referred to in the body of the theory as situation S and the theory is assumed to hold for all such situations.

[4] In addition, formal and informal norms exist in social systems which limit or constrain the comparison process. For example, in organizations some of these norms define the exchange of information concerning salaries as illegitimate.

[5] Many important social processes affect the emergence, legitimation, salience and maintenance of distribution "rules" within a social system. These processes will be the subject of future research.

[6] The specification of the scope of a theoretical formulation includes primarily a description of the social conditions under which the theory is assumed to hold. (See discussion of scope-defined formulations, Berger et al., 1974:15-6.)

[7] Homans (1961; 1974) uses the term "rewards" and Berger, Zelditch, Anderson and Cohen (1972) use the phrase "goal object." However, since both of these terms imply that the item is positively valued, we have chosen to use the more neutral term "outcome." The outcome can be something worthwhile or something which a person finds aversive. Bobrow (1971) suggests that justice evaluations are made of the distribution of punishments in society, not simply rewards.

[8] How objects get imbued with social value (in particular, status value) is an interesting problem and one that is addressed in the Berger, Zelditch, Anderson and Cohen (1972) status-value formulation.

[9] This condition will be relaxed in future research in which the task will be to determine how individuals develop or form expectations for outcome possession in the absence of a distribution rule.

[10] An "illegitimate" rule would be one which obtains little or no normative support from members of the system. The cries of injustice which arise over the application of what is perceived to be an inappropriate or illegitimate distribution rule (e.g., particularism or sexism) in determining the allocation of outcomes have been ignored in the experimental work on inequity and provide fertile ground for future research activity.

ASSUMPTIONS, DEFINITIONS AND DERIVATIONS

The first two theoretical assumptions deal with the formation of expectations concerning the possession of outcomes in S. In order to simplify the presentation of these theoretical assumptions, the following notation has been adopted:

D	=	dimension of evaluation
O	=	outcome
m, n, i, j	=	indices
p	=	focal person in S
o	=	other person in S

Equity evaluations depend upon the existence of well-formulated expectations of the proper allocation of outcomes. In order for an individual to know which outcome an actor should possess, he must not only know the distribution rule, but *also* the positions or ranks of the individuals on the evaluative dimension. This information is necessary for p to arrive at an equity judgment concerning the distribution of outcomes.

Assumption 1.1

If p believes that an actor possesses a specific level of

D_i, (D_{mi}) (where D_{mi} refers to level m of dimension i), then p *expects* that actor to possess the level of

O_j, (O_{nj}), associated with D_{mi}.

Assumption 1.1 states that p will expect any actor in S to receive the outcome he "deserves" by virtue of his position on the dimension of evaluation specified by the distribution rule to be the legitimate basis for allocation. These expectations include *both* an anticipatory and a normative aspect; p not only anticipates that the actor (p or o) will receive a specific outcome, but believes that the actor *should* receive it.[11] It is the violation of this imperative, not a simple anticipation, which results in distributive injustice. The violation of an anticipation might result in dissatisfaction, but not the moral outrage which typically accompanies cries of injustice.

If an individual does *not* know his position on the dimension of evaluation, he has no basis upon which to form definite expectations concerning the just allocation of outcomes. In fact, a different social process, not initially an equity process, comes into play in this condition. This process is referred to as a self-evaluation process which we believe to be peculiar to situations in which performance criteria serve as the dimension of evaluation.[12] The process is described formally in the next two assumptions; Assumption 1.2 identifies the first stage of the process.

Assumption 1.2

If p initially holds no beliefs concerning the level of D_i that an actor possesses and that actor is allocated O_{nj}, then p will expect that actor to possess D_{mi}, which is associated with O_{nj} in S.

The situation described in this assumption is referred to as the "undefined" condition in contrast to the well-defined situation described in Assumption 1.1. In the absence of information concerning the relative ranks of the actors in S, Assumption 1.2 implies that p will expect any particular actor to occupy the position on the dimension of evaluation consistent with the level of outcome he is allocated. This is a status consistency or rank balance notion; reward rank should be consistent with rank on the relevant dimension of evaluation. It is precisely this process which we have argued is to some extent responsible for confounding the results obtained in the Adams' studies and various other equity experiments.

Given that p has formulated some expectancy concerning an actor's position on the dimension of evaluation salient in S, in the next stage of the process p will develop beliefs consistent with these expectancies.

Assumption 2.1

If p initially holds no beliefs concerning the level of D_i that an actor possesses and p expects that actor to possess D_{mi}, then p will come to believe that the actor possesses D_{mi}.

There are several possible ways in which one could formulate the process by which p "comes to believe" that he possesses

[11] Assumption 1.1 is not intended to specify the exact process of expectation formation; this is dealt with more completely in the Berger, Zelditch, Anderson and Cohen (1972) status-value formulation.

[12] The majority of the studies on inequity have used performance characteristics (e.g., level of skill) as the basis for the equitable or inequitable distribution of pay.

D_{mi}. Whatever the mechanism involved, it is clear that the likelihood beliefs will form concerning actors' ranks on D_i and the appropriate allocation of outcomes is lower (or the rank expectations are weaker) for those individuals in the undefined condition than for those in the defined condition who initially are given objective information about their ranks on D_i. We will refer to this as an auxiliary assumption in the theory since at this point more research is necessary before we can offer a formal assumption concerning this process.

Assumption 2.1 enables us to return to Assumption 1.1 and make the prediction that given the scope conditions specified in the theory, whether p initially knows the actors' positions on the dimension of evaluation or not, p will form expectations concerning the appropriate distribution of outcomes based on the information available. In the case in which p initially has information concerning actors' ranks on the relevant dimension of evaluation, the formation of reward expectations is straightforward. When this information is not available, initially a rank balance process operates such that p comes to form expectations for the appropriate distribution of outcomes based on their actual distribution. Kimberly (1972) refers to this process as the justification of the *status quo*. One would assume that this would be the case for those who stand to gain from this definition of the situation, but not for those who stand to lose. However, in the absence of objective evidence or some other information concerning relative evaluation, how can p arrive at an evaluation of himself other than that implied by the allocation of valued outcomes? The pressure which arises in social systems to use objective criteria as bases for differential treatment can be attributed to this process. Homans says, "Many labor unions have in effect come down on the side of seniority, partly because differences in seniority can be determined more objectively than can differences in skill, and so leave less room for argument" (1974:251).

In this context, the theoretical concept "equity" can be defined more precisely. A state of equity is said to exist if the actual allocation of outcomes coincides with p's beliefs about how they should be distributed in S. This general definition can be written as follows:

Definition 1.1

> *Equity*: Exists in S if the levels of outcome allocated to the actors are consistent with the outcome levels p expects the actors to possess.

If, on the other hand, the actual allocation is not in accord with p's expectations, then a state of *inequity* exists in S. There are two ways in which p's reward expectations can be violated: (a) an actor receives a less valued outcome than p expects or (b) an actor receives a more valued outcome than p expects. These conditions are referred to as inequity-advantage and inequity-disadvantage respectively.

Definition 1.2

> *Inequity-advantage*: Exists in S if the level of outcome an actor is allocated is greater in value than the level of outcome p expects that actor to possess.

Definition 1.3

> *Inequity-disadvantage:* Exists in S if the level of outcome an actor is allocated is less in value than the level of outcome p expects that actor to possess.

This distinction is important since research indicates that the type of inequity has significant implications for the nature of the reactions expected to occur. Furthermore, it should be noted that this theory allows for the situation in which p and o are both over-rewarded or both under-rewarded, a situation of "collective injustice" (cf. Berger, Zelditch, Anderson and Cohen, 1972), in contrast to exchange theories in which whenever p is over-rewarded o is necessarily under-rewarded or vice versa. The remaining assumptions in the theory relate to individuals' reactions to the allocation of outcomes in S. Current theories fail to identify the structural conditions which make one mode of inequity resolution more likely to occur than others. This problem has been addressed partially in recent experimental work (e.g., Burgess and Nielsen, 1974), but theoretical treatments remain inadequate. The attention of this study, therefore, is focused upon the specific conditions which lead to redressive behavior on the part of inequitably rewarded individuals. When p is the victim of inequity, reactions of anger or hostility and attempts to restore equity seem only "natural." That

victims sometimes fail to react, therefore, requires explanation. Similarly, when p is the beneficiary of an inequity, reactions of guilt and behavior oriented to restoring equity seem self-defeating and require explanation. Experiments by Blumstein and Weinstein (1969) and by Leventhal, Weiss and Long (1969) give us clues to the conditions which generate such "selfless" reactions. In these experiments, redressive behavior was observed in a zero-sum reward situation. Since p's gain was automatically o's loss, it can be hypothesized that p felt responsible, as the beneficiary, for o's deprivation, and for that reason attempted to redress the injustice in response to this "guilt." Conversely, since o's loss was p's gain, o might have held p responsible for his inequitable deprivation and thus might have engaged in redressive behavior in response to felt anger.

Whether or not responsibility for the inequity can be ascribed to some specific source has been identified as an important determinant of the reactions to inequity (Pritchard, 1969; Walster et al., 1973). Responsibility in this context refers to causal involvement in the inequitable situation. Certainly, the degree of actual involvement is an important variable. Analytically the situation is complicated by the fact that an individual (p) can be "responsible" for the inequity in one of several ways which previously have been confounded in equity research. One way in which p may be responsible is that p is the source of the inequity; that is, p has control over the allocation process and distributes outcomes inequitably. In this case, however, reciprocity may be confounded with equity considerations. Another way is that if some *third* party allocates the rewards, p may feel *indirectly* responsible for the inequity if his gain is at o's expense.

Two additional theoretical assumptions present arguments concerning ascription of responsibility as one structural determinant of p's response to inequity.

Assumption 3.1

> If p is in a state of inequity-advantage and o is in a state of inequity-disadvantage and p ascribes some measure of the responsibility for o's inequity to self, then there will be pressure upon p to restore equity in S.

According to this assumption, if p feels responsible for o's deprivation, there will be pressure for p to act to bring about an equitable situation.[13] The extent to which this pressure is self-imposed guilt or stems from anticipated external social sanctions, possibly even from o (cf. Anderson and Shelley, 1969), remains an open theoretical question. If p fails to engage in actions designed to restore equity, what o will do also remains an open question at this point.[14]

Assumption 3.2

> If p is in a state of inequity-disadvantage and o is in a state of inequity-disadvantage and p ascribes some measure of the responsibility for the inequity to o, then there will be pressure upon p to restore equity in S.

In this case, p holds someone else in the situation responsible for his deprivation and thus should seek restitution.[15]

In order to predict what behavior will result from the pressure to restore equity in S, the nature of the conditions which obtain in the situation in which the inequitable distribution of outcomes occurs must be specified. Of particular theoretical interest is the situation in which p becomes the allocator instead of some third party, q. In this case, p is given the opportunity to respond to q's method of distributing rewards.

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[13] One way of testing the validity of this argument would be to conduct a study in which no ascription of responsibility to a specific individual can be made. Pritchard (1969) suggests that if the inequity is created by some third party, not in a direct exchange relationship, an over-rewarded individual may feel that "it is unfortunate that the other individual is not rewarded as well as he is, but that is the fault of the System, not the Person's fault . . . ." Thus he should not feel guilty (Pritchard, 1969:208). This is clearly an issue of responsibility.

[14] R. Emerson has suggested that if p does assume responsibility, o will foster and support p's guilt, but will not himself attempt to redress inequity. By contrast, if p does not assume responsibility, it is more likely that o will take redressive action (if possible) partly *because* p has not done so.

[15] In this theoretical formulation, consideration is given to only two of the possible cases of inequity, the two symmetrical cases in which one person is in the inequity-advantage condition and the other in the inequity-disadvantage condition. Two additional classes of inequity situations will be considered in future theoretical work: (a) the situation in which only one party is in a state of inequity and (b) the situation in which both parties are in the same state of inequity, collective injustice.

The final two assumptions in the theory, referred to as "reaction" assumptions, specify what p's actions are likely to be when given the right to make allocation decisions.

Assumption 4.1

If there is pressure upon p to restore equity in S when p is given control over the allocation of outcomes, p will allocate outcomes so as to increase equity in S.

Assumption 4.2

If a state of equity exists in S when p is given control over the allocation of outcomes, p will act to maintain the existing distribution of outcomes.

This completes the formal theory. From the initial conditions, the definitions (1.1-1.3) and the assumptions (1.1-4.2), three primary derivations follow.

In situation S:

Derivation 1

Given: p believes that p and o possess specific levels of D_i and the allocation of outcomes is consistent with p's expectations about outcomes; when p is given control over the allocation of outcomes, p will act to maintain the existing distribution of outcomes.

Derivation 2

Given: p believes that p and o possess specific levels of D_i and the allocation of outcomes is not consistent with p's expectations and p ascribes some measure of responsibility for the inequity to self or other; when p is given control over the allocation of outcomes, p will allocate outcomes so as to increase equity in S.

Derivation 3

Given: p initially holds no beliefs concerning the positions of p and o on D_i and levels of the outcome are allocated to p and o; when p is given control over the allocation of outcomes, p will act to maintain the existing distribution of outcomes.

In the situation described in the first derivation, p has knowledge of the relative ranks or positions of p and o on the evaluative dimension which serves as the basis for the distribution of outcomes and the actual allocation is consistent with p's expectations. Given this situation, it is predicted that p will attempt to maintain the state of equity or distributive justice (Derivation 1). If the actual allocation is inconsistent with p's expectations for outcome possession, then p will attempt to restore equity given that p holds someone in the situation responsible for the inequity (either other if p suffers inequity-deprivation or self if p is the beneficiary of the inequity) and has the right to allocate outcomes (Derivation 2). In both situations, it is *clear* whether or not a legitimate distribution rule has been violated because of the information p has concerning the relative ranks of p (and/or o) on the evaluative dimension.

In the case in which p initially does not have this information, a more complicated process occurs. In this situation, the only information available to p is the relative distribution of outcomes to p and o. It is hypothesized that a balance or consistency process will operate and p will come to believe that the relative ranks of p and o on the evaluative dimension are represented by the actual allocation of outcomes. Thus p's rank expectations come to be consistent with the distribution of outcomes in S (cf. McCranie and Kimberly, 1973). According to Definition 1.1, a state of equity exists and p is predicted to attempt to maintain the *status quo*. As Homans (1974:257) suggests, in this situation it is likely for p to decide that his "investments and contributions aren't what they seem," or conversely p decides that his "investments and contributions" are more valuable than he had assumed.

The experiment described in the following section was designed to test the primary derivations of the theory.

THE EXPERIMENT

Three experimental conditions are included in this study: (1) defined-equity; (2) undefined-equity and (3) defined-inequity. Initially, consideration is limited to the two-person situation in which p and o are allocated an outcome on the basis of one dimension, D_i, an ability or skill. In the clearly *defined* conditions, p must know p and o's positions on D_i. In the undefined condition, p must not know p and o's positions on D_i. Two variations are of interest in the clearly defined cases: (a) the allocation of outcomes is consistent with p's expectations (equity) and (b) the allocation is

not consistent with p's expectations (inequity).

In this experiment, the assumptions concerning the ascription of responsibility were not directly tested.[16] To have done so would have entailed a considerably more complex and costly experimental design. In addition, it should be noted that the only conditions included are those in which the subjects receive more pay than their partners because these cases are the most interesting in terms of equity theory. Furthermore, it is the inequity-advantage or "over–reward" condition which has been the primary source of inconsistent findings.[17]

The following conditions must be met in order to perform an adequate test of the theory:

(a) the group must include at least two individuals, p and o;
(b) one outcome is allocated to p and o;[18]
(c) p values the outcome;
(d) p knows what dimension of evaluation is the basis for the allocation, a performance characteristic, D_i;
(e) p knows how D_i is related to the allocation of the outcome;
(f) p regards D_i as a legitimate basis for the allocation of the outcome.

Level of ability at a particular task is used as the dimension of evaluation in the experiment and money is used as the outcome. It is assumed that pay is valued by the participants in the study[19] and that ability is perceived to be a legitimate basis for the differential allocation of pay.

The Experimental Procedures

The experimental situation consists of three phases. In the first phase, performance expectations are manipulated by giving subjects an ability test. In the defined conditions, the subjects know their relative abilities at the task; in the undefined condition, they do not know their relative abilities (or relative ranks on D_i). During the second phase of the study, a third person defined as another student allocates pay to the subjects for working on a task similar to that performed during phase one. All subjects in the three conditions are allocated more pay than their partners. The "inequity" is created in the defined-inequity condition by the unequal allocation of payments in phase two to individuals with the same level of task ability. Equity in the defined-equity condition is created by the allocation of more pay to the individual with more ability.[20] During the third phase, the subjects are asked to take over the task of allocating payments. The allocation decisions the subjects make during this phase constitute the main source of data collected from the study. A brief description of the actual experimental sequence follows.

Upon arrival the subjects were asked to draw a number from an envelope and were told that the number they drew would determine the role each subject would perform in the study. They were not permitted to interact prior to the experiment. Only two subjects participated in each group, although they were led to believe that there were three students participating in the study. The third "subject" was actually a co-experimenter in an adjacent experimental room.[21]

[16] Anecdotal evidence would suggest, however, that in the inequity–advantage condition, if p does not feel responsible for o's deprivation (perhaps it is the fault of the "system") p will not redress inequity; rather, p will attempt to remain the beneficiary.

[17] In addition to research on inequitably underrewarded individuals, future work will include an expansion of the scope of the theory as well as an extension of the formulation to deal with additional theoretical problems. In particular, there are three problem areas: (a) the specification of the structural determinants of reactions other than redressive behavior to the types of equity situations described in the theory; (b) the examination of new situations in which multiple, possibly competing or inconsistent, distribution rules exist and (c) the analysis of situations in which the actual distribution rules perceived to apply in a given social situation are defined as unjust.

[18] There may be one or more levels of the outcome.

[19] This appears to be a reasonable assumption since the students volunteered to participate in the study for pay.

[20] In most of the previous research (e.g., Leventhal's research), equity and equality have been synonymous; that is, the equal allocation of pay is equitable. Since the equality norm is strong in our culture, it was our intent to determine the strength of the equity norm apart from the issue of equality. We asked under what conditions will individuals define as equitable the unequal allocation of pay to individuals having unequal ability levels.

[21] A third person was used to make the allocation decisions instead of one of the two subjects in order

In the first phase of the study, all three subjects were asked to take a test of their "contrast sensitivity ability." The test involved the visual judgment of slides. Each slide consisted of two rectangular patterns composed of different arrangements of smaller black and white rectangles. The problem for the subject was to determine whether the top pattern or the bottom one contained the greater area of white. The task was constructed to be ambiguous; that is, there are no objectively "correct" solutions to the task. Standardization of the task has been performed and provides evidence that the actual probability that either pattern will be selected as the correct response is .5 (cf. Ofshe, 1967). This task was selected for use in the study since prior experience with it indicates that subjects will believe any score they are given and it is difficult for them to determine objectively how they are performing on the task. It is also unlikely that subjects would have had previous experience with this task.[22]

The subjects were seated in individual booths to eliminate face–to–face interaction and communication was controlled by an interaction control machine. The phase one test consisted of fifteen items. Subjects indicated their answers to the slides and received feedback by means of buttons and lights on their consoles. At the end of phase one, subjects in the defined conditions were given score sheets indicating their test scores. Equal scores (9 out of 15 correct) were given to subjects in the defined-inequity condition. Unequal scores were given to subjects in the defined-equity condition. These subjects were told that they had received a high score (14 out of 15) and their partner had received a low score (5 out of 15). Since the test was presented as a highly accurate measure of ability, it was expected that subjects would develop performance expectations consistent with their test scores. Subjects in the undefined condition were not given test results at the end of phase one; instead, they were instructed that they would receive their scores at the end of the study. The main experimental manipulation involved the reporting of scores and score values; in fact, this constituted the only difference between the experimental conditions.

During phase one of the study, the third "student" was located in an adjacent room where the test was also being administered; however, this student's score was not reported to the subjects. An intercom arrangement allowed the isolated student to hear everything that was going on in the experimental room and she could communicate via intercom with the experimenter. The role of student #3 was explained in greater detail during the second phase of the study. The experimental instructions included the following rationale:

> In today's work session you will each have jobs to perform and you will be paid for the work you do. How the pay decisions are made and how the jobs are assigned will be different in the two parts of the work session. . . .
>
> Let me explain how the jobs are assigned. Since we did not know anything about you before the study began, the numbers you drew when you arrived were used to make the job assignments. Persons #1 and #2, in the work session we want you to use your contrast sensitivity ability to solve problems like the ones on the test. There are a total of 40 problems for you to work on. These problems are divided into two sets for the two parts of the work session; the first set contains 15 problems and the second set contains 25 problems. In the first part of the work session, Person #3 will have the job of evaluating the work you do and determining the pay you are to receive.

During phase two the subjects were asked to solve visual problems identical to those on the contrast sensitivity test. Person #3 was asked to make decisions about the pay Persons #1 and #2 should receive on each problem. In addition, Person #3 was told that she could see the task decisions for each subject and would be shown the same slides. She was given instructions to allocate a total of 40¢ on each problem to be divided in one of three ways:[23] (a) pay number one 30¢ and

to eliminate reciprocity as an alternative explanation of the findings. The situation is clearly more complicated when p is not only a party to the inequitable situation, but also the source of the inequity.

[22]Due to this feature of the task, subjects entered the experimental situation with no prior conceptions of how they would do on the contrast sensitivity problems.

[23]The mechanical limitations of the available equipment dictated that the design include only three alternative choices.

number two 10¢; (b) pay each 20¢ or (c) pay number two 30¢ and number one 10¢. The subjects made task decisions by pushing the appropriate buttons on their consoles, and these decisions were transmitted to Person #3's console. Person #3's payment decisions were recorded on each of the subject's consoles. These decisions were actually controlled by the experimenter. Each subject was told she had been given 30¢ and the other person 10¢ on every problem (with the exception of three problems on which the pay was divided equally to reduce suspicion). Thus each subject was put in the high reward condition and believed that the other person was in the low reward condition. At the end of phase two, Person #3 was dismissed by the experimenter and told to collect her pay from the lab secretary. The total amount of pay allocated each subject was recorded on a pay sheet and distributed to each subject.

In the last phase of the study, the subjects were instructed that they would have the responsibility of making the payment decisions for the work they performed in the absence of Person #3. They were told that at the end of the study each subject would draw a card from an envelope to decide whose payment decisions would determine the actual pay distribution for the third part of the study. In this way, each subject believed that her decisions might determine the final allocation of pay. This pay would be added to that which she would receive for phase two. The subjects made decisions on the last set of twenty–five contrast sensitivity problems. After making a task-decision, each subject had to decide how to divide the pay on each problem using one of the three pay alternatives. During the entire experiment, the host experimenter read the instructions to the subjects, answered questions about procedures and recorded all of the subjects' decisions.

At the end of each phase, the subjects were given questionnaires designed to determine the effectiveness of the experimental manipulations. Upon completion of the final questionnaire, each subject was interviewed individually to gather more information about the subjects' perceptions of the study, to debrief and to answer questions the students had concerning the experiment.

The subjects were all female students between the ages of 18 and 24 recruited from a junior college in California. They were told that they would be paid for their participation, but not how much they would receive. The students were assigned randomly to one of the three experimental conditions. It is customary to exclude from analysis the data of those subjects who fail to meet the scope conditions of the theory. However, since sufficient evidence was not available from the interviews concerning whether or not each subject met all three scope conditions of the theory, no exclusions were made on this basis in order to provide a conservative test of the theory.

Subjects were excluded for two reasons: (a) suspicion or (b) confusion about some critical aspect of the experiment. Subjects were classified as suspicious only if there was clear indication in the interview responses that the students had detected the deceptions involved in the study. The data from sixteen subjects (18% of the sample) were excluded from analysis on the basis of suspicion. This suspicion rate is high, although not surprising, given the nature and amount of deception involved in the experiment and the fact that due to a limited supply of subjects it was not feasible to exclude students who had previously participated in deception experimentation. In addition, ten subjects (11% of sample) were excluded from analysis because they misunderstood crucial aspects of the experimental instructions. (For example, some students misinterpreted the labels on their award panels.) After these exclusions,[24] a total of sixty subjects remained, twenty subjects in each of the three experimental conditions. The predictions concerning the subjects' allocation responses and the results of the study are presented in the following section.

Predictions and Results

The theory presented here allows us to make specific predictions concerning the nature of the allocation decisions of the subjects in the three experimental conditions. According to the first derivation of the theory, subjects in the defined-equity condition should allocate more pay to themselves during the third phase of the study. Having received feedback that they have more ability

[24] There were approximately the same number of exclusions from each condition with the exception of the defined-equity condition in which there were fewer exclusions for suspicion, as one might expect.

at the experimental task, these students should define the unequal allocation of pay in their favor by Person #3 as just or equitable. The distribution of pay should be perceived as consistent with their ability expectations and the distribution rule which implies that pay should be allocated on the basis of ability.

In contrast, subjects in the defined-inequity condition should define the unequal distribution of pay to individuals with equal ability as unfair. In this case, subjects who know they do not have any more ability than their partners should feel that it is unfair to receive more pay than their partners receive. In the third phase of the study, the theory would predict that these subjects will distribute the pay more equitably than did Person #3. In fact, it is anticipated that if they assume some responsibility for the inequity created during phase two of the study, they will attempt to redress inequity by allocating less pay to themselves.

The most informative case is the undefined-equity condition. For this condition, the theory predicts that the allocation behavior of these subjects will approximate that of the subjects in the defined-equity condition. Given lack of information concerning ability levels, it is predicted that subjects in the undefined-equity condition will come to believe that they have more ability at the task because they have been allocated more pay by Person #3. Assuming that these ability expectations develop during the course of phase two, these subjects are expected to allocate more pay to themselves during phase three. Although their behavior will be similar to that of subjects in the defined-equity condition, it is expected that subjects in the undefined-equity condition will allocate less to themselves. According to the auxiliary assumption in the theory, the expectations predicted to form in this condition are less likely to form; and if they do form, they are likely to be weaker than those formed in the situation in which ability information is initially given to the subjects. More research on this expectation formation process is required before it is possible to estimate precisely the strength of these expectations and their exact effect upon allocation behavior. In summary, the predicted ordering of the conditions with respect to the amount of pay subjects will allocate to themselves is:

Table 1. The Number of Subjects Who Reported Equal or Unequal Ability Beliefs for Self and Other at the End of Phase I

Experimental Condition	N	Ability Beliefs p = o (=)	p > o (+ -)	p < o (- +)
1. Defined-Equity	20	1	19	0
2. Undefined-Equity	20	7	0	13
3. Defined-Inequity	20	19	0	1

Condition 1 > Condition 2 > Condition 3
(Defined-Equity) (Undefined-Equity) (Defined-Inequity)

Effectiveness of the Experimental Manipulations

The first phase of the study was designed to manipulate the perceived ability beliefs of the subjects. In the defined-equity condition, the subjects were given high scores on the contrast sensitivity test and were told that their partners had received low scores in order to create differential ability beliefs. The success of the manipulation can be assessed by evaluating the results of one of the questionnaire items presented in Table 1. An analysis of the first row of this table clearly indicates that the ability manipulation did have the intended effect. Subjects in the defined-inequity condition were told that they had received the same score (9 out of 15 correct) on the test as their partners. Data in row three of Table 1 indicate that the subjects believed that they and their partners were equal in task ability at the end of phase one of the study.

Subjects in the undefined-equity condition were not given test scores.[25] Since the task

[25] One variant of this experiment would be to omit phase one for subjects in the uncertain condition. This alternative was rejected in an effort to make the three experimental conditions as comparable as possible, as well as to eliminate the possibility that the data might be explained by the simple omission of a phase one test manipulation and not by the theoretical explanation of interest.

was ambiguous, it was expected that these subjects would have no specific expectations for their own or their partner's performance. However, on the phase one questionnaire the subjects did report differential ability evaluations, although sufficient information concerning the strength of these expectations is not available. The results are presented in row two of Table 1. An examination of this data provides evidence that 65% of the subjects in the undefined condition developed differential ability beliefs (low-high).[26] The remaining subjects reported equal ability beliefs.

In general, it seems that the ability manipulations were effective in creating the expected performance expectations. The problematic aspect of the manipulation was that subjects in the undefined condition may have started phase two with low-high ability beliefs instead of having no specific performance expectations. The success of the phase two manipulation for the defined conditions was contingent upon the stability of the ability beliefs established as a result of the first phase.

The second phase of the study was identical for all three experimental conditions. The subjects received payments for each contrast sensitivity problem they worked on. On all but three of the 15 problems, the subjects were told that they had received more pay than their partners. At the end of phase two, the total amount of pay was calculated and the subjects were told that they had been allocated $4.20 and their partners had been allocated $1.80.

In the defined-equity condition, if the subjects maintained their differential ability beliefs, it was expected that they would define the differential distribution of pay as "fair" or "equitable." In the defined-inequity condition, on the other hand, the subjects were expected to evaluate the differential distribution of pay as unfair given that they maintained their equal ability beliefs. Table 2 presents data on the ability beliefs reported at the end of phase two.

Table 2. The Number of Subjects Who Reported Equal or Unequal Ability Beliefs for Self and Other at the End of Phase II

Experimental Condition	N	Ability Beliefs p = o (=)	p > o (+ =)	p < o (- +)
1. Defined-Equity	20	1	19	0
2. Undefined-Equity	20	4	14	2
3. Defined-Inequity	20	10	10	0

The results shown in Table 2 for the defined-equity condition indicate the stability of the ability beliefs over time when compared to the data in Table 1. The results for the subjects in the defined-inequity condition in Table 2 indicate some change in beliefs when compared to the phase one data reported in Table 1. Approximately one-half of the subjects shifted from equal ability beliefs to unequal ability beliefs (high-low).

Additional data tends to suggest that the perceived ability differences are relatively small. When asked to estimate the number of test items answered correctly during phase two, the ten subjects with high-low (+ –) ability expectations guessed that they made an average of 16.5 correct responses out of 25 items and that their partners made an average of 14.9 correct responses. Possible explanations for this shift in expectations are presented in the discussion of the experimental results which follows.

Since the subjects in the undefined condition had no definite basis for the formation of ability beliefs (i.e., no feedback in phase one or phase two), it was predicted that they would come to develop differential ability beliefs consistent with the unequal distribution of pay in phase two. The results reported in Table 2 support this prediction. Even more striking evidence is obtained when one compares the results in Table 2 with those presented in Table 1 for this condition. The

[26] There are several possible explanations for this finding. Subjects reported the task to be fairly difficult, thus the perceived difficulty of the task might have led to low ability expectations for self. The use of a less ambiguous task might solve this particular problem, although it would create others. In addition, many of the subjects in this junior college population reported generalized low self-expectations. The fact that the study was conducted at a prestigious university seemed to accentuate this effect.

Table 3. Mean and Variance of the Allocation Responses to Self

Experimental Condition	Number of Subjects	Allocation Responses	
		Mean	Variance
1. Defined-Equity	20	$5.76	.615
2. Undefined-Equity	20	$5.50	.493
3. Defined-Inequity	20	$4.97	.150

number of subjects who reported that they had more ability than their partners increased from zero at the end of phase one to fourteen at the end of phase two.

Discussion of the Results

The major experimental results are presented in Table 3 which includes the mean pay allocations to self and the variance of these allocation responses for the subjects in each condition. The allocation responses are the sum of each subject's pay decision on the twenty-five problems presented during phase three of the study. An analysis of the mean allocations to self listed in column two of the table support the primary predictions of the theory and the predicted ordering of the conditions.

In order to evaluate the magnitude of the differences in the mean pay allocations to self across conditions, the Mann-Whitney U test was performed on each pair of conditions. In addition, the Jonckheere (1954) test was used to evaluate the predicted ordering of the conditions. These test statistics are reported in Table 4. Two of the three predicted comparisons between pairs of conditions achieve significance beyond the customary .05 level (one-tailed). The third is in the predicted direction, although not significant at the .05 level. The predicted overall ordering of the conditions presented in row 4 of the table is significant beyond the .05 level. The data provide clear support for the primary ordering derivation of the theory.

The fact that the difference in allocation responses between the defined-equity and the undefined-equity conditions does not achieve significance, and that there is a marked shift in expectations for the majority of the subjects in the undefined condition from low-high at the end of phase one to high-low for self and other in phase two (cf. results presented in Tables 1 and 2) gives some indication that the expectation formation process described in Assumptions 1.1 and 1.2 is an important process and deserves further attention. This data also indicate that additional work on refining the auxiliary assumption is necessary.

In order to analyze the trends in the data over time, the twenty-five allocation responses for each subject were arbitrarily divided into blocks of five trials. Figure 1 presents a graph of the mean allocation response curve through time for subjects in each condition. Because

Table 4. Results of Statistical Tests of Differences between Conditions

Predicted Relation of Conditions	Test Statistics		
	U	z	p
1. Defined Equity > Undefined Equity	172.0	- .76	.22
2. Undefined Equity > Defined Inequity	92.5	-2.92	<.01
3. Defined Equity > Defined Inequity	82.5	-3.21	<.001
4. Defined Equity > Undefined Equity > Defined Inequity		-3.43	<.001

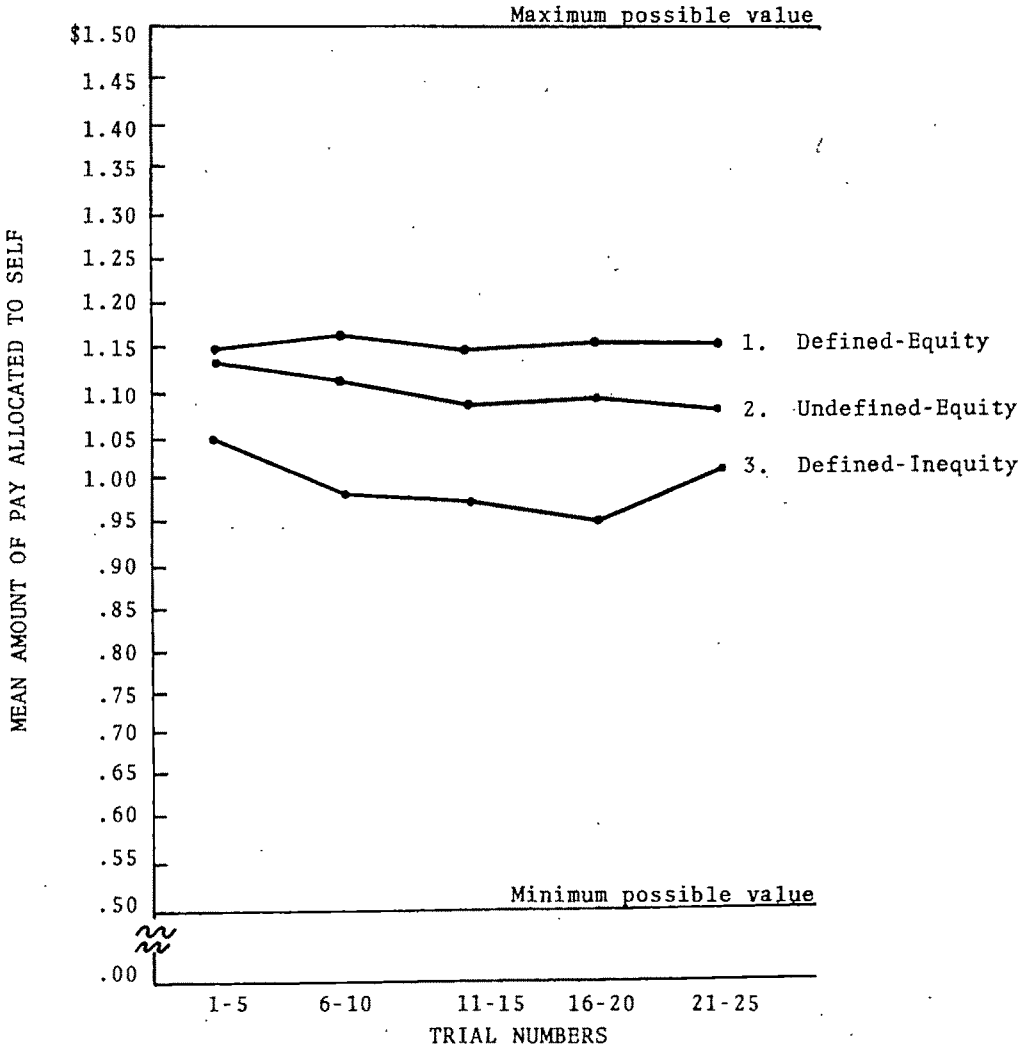

Figure 1. Mean allocations to self over time (by blocks of five trials)

each subject could allocate 10¢, 20¢ or 30¢ to herself on each trial, the possible range of responses for any block of five trials is a minimum of 50¢ and a maximum of $1.50. The response curves are remarkably stable, indicating little change in behavior through time. The ordering predicted for the total allocations to self should be apparent in the first block of trials and maintained through time. The predicted ordering of the three conditions is observed to persist over time (condition 1 > condition 2 > condition 3).

In the defined-equity and undefined-equity conditions, as predicted, the subjects allocated pay in their own favor. The stability and similarity of the allocation responses made by the subjects in these two conditions give some indication that the performance expectations predicted to form in the undefined condition are well-formulated prior to the initiation of the response process in phase three of the experiment. As predicted, the only difference in the allocation responses between these two conditions is a difference in magnitude or a simple linear transformation of the mean.

One of the interesting aspects of the findings is that the redressive behavior in the defined-inequity condition was not as signifi-

cant as anticipated. One reason for this is that a subset of the subjects[27] in this condition tended to allocate in their own favor. This result can be attributed in part to the shift in ability expectations noted in Table 2 which occurred among half of the subjects in this condition. Even though nineteen out of these twenty subjects reported at the end of phase one that they and their partners had equal ability, at the end of the second phase ten of the twenty indicated that they felt they had more ability than their partners. Given that the subjects did not have objective evidence concerning their performance during phase two, this finding suggests that the rewards may have been perceived by the subjects as implying a differential evaluation of their performance by Person #3 which affected their perceived expectations. This would have reduced the extent to which these subjects defined the differential allocation of outcomes as inequitable. It appears that degree of external social support for performance expectations is clearly a factor to be taken into account in future modifications of the theory.

Another possible explanation for the weakness of the redressive tendency has to do with the ascription of responsibility. During the post-experimental interviews, some of the subjects in the defined-inequity condition indicated that they had rejected responsibility for the inequity created in phase two of the study by the third person, thus they did not feel compelled to redress the inequity. Instead, they attempted to act "equitably" during phase three by allocating equal payments to subjects with equal ability. To redress the inequity created by Person #3 would have required them to act "inequitably," to allocate unequal payments to individuals having equal ability. This particular issue concerning the choice of response strategies is fundamental to current controversies over issues of distributive justice. For example, in the case of civil rights, society asks: Who is responsible for the past two hundred years of injustice? Does one attempt to develop just and fair practices to be implemented now, or is society responsible for making reparations, for "redressing" past inequities? Further consideration should be given to understanding the conditions under which subjects select one strategy over the other in response to inequity. The results of this study would seem to suggest that redressive behavior on the part of individuals who have benefited from an inequity, when it requires self-deprivation, is not as likely to occur as equity theorists once assumed. Furthermore, the ascription of responsibility for an inequity appears to be an important determinant of the nature of an individual's reactions to inequity or distributive injustice.

[27] Seven of the twenty subjects allocated more pay to themselves with a mean allocation to self of $5.34.

SUMMARY AND CONCLUSIONS

A review of the theories and research related to equity or distributive justice revealed that existing theoretical formulations are inadequate when it comes to specifying the conditions that lead to equity or justice processes. These theories do not specify the nature of the structural conditions that determine which responses to inequity are likely to occur. In order to deal with these problems, an initial theoretical formulation is offered which states clearly the conditions under which justice concerns are likely to emerge in social systems. The key concept, a distribution rule, specifies how the dimensions of evaluation in a social system (e.g., skill level, seniority, etc.) are related to the allocation of valued outcomes in that system. If the rule is perceived to be legitimate and if an individual knows his position (or that of any other member of the system) on the dimension of evaluation, then he can determine whether or not he (or any other actor) has been treated inequitably. If this individual's reward expectations based upon the legitimate distribution rule are violated, it is predicted that he will define the situation as unjust.

When the individual initially does not know the positions or ranks of the members of the system on the dimensions of evaluation, the theory makes the prediction that through the operation of a balance or rank consistency process the individual will form rank expectations consistent with the actual distribution of rewards or outcomes. Once these expectations have formed, the situation will be defined as equitable. In a sense this is what Homans (1961) referred to when he said, "What is, is always turning into what is right."

The theory states that whether or not an individual can ascribe responsibility for the

inequity to some specific source will affect his reactions to inequity. This investigation focused upon the situation in which one individual benefits from the inequity at the expense of another. It was predicted that if the beneficiary assumes some responsibility for the inequity-disadvantage experienced by another person then he will attempt to restore equity. When allowed to alter the distribution of rewards he should redress the inequity.

An experiment was designed to test the major derivations of the theory. The results provide support for the theoretical predictions. These findings indicate that whether or not a situation is defined as inequitable depends upon the amount of knowledge an individual has concerning the positions or ranks of persons in the same situation on the social dimensions which serve as bases for the distribution of valued outcomes, goods or services. In the absence of such information, the results from this study suggest that individuals do not enter states of inequity. Instead, they tend to assume that their position or rank on the dimension of evaluation is consistent with the level of outcome they have been allocated. Our findings support earlier notions in sociological literature of rank consistency (cf. Lenski, 1956; Jackson, 1962; Kimberly, 1972; Zelditch and Anderson, 1966) as well as the conclusions reached by McCranie and Kimberly (1973) that knowledge of ranks is an important determinant of reactions to inconsistency. The theory described in this paper offers an explanation of this process and its relationship to distributive justice. Furthermore, it offers one explanation for the frequently observed inaction of those one might consider to be inequitably treated. Unless a clearly defined situation exists in which there is some evidence that what is perceived to be a legitimate or normatively supported distribution rule is violated, it is unlikely that the psychological and social unrest often associated with distributive injustices in society will occur.

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REVITALIZATION MOVEMENTS AND SOCIAL STRUCTURE: SOME QUANTITATIVE TESTS*

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After reviewing some of the methodological difficulties which have faced investigators trying to investigate the rise of revitalization movements, the methodological advantages of studying the acceptance of the Ghost Dance (circa 1889) by North American Indians are delineated. These advantages are: (1) a relatively large number of tribes (N=37) were all exposed to what was more or less the same revitalization movement; (2) information on the degree to which each tribe accepted the movement is available from a source contemporary to the event and (3) information relating to some of the social characteristics of these tribes is available from Murdock's Ethnographic Atlas. *The sample thus provides what is probably a unique opportunity for quantitatively testing several hypotheses relating to the acceptance of revitalization movements. After ascertaining that diffusion alone could not account for acceptance of the Ghost Dance, several other hypotheses, derived from different theoretical frameworks, were tested. Thus, using a "cultural deprivation" argument, it was predicted, and found, that those tribes recently deprived (because they were living in areas in which the buffalo had only recently been exterminated) were far more likely to accept the Ghost Dance than those not recently deprived. The assertion—implicit in Worsley's analysis of Cargo Cults—that acceptance of a revitalization movement would vary inversely with degree of political centralization was* not *supported by the data. Finally, based upon a consideration of the social conditions promoting "integration" in Durkheim's sense of that word, it was predicted, and found, that acceptance varied inversely with the presence of unilineal kin groups and with the presence of a system of inheritance.*

Social scientists have long been interested in the rise of social movements whose purpose has been to radically re-structure virtually all aspects of social and cultural life. Such movements have been variously termed "messianic," "utopian," "millennarian," "nativistic," "revivalistic," etc. Wallace (1956) has convincingly argued that all such movements are concerned with a single goal, which he terms "revitalization." Simplifying Wallace's definition a bit, the defining characteristic of a revitalization movement is the emphasis upon a simultaneous restructuring of virtually all social and cultural relations. It is the emphasis upon this *gestalt* shift that distinguishes revitalization movements from other reformist movements that emphasize a program of more gradual change.

That so many movements, which have arisen in so many different cultures, should appear so similar has naturally led investigators to suggest the possibility that the rise of such movements is facilitated by the same general processes in all cases. The purpose of this paper is to review briefly some of the methodological difficulties facing investigators who have attempted to develop general theories explaining the rise of such movements, to focus upon a specific case—that of the Ghost Dance Religion in North America—which offers unique methodological advantages, and to use that case to test some of the hypotheses which attempt to explain the rise and acceptance of revitalization movements.

Methodological Problems

Attempts to investigate the rise of revitalization movements can generally be sorted into three categories. In the first would be placed those studies which try to explain

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the rise of particular movements in particular cultures (e.g., Weinstein, 1970; Simpson, 1970; Shih, 1967). A second mode of investigation is to survey a wide range of movements across a variety of cultures with an eye towards bringing out their common features (e.g., Wallace, 1956; Burridge, 1969). The third approach to the subject takes something of a methodological middle ground in that it restricts itself to a particular culture area (if not to a particular culture) and surveys a variety of movements that have all arisen in that area (the best known works of this sort being those by Worsley, 1959; Cohn, 1970).

Whatever the merits of these three approaches, it is clear that if the concern is to relate aspects of social structure to the acceptance of revitalization movements, each of the three contains some methodological flaw. Thus, for instance, those investigators who restrict attention to particular cultures cannot relate social structure to the acceptance of such movements simply because in their studies there is, by definition, no variation with respect to social structural variables. Further, while investigating a variety of movements in a variety of cultures does introduce variation with respect to social structure, investigators employing this mode of analysis can never rule out the possibility that variations with respect to the acceptance of the revitalization movements studied are due to variations in the doctrinal content of these movements. Finally, analyses of the Worsley and Cohn type exhibit to some degree both of these methodological flaws: by restricting their attention to a particular culture area, variation with regard to social structure is minimal and by surveying several different movements, it is not possible to control for the effects of doctrinal variation.

Ideally, if macro-sociological phenomena were as amenable to experimental manipulation as are certain types of physical phenomena, the methodological procedure for testing hypotheses which would be least subject to limitations would be easy to specify: a relatively large number of societies, which exhibited a wide range of variation along a number of social and economic characteristics, would all be exposed to the same revitalization movement (thus controlling for doctrinal content) and correlations could then be run between subsequent acceptance of the movements and those societal characteristics thought to be important in facilitating such acceptance. Such a procedure is of course impossible in principle, but there is at least one naturally occurring case that very closely approximates this experimental ideal and that case concerns the spread of the Ghost Dance Religion among the Indian tribes of North America at the end of the last century.

The Ghost Dance

The doctrine of the Ghost Dance was originally promulgated around 1889 by a Paviotso Indian named Wavoka, who lived in western Nevada. (The 1889 Ghost Dance is not to be confused with a similar movement that also arose in the same region in 1870.) The message of the doctrine formulated by Wavoka contained two key elements. First, he prophesied that all dead Indians were returning and would come to live in an earthly paradise, filled with all the game—notably the buffalo—that had been drastically reduced with the arrival of the white man. He also prophesied a cataclysm of some sort, from which the believers of the doctrine would be saved and in which all the others would perish. As the doctrine spread across the Plains certain modifications were made, or rather certain aspects of the doctrines were filled in with greater detail, but these two key elements remained more or less constant in all the versions. (Perhaps the most notable modification was introduced by the Sioux: whereas there are indications that Wavoka believed that the cataclysm would be a natural disaster, certain of the Sioux interpreted the prophesy of such a cataclysm as a call to military action against the whites.) The doctrine was invariably accompanied by a ritual—the Ghost Dance itself—whose purpose was to hasten the return of the dead, although here too there were some stylistic variations among the various tribes. Thus, although the experimental ideal of holding constant all aspects of the movement is not achieved, the constancy of the two ideological elements mentioned above and the invariable presence of some form of the Dance itself probably provides a reasonable approximation to this ideal.

The single most important source of information on the Ghost Dance is undeniably

that produced by James Mooney (1973; originally issued as a report to the Bureau of American Ethnology in 1896). Mooney was dispatched to investigate the Ghost Dance religion primarily because it was believed by some that the unrest among the Sioux (which culminated in the massacre at Wounded Knee) was in large measure brought about by their acceptance of this doctrine. Mooney travelled over 32,000 miles during the years 1890-1892, personally visiting over 20 tribes, gathering secondary information on many others and interviewing Wavoka himself. For present purposes it should be noted that Mooney—who himself was sympathetic to the contemporary plight of the Indians—provides some data about those tribes that heard about the doctrine *and,* from among these, those that accepted it and those that did not.

Further, it happens that most of the tribes mentioned by Mooney are also listed in Murdock's *Ethnographic Atlas* (1967) and that the "ethnographic present" (i.e., the approximate time level to which the ethnographic information coded in the Atlas refers) for most of these tribes is more or less contemporary with the 1889 Ghost Dance. Thus, assuming that we can correctly set up correspondences between the tribes mentioned by Mooney and the societies listed by Murdock, Mooney can present some information on the acceptance of the Ghost Dance by each tribe and Murdock on some of the social characteristics of that same tribe.

Constructing the Sample

Since it would make little sense to talk about "degree of acceptance of the Ghost Dance" with regard to those tribes that had never heard about the Dance, the first list to be constructed is a list of all those tribes that did possess an awareness of the Dance and its associated doctrine. Operationally, a tribe was classified as having known about the Dance if Mooney:

1. explicitly states this to have been the case;
2. states that the tribe had sent delegates to Wovoka to learn about the doctrine;
3. reports that the tribe in question was in regular contact with at least two other tribes that definitely did know about the Dance.

The use of these three coding rules yielded a list of 39 tribes. In order to stay as close as possible to *observable* measures (and thus hopefully minimize as much as possible the subjective biases that Mooney might have possessed with respect to the tribes that he commented upon), it was decided to operationalize "acceptance of the Ghost Dance" in terms of the extent to which the members of the tribe are recorded by Mooney as having participated in the *ritual* of the Ghost Dance itself. (An alternative index, for instance, might have been based upon Mooney's subjective estimates as to the degree that the members of the tribe "really believed" in the doctrine.) Accordingly, the following rules were used:

1. If Mooney reports that the entire tribe, or at least a majority of the tribe's members, performed the Dance, then that tribe was put in the "relatively *high* acceptance" category.
2. If Mooney definitely reports that the Dance was not taken up by any members of the tribe, or at best by a minority of its members, then that tribe was put into the "relatively *low* acceptance" category.

Of the 39 original tribes, only two could not be reasonably coded by this procedure. In these two cases (the Mohave and the Crow), Mooney (1973: 805, 814, 816) expresses some doubt as to the official estimates regarding the degree to which they either knew about, or participated in, the Dance, and thus these two cases were excluded from the sample. For the remaining 37 tribes, 12 were classified as having exhibited relatively low acceptance of the Ghost Dance, while 25 were classified as having exhibited relatively high acceptance.

Although Mooney's book remains the single most authoritative source for information relating to the Ghost Dance, subsequent work has been done (notably Lesser, 1933a: 53-81; La Barre, 1969; 1972) and the codings with respect to acceptance of the Ghost Dance were cross-checked with these later sources. In no case did these later works provide any information that would suggest that the codings used here are incorrect or misleading.

The last step is to establish correspondences between the tribes mentioned by Mooney and the societies mentioned by Murdock in the *Ethnographic Atlas* (1967).

This is complicated by the fact that Mooney sometimes refers to a single broad category (e.g., "Paiute," "Shoshoni") that could with equal validity refer to several of the societies listed by Murdock. Fortunately, however, Mooney also specifies the precise location of the tribes to which he is referring by locating them with respect to various reservations, agencies, forts, cities, etc. This information, together with the geographical coordinates provided by Murdock for each society, a map published by the Bureau of Indian Affairs in their Annual Report for 1889 and general reference works on North American Indians (Hodge, 1912; Wissler, 1950; Driver, 1969) were all used to establish the necessary correspondences.

The final sample, with the dichotomization of the tribes according to high and low acceptance, is given in the Appendix. The notations in parenthesis after each tribe refer to (1) the identification code number used by Murdock and (2) the "ethnographic present" for that society in the *Atlas*. (For all but four of the societies the ethnographic present falls between 1850 and 1890.)

Cultural Diffusion

Every researcher utilizing a cross-cultural sample to test his hypotheses must inevitably confront the problem of cultural diffusion. Simply put, the problem is whether obtained correlations result from causal relationships between the variables in question or from the diffusion of associated traits from a small number of cultural sources. This problem—typically called Galton's problem after the man who first explicitly formulated it—has been widely discussed (see Naroll, 1968:258-62 for a review of this discussion) and several solutions proposed. Most of the solutions assume that geographical proximity increases the likelihood of cultural diffusion and thus suggest that (1) some attempt be made to insure that societies in close proximity to each other are not included in the same sample and that (2) a given sample include societies from several different culture areas (see, for instance, the solution proposed by Murdock, 1967:3-6).

Unfortunately, the nature of the present sample precludes the use of any such solution. Presumably it is precisely because all the Indian tribes in the present sample were in reasonably close proximity to one another which explains how all the tribes learned about the Ghost Dance in the first place. Further, to include only one tribe from a given culture area (such as those listed by Murdock, 1967:8-45) would substantially reduce an already small sample of 37.

Nevertheless, since the source of the 1889 Ghost Dance—in western Nevada—is known from Mooney's account, it is possible to assess the role of cultural diffusion in explaining variation along the dependent variable in this analysis. In other words, while this whole analysis is predicated on the assumption that *knowledge* of the Ghost Dance did diffuse from this source, it can reasonably be asked if *acceptance* of the doctrine of the Ghost Dance also diffused.

This possibility can be investigated by correlating "acceptance of the Ghost Dance" with "distance from the source of the doctrine," which is operationally defined as the Walker River Reservation in western Nevada, near which Wavoka lived. To do this, the distance between the Walker River Reservation and each of the 37 tribes in the sample was calculated using the map that accompanies the Bureau of Indian Affairs Annual Report for 1889. (It must be remembered, however, that these distances are linear distances across the surface of the map and thus do not take into account those natural phenomena—mountains, rivers, etc.—that might hinder or facilitate cultural diffusion.) These distances for all the tribes were dichotomized at the median (the median distance being about 750 miles) and cross-tabulated with acceptance. The resulting distribution is presented in Table 1.

Table 1. Relationship between Tribe's Distance from Source of Ghost Dance (Walker River Reservation) and Acceptance

Distance from Source	Acceptance	
	High	Low
Greater than median distance	12	6
At or less than median distance	13	6

(Q = -.04, p = n.s.)

The data in Table 1 indicate that there is virtually no relation between distance from the Walker River Reservation and acceptance of the Ghost Dance (Q = –.04, p = n.s., using Fisher's Exact Test, one-tailed), and thus the hypothesis that simple diffusionary processes alone could account for such acceptance can be tentatively rejected.

Some Theories

There are two prevailing theoretical frameworks used in the study of revitalization movements: one focuses upon the rise of revitalization movements as resulting from feelings of cultural deprivation, while the other is primarily concerned with what Worsley (1957:227) has called the "integratory role" of such movements. This is, of course, not to say that these two approaches are either exclusive or exhaustive. Thus, at least one writer (Barber, 1941) combines both approaches by explaining the rise of the Ghost Dance as a response to the social disorganization brought about by widespread feelings of cultural deprivation. Alternately, certain investigators have utilized neither framework and have explained such movements in terms of pre-existing cultural traditions. Thus Spier in 1935 (quoted in Aberle, 1959:74-5) argued that the rise of the Ghost Dance was facilitated in large measure (though not totally) by the presence of indigenous, pre-contact traditions among certain Northwest tribes which emphasized cosmic regeneration. Nevertheless, because the "cultural deprivation" and "integratory role" approaches are those most used, the remainder of this analysis will be concerned with testing hypotheses derived from these two frameworks.

Cultural Deprivation

Perhaps the single variable most often used in formulations which attempt to explain the rise of revitalization movements is cultural deprivation. The basic idea is that the deprivation typically encountered by a culture in contact with a more powerful culture generates feelings of stress among the members of the weaker culture, and that the acceptance of the revitalization doctrine is a mechanism for coping with such stress. (See Wallace, 1956:268-9, for a more detailed version of this argument.)

For some authors, the important variable is simply absolute level of deprivation. For instance, in discussing the rise of the Ghost Dance, Barber (1941:664) argues:

> The general sociocultural situation that precipitates a messianic movement has been loosely described as one of "harsh times." Its specific characteristic is the widespread experience of "deprivation"—the despair caused by the inability to obtain what the culture has defined as the ordinary satisfactions of life. The fantasy-situation pictured in the messianic doctrine attracts adherents chiefly because it includes those things which formerly provided pleasure in life, the loss of which constitutes deprivation.

Other authors prefer to emphasize *relative* deprivation. The argument here is that a rapid change (for the worse) in the condition of a society leads the members of that society to compare their present deprived condition with the non-deprived condition of themselves, or, more generally, of their society, in the recent past, and that this comparison process generates feelings of relative deprivation. In other words, in this type of formulation, the absolute level of deprivation is less important than the perceived discrepancy between societal conditions in the recent past and in the present. (For a general consideration of the concept of relative deprivation as applied to revitalization movements, see Aberle, 1970.)

Deprivation theorists, however, have paid very little attention to the link that is crucial to their formulations: that persons experiencing deprivation—either absolute or relative—will turn to a revitalization movement. Such theorists seem to assume that deprived persons are likely to have an affinity for any doctrine—however far-fetched—that promises to raise their deprived status.

One of the few studies that does analyze the link between deprivation and revitalization movements is by Harris (1974). Harris essentially argues that, far from being irrational, those revitalization movements which are successful are successful only because they represent relatively rational attempts on the part of a subordinate group to redistribute the wealth held by a superordinate group. (Harris is specifically concerned with Cargo Cults and early Christianity.)

With regard specifically to the Ghost Dance, most of the investigators who have dealt with this movement have simply tended to attribute its rise to a condition of absolute deprivation, and virtually all have singled out for special consideration one particular factor as contributing to such a condition: the loss of the buffalo herds. This point of view is illustrated in the following passages:

> The Ghost Dance spread among American Indian tribes at a time when the final destruction of native culture was well advanced. Perhaps the greatest destructive influence was not so much the influence of white settlers or the consequent appropriation of tribal lands, as the annihilation of the great herds of buffalo. With the disappearance of the buffalo, the economic stability and security of the Indian tribes vanished. In its place came want and hunger. A feeling of desolation which spread among these tribes made them ripe for any message of hope. (Lesser, 1933b:109)
>
> Now, the killing off of the buffalo was probably the greatest blow to the Plains Indians. . . . In the aboriginal Sioux culture millions of buffalo furnished an unlimited supply of food. Buffalo and their by-products were perhaps the most important commodity in the Sioux economy, being employed as articles of exchange, as material for tepees, bedding, war shields and the like. In addition, the buffalo was the focal point of many ritual and social activities of the Sioux. When the buffaloes were destroyed, therefore, the Sioux were deprived not only of food, but also of many culturally significant activities. (Barber, 1941:664)

It is, however, not difficult to construct an argument which would lead one to expect that the loss of the buffalo herds would also result in relative—as well as absolute—deprivation. Thus, because the disappearance of the herds took place over a very short time span, an image of what things had been like in the recent past could easily survive in the memories of the various members of the various tribes in 1889. If tribes dependent upon the buffalo were to compare their present condition of absolute deprivation (due to the loss of the herds) with the situation in the recent past (in which the herds were plentiful), then they should in fact experience feelings of relative deprivation.

But whether one emphasizes absolute deprivation or relative deprivation, it is possible to derive the prediction that, all else being equal, those societies which had been dependent upon the buffalo would feel more deprived and thus would be more likely to have accepted the Ghost Dance than those not so dependent.

Each of the authors cited above tries to support this prediction by pointing to a single case in which both prior dependence upon the buffalo and acceptance of the Ghost Dance were high. (Lesser deals with the Pawnee, Barber with the Sioux.) Certainly the argument would be far stronger if it were supported by a larger number of cases, and the gathering of such support will be the goal of the remainder of this section.

Ideally, of course, the best test of the above arguments would be to relate a direct measure of the degree of deprivation experienced by each tribe in 1889 to acceptance of the Ghost Dance. Unfortunately, a direct measure of deprivation is not available. As was mentioned in the section on methodology, the approximate "ethnographic present" for the primary ethnographic sources for almost all of the societies in the present sample is *not* 1890, but instead falls generally in the years 1860-1880. The methodological assumption on which much of the analysis in this article will be based is that aspects of social structure are unlikely to have changed drastically between 1860 and 1890—but such an assumption simply cannot be made with regard to "deprivation." In other words, simply because a society is described to have been non-deprived in, say, 1870, can say nothing about whether that same society was deprived in 1890, simply because the condition causing deprivation—the extermination of the buffalo—occurred precisely in the years 1870-1890.

Another obvious hypothesis that suggests itself is the following: since the extermination of the buffalo would only lead to deprivation for those tribes dependent upon the buffalo, it should be the case that in 1889 those tribes which had been dependent upon the buffalo would be more likely to accept the Ghost Dance than those not so dependent (assuming, of course, that deprivation does increase the chances of acceptance). But this hypothesis too cannot be tested, simply because *all* the tribes in the present sample were buffalo

dependent. Thus, Roe (1970)—in what is probably the most authoritative work on the North American buffalo available—lists every tribe in the appendix as having been dependent upon the buffalo or upon buffalo products at some point in its history. Further, neither the Roe discussion nor the original ethnographic sources for each society, is precise enough to allow the coding of various degrees of dependence upon the buffalo.

Nevertheless, the deprivation arguments of Lesser and Barber (see above quotes) can be tested if the following assumption is accepted: the greater the length of time prior to 1889 that a tribe had to adjust to deprivation, that is, to the extermination of the buffalo in its area, the less will be the feeling of deprivation experienced by the tribe in 1889. If this assumption is combined with the assumption that feelings of deprivation increase the likelihood of accepting the Ghost Dance, the hypothesis becomes:

> Hypothesis 1. The greater the length of time prior to 1889 that a previously buffalo dependent tribe had had to adjust to the extermination of the buffalo in its area, the less likely is that tribe to have accepted the Ghost Dance.

In order to operationalize the independent variable in Hypothesis 1, it is necessary to briefly discuss the chronological stages characterizing the extermination of the buffalo in the United States, and this discussion will be based entirely upon the discussion given in Roe (1970).

By 1850, the buffalo had for all practical purposes been wiped out in all but two regions of the United States, these two being the northern Plains and the southern Plains. (The "Plains" roughly refers to the area lying between the Missouri River on the east and the Rocky Mountains on the west; the north/south dividing line for this area is generally taken to be the Platte River in southern Nebraska [Roe, 1970:447].) The buffalo on the southern Plains—the so-called "southern herd"—was exterminated during the years 1870-1876. To get a feeling for what "extermination" means in this context, consider that in 1871 the southern herd probably contained at least 3 million buffalo, while by 1876 that number was reduced to something like ten thousand (Roe, 1970:416-46). According to Roe (1970:447-66), the northern herd remained more or less intact until 1880 and was rapidly exterminated in the years 1880-1883.

Given this chronology, it is clear that those tribes living in proximity to the northern herd had had less time (by 1889) to adjust to the loss of the buffalo than those tribes living elsewhere; and thus, according to Hypothesis 1, those tribes living near the northern herd should have been the most likely to have accepted the Ghost Dance. Based upon the map accompanying the Bureau of Indian Affairs' Annual Report for 1889, and upon the geographical references made by Roe (1970:447-66) in describing the various segments of the northern herd, the following seven tribes can be said to have been living in proximity to that herd: Arikara, Assinboin, Gros Ventres, Hidatsa, Mandan, Teton (Sioux) and Wind River Shoshoni.

The cross-tabulation relevant to Hypothesis 1 is presented in Table 2, and it is both strong ($Q = +1.00$) and statistically significant ($p = .05$). (Due to the small sample size, Fisher's Exact Test will be used to determine statistical significance throughout the analysis; because the directionality of the relationship is in all cases predicted beforehand on the basis of theoretical arguments, the tests will be one-tailed.) In other words, tribes living near the northern herd—that is, tribes which presumably had had less time to adjust to the extermination of the buffalo—*were* more likely to have accepted the Ghost Dance than tribes living elsewhere.

In summary, the general argument that the extermination of the buffalo led to widespread feelings of deprivation, which in turn facilitated the rise of the Ghost Dance, has been modified. The modified theory assumes

Table 2. Relationship between Recency of Deprivation Occasioned by the Loss of the Buffalo and Acceptance of the Ghost Dance

Deprivation	Acceptance	
	High	Low
Recent	7	0
Not recent	18	12

($Q = +1.00$, $p = .05$)

that such extermination did lead to feelings of deprivation, but that such feelings of deprivation would diminish as the length of time between the extermination of the buffalo for a particular tribe and that tribe's encounter with the Ghost Dance in 1889 increased. Thus, it was expected that tribes living in the areas of relatively recent extermination of the buffalo would be more likely to accept the Ghost Dance than tribes living elsewhere—and this expectation is borne out by the data.

Social Integration

In the final chapter of his work on Cargo Cults, Worsley (1957) attempts to use his Melanesian data to develop a general theory of millenarian movements. Specifically, he argues that the main function of millenarian cults is to provide political integration to highly segmented societies brought into contact with a more dominant culture. In order to insure that the test to be constructed in this article is seen as a fair test of Worsley's arguments, it is best to let him state that argument in his own words:

> The cults generally occur among people divided into small, separate, narrow and isolated social units: the village, the clan, the tribe, the people of the valley, etc. They occur firstly, among people living in the so-called "stateless" societies, societies which have no over-all unity, which lack centralized political institutions, and which may lack specialized political institutions altogether. They have thus no suitable machinery through which they can act politically as a unified force when the occasion arises, except on a temporary, localized, or *ad hoc* basis. (Worsley, 1957:227)
>
> The main effect of a millenarian cult is to overcome these divisions and to weld previously hostile and separate groups together into a new unity. The social necessity which produces this drive towards integration is the subjugation of all the separate units to a common authority—the Europeans. (Worsley, 1957:228)

But as indicated earlier, Worsley's ability to empirically support this argument is limited in that, because virtually all the societies in his study were stateless, he could not document the second half of the theoretical proposition explicit in the above passages: that millenarian movements are *less* likely to appear in societies which possess a state.

Of course, given the common usage of the term "state," it is probably true that all of the Indian tribes in the present sample—like all of the societies considered by Worsley—would have to be termed "stateless." It thus must be conceded that Worsley's final conclusion—that revitalization movements are more likely to occur in stateless societies—cannot, strictly speaking, be tested with the present data. But an inspection of the reasoning that Worsley uses to arrive at this conclusion (summarized in the passages cited above) suggests an indirect test of his argument: Worsley seems to be saying that the probability of a society embracing a revitalization movement is *inversely* related to the degree of political centralization in that society, and it is only because societies possessing a state exhibit a relatively high degree of such centralization that he expects such movements to arise in stateless societies.

Still, even stateless societies can differ among themselves with respect to degree of political centralization; and thus, with respect to the societies in the present sample, a prediction derivable from the Worsley argument would seem to be:

> Hypothesis 2. The greater the degree of political centralization which characterized a tribe, the less likely would that tribe have been to have accepted the Ghost Dance.

The *Atlas* provides information relating to political centralization in columns 32 and 33. These two columns refer, respectively, to the number of jurisdictional levels up to and including the local community and the number of such levels beyond the local community. For present purposes, those societies which lacked any jurisdictional levels above the local community were considered relatively low in political centralization, while those societies possessing one or more such levels above the local community were considered relatively high. (The *Atlas* lacks information on this variable for one society, the Kickapoo.)

The data relevant to Hypothesis 2 is presented in Table 3. Not only is the correlation both weak and non-significant (Q = +.24), but it is in the wrong direction (it should have been negative if Hypothesis 2

Table 3. Relationship between Number of Jurisdictional Levels Above the Local Community and Acceptance of the Ghost Dance

No. of jurisdictional levels above local community	Acceptance	
	High	Low
one or more	12	4
none	13	7

(Q = +.24, p = n.s.)

were correct). Thus there seems to be no support for the link between acceptance of a revitalization movement and political centralization implied by the Worsley analysis. (That Worsley would consider the Ghost Dance to be a "millenarian" movement of the sort that he is studying in Melanesia is clear from his references to the Ghost Dance [Worsley, 1957: 244-5].)

"Integration" was used by Worsley to refer to the degree to which the social *groups* which compose a given society were tied together into a cohesive whole, but "integration" can also refer to something quite different, namely, the degree to which an *individual* feels a sense of solidarity with some social group. These differing usages of the same term are probably correlated with disciplinary background of the investigator. Thus anthropologists, especially those associated with what has loosely been called "alliance theory" (Buchler and Selby, 1968:105-50), are usually more concerned with the exchange relations existing among various societal subgroups and so likely to use the term in the former sense. Sociologists, on the other hand, following a pattern established by Durkheim in his *Division of Labor* (1961) and *Suicide* (1966), are more likely to use the term in the latter sense. It is the relation of "integration" in its Durkheimian sense—where the term refers to an individual's feeling of solidarity with a social group—to the acceptance of revitalization movements that will now be considered.

To my knowledge, few, if any, formulations have explicitly hypothesized that revitalization movements are a response to a lack of integration in Durkheim's sense of the word, although Barber (1941) in his claim that the Ghost Dance was a response to an undermining of the normative order probably comes the closest. In any case, it will be hypothesized here that the more the members of a society feel a sense of solidarity with any of the groups which compose the society, the *less* likely are the members of that society to accept a revitalization doctrine.

The conditions fostering such a sense of felt solidarity are many. For Durkheim, in his study of *Suicide* (1966), such a sense could be provided by membership in a variety of groups, ranging from a family (a premise he used to explain why married people had a lower suicide rate than single; [1966:180-9]) to organizations based upon occupational similarities (the establishment of such organizations being Durkheim's suggestion for increasing integration in modern societies [1966:378-84]).

But it is probably fair to say that among the Indian tribes in the present sample, one of the most important bases of social organization was kinship. Groups formed on the basis of kinship can be divided into two categories: unilineal kin groups and non-unilineal kin groups. Unilineal kin groups are those established by tracing descent through either the patrilineal (male) or through the matrilineal (female) line. Non-unilineal kin groups can be established in a variety of ways (cf. Zelditch, 1964:719-20) but always involve some element of bilateral descent, that is, the tracing of descent through both the male and the female line. There is an interesting structural property that to a certain degree distinguishes the use of a unilineal (as opposed to a bilateral) rule of descent: a unilineal descent rule produces a series of *discrete* kin groups, that is, kin groups with clearly defined boundaries and non-overlapping memberships (cf. Zelditch, 1964:718). (This is not to say that such non-overlapping kin groups are impossible to form using some form of bilateral descent, only that they are *easier* to establish and maintain using unilineal descent.) Furthermore, once established, the series of discrete kin groups formed by a unilineal descent rule are likely to take over a whole range of economic, social and religious functions (Fortes, 1953; Middleton and Tait, 1958:3-8), a condition that is likely to increase the felt solidarity of the members of that group (i.e.,

for Durkheim a set of values held in common by the members of a group was one of the primary determinants of solidarity in social groups).

A fair summary of the above argument would then be: all else being equal, the use of a unilineal descent rule (either patrilineal or matrilineal) is more likely to divide the society into a set of discrete, cohesive subgroups than the use of a non-unilineal rule. But if the presence of a unilineal descent rule does increase the number of discrete, cohesive kin groups composing the society then the use of such a rule should be *negatively* correlated with the acceptance of a revitalization doctrine (since it has been hypothesized that such acceptance is a response to a *lack* of cohesion, or integration, within the groups that compose a society). Restating this more formally:

> Hypothesis 3. Those societies which possessed unilineal descent groups should have been *less* likely to have accepted the Ghost Dance than those which did not possess unilineal descent groups.

Hypothesis 3 is especially interesting because one of the premises from which it is derived states that the presence of a large number of discrete, cohesive subgroups should *hinder* the development of a revitalization movement, whereas the passages previously quoted from Worsley's argument clearly indicate that he proposes just the opposite, namely that the "segmentation" produced by highly cohesive subgroups should *facilitate* the development of such movements.

In the *Atlas,* the presence of unilineal kin groups is indicated by the presence of any substantive code other than "O" or "E" in column 20 (for patrilineal kin groups) or column 22 (for matrilineal kin groups).

The cross-tabulation relevant to Hypothesis 3 is presented in Table 4, and the results are clearly supportive Q = -.68, p = .05).

But membership in a social group with commonly held values is not the only condition that promotes integration in a Durkheimian sense. For instance, it could be argued that the more a person feels a sense of continuity between himself and the deceased members of his kin group, the more he will feel a sense of solidarity with that group. But if this assumption (that such a feeling of continuity promotes integration) is combined with the previous argument (that the acceptance of a revitalization doctrine is inversely related to the amount of felt integration within a society), then it follows as a strictly logical consequence that the presence of such a feeling of continuity should also be negatively correlated with the acceptance of revitalization doctrines.

Table 4. Relationship between Presence of Unilineal Kin Groups and Acceptance of the Ghost Dance

Unilineal kin groups	Acceptance	
	High	Low
Present	4	6
Absent	21	6

(Q = -.68, p = .05)

One index that could conceivably be used as an operational measure of "degree of felt continuity with past generations" would be the presence or absence of an inheritance system. In other words, it seems reasonable to suppose that, all else being equal, the members of a society which possesses individual rights in property and rules governing the transmission of such rights across generations (that is, which possesses a system of inheritance) are more likely to feel a sense of continuity with past generations than the members of a society which does not possess such rights and such rules. Thus, in its final, operational form the hypothesis becomes:

> Hypothesis 4. Those societies which possessed some form of inheritance system should have been *less* likely to have accepted the Ghost Dance than those which did not possess any inheritance system.

The *Atlas* presents information on inheritance in column 74 (for real property) and in column 76 (for moveable property). For both columns, a code of "O" indicates the absence either of individual rights in property or rules governing the transmission of such rights. Thus, the presence of some form of inheritance system was inferred from the

presence of any substantive code other than "O" in either column 74 or column 76. The absence of an inheritance system was inferred from a code of "O" in both columns. Societies for which the code in one column was "O" and for which the other column was coded "no information" were excluded from the analysis.

The cross-tabulation between the presence of inheritance and acceptance of the Ghost Dance is presented in Table 5. The results are both in the expected direction and moderately strong (Q = –.60), although not statistically significant.

Brief Multivariate Considerations

Two hypotheses have received strong support from the data thus far: Hypothesis 1 which states that the more recent the deprivation experienced by a tribe as a result of the extermination of the buffalo, the greater the acceptance of the Ghost Dance and Hypothesis 3, which states that such acceptance should vary inversely with the presence of unilineal kin groups. Cross-tabulating the two independent variables involved in these hypotheses (that is, cross-tabulating "deprivation was recent/not recent" with "presence/absence of unilineal kin groups") indicates that these two variables are themselves not related (Q = +.04, p = n.s.; cross-tabulation not shown).

It is difficult to carry the investigation of possible relationships between these two independent variables much further, simply because, with respect to each of these variables, the cases in the present sample are overwhelmingly concentrated in one half of the dichotomy that defines the variable. In addition, there appears to be no reasonable way to change the cutting point for either variable so as to produce a more even marginal distribution (that is, it makes no sense to dichotomize unilineal kin groups other than as present or absent, and—apart from the seven tribes living in proximity to the northern herd—there is no other small subset of tribes that can reasonably be considered recently deprived). But given the highly skewed marginal distribution that exists for each variable, it very quickly happens in any multivariate analysis that some part of the analysis rests upon an extremely small number of cases.

Table 5. Relationship between Presence of an Inheritance System and Acceptance of the Ghost Dance

Inheritance system	Acceptance	
	High	Low
Present	5	5
Absent	12	3

(Q = -.60, p = n.s.)

Nevertheless, without minimizing the limitations involved, some sort of crude multivariate analysis might prove useful in the sense of suggesting hypotheses that might be investigated further in other contexts. In this connection, one of the first things to note is that all seven of the tribes that were recently deprived exhibited high acceptance of the Ghost Dance, despite the fact that two of these (the Hidatsa and the Mandan) possessed unilineal kin groups (which should have, according to Hypothesis 3, resulted in relatively low acceptance). On the other hand, eliminating these seven tribes from the sample—that is, considering only those 30 tribes not recently deprived—does not diminish the previously observed inverse relationship between the presence of unilineal kin groups and acceptance, as the correlation obtained with this revised sample is still strong and statistically significant (Q = -.78, p = .05, N = 30; cross-tabulation not shown).

Thus, if the above multivariate considerations—crude as they are—suggest anything at all, they suggest that "recent deprivation" and the "absence of unilineal kin groups" are each *sufficient* conditions for the acceptance of the Ghost Dance.

Summary and Conclusion

After constructing the initial sample and ascertaining that diffusionary processes alone could not to any significant degree explain the acceptance of the doctrine of the Ghost Dance, the analysis has proceeded to evaluate various hypotheses that have attempted to explain either the rise of revitalization movements in general or the Ghost Dance in particular.

Thus, using "cultural deprivation" argu-

ments, it was predicted that acceptance of the Ghost Dance should have been higher among those tribes most recently deprived as a result of the extermination of the buffalo, and this prediction was supported by the data. Further, although the absence of any societies possessing a state in the sample made it impossible to test Worsley's conclusion that revitalization movements ("millennial cults" in his terminology) are more likely to arise in stateless societies, his analysis did suggest that acceptance of such movements should be inversely related to degree of political centralization. This conclusion was not supported by the data.

Finally, an attempt was made to bring to bear on the Ghost Dance case a conceptual framework not usually applied to the study of revitalization movements. Thus, rather than see such movements as a response to a lack to integration in Worsley's sense of the word (referring to the integration of societal subgroups), these movements were interpreted as being a response to a lack of integration in Durkheim's sense of the word (referring to an individual's feeling of solidarity with a particular social group). Use of the Durkheimian perspective is somewhat distinctive in that it leads the investigator to seek out social conditions which promote feelings of integration and which therefore *inhibit* the rise of revitalization movements. More usually, the concern is just the opposite: the investigator seeks the conditions that would *promote* the rise of such movements. In any case, the fact that one of the two predictions was strongly supported by the data (this being the prediction that the presence of unilineal descent groups should have been inversely correlated with acceptance of the Ghost Dance) and the other moderately so (this being the prediction that the presence of an inheritance system should also be inversely correlated with acceptance), should at least suggest that this perspective might prove fruitful in future analyses of revitalization movements.

Apart from the specific hypotheses which have been derived and tested, an over-all goal has been to make more visible the methodological advantages of studying the Ghost Dance. This will hopefully encourage other investigators to develop quantitative tests of those theories dealing with revitalization movements that, to this point, have been developed on the basis of very small and relatively homogeneous sample of societies.

APPENDIX

Sample of Societies Used in the Analysis (with *Ethnographic Atlas* Code Number and Approximate Date of Ethnographic Present) by Level of Acceptance.

High Acceptance

Arapaho (Ne9; 1860)
Arikara (Ne10; 1860)
Assinboin (Nell; 1370)
Bannock (Nd63; 1860)
Caddo (Nf14; no date)
Chemehuevi (Nd54; 1860)
Cheyenne (Ne5; 1860)
Gosiute (Nd48; 1860)
Gros Ventre (Ne1; 1870)
Havasupai (Nd3; 1880)
Hidatsa (Ne15; 1860)
Kiowa (Ne17; 1860)
Kiowa-Apache (Ne2; 1870)
Mandan (Ne6; 1830)
Oto (Nf11; 1870)
Paiute, Pyramid Lake (Nd27; 1860)
Pawnee (Nf6; 1860)
Shoshoni, Wind River (Nd64; 1860)
Taos (Nh6; 1890)
Teton (Ne8; 1870)
Ute, Uintah (Nd58; 1860)
Ute, Uncompahgre (Nd62; 1860)
Walapai (Nd65; 1870)
Washo (Nd6; 1850)
Wichita (Nf5; 1860)

Low Acceptance

Commanche (Ne3; 1870)
Fox (Nf7; 1830)
Hopi (Nh18; 1920)
Jicarilla Apache (Nh16; 1870)
Kickapoo (Nf15; 1870)
Mescalero Apache (Nh15; 1870)
Navaho (Nh3; 1930)
Nez Perce (Nd20; 1850)
Omaha (Nf20; 1850)
Santee (Ne 20; 1850)
Southern Ute (Nd2; 1860)
Winnebago (Nf2; 1850)

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CONTEXTUAL EFFECTS IN THE HIGH SCHOOL ATTAINMENT PROCESS*

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This paper presents a multi-dimensional mediation model of composition influences. Ability and social status of student body composition are distinguished, and the interpersonal and social comparison processes by which their effects tend to offset each other are specified. Whereas the contextual effects of ability were found to be negative, those of social status were positive. Since the two contextual variables themselves are positively and strongly correlated, their effects are almost exactly offsetting in the long run. The model is examined on data for a national sample first surveyed in 1955 as high school sophomores and followed up in 1973.

Most past research on the effects of student body composition on educational processes is especially lacking in any sense of how aggregate characteristics of student bodies might actually influence educational outcomes.[1] Thus, the interpretation of contextual effects usually has been *ad hoc* and, as a consequence, often unpersuasive. Recently, however, a number of studies have begun to suggest the broad outlines of a model of contextual influences (Campbell and Alexander, 1965; Davis, 1966; Meyer, 1970). This report will integrate the findings of these studies into a single school process model of composition effects and evaluate its empirical utility using fifteen-year longitudinal data from the EEO survey of a national sample of youth, first surveyed in 1955 as high school sophomores and followed up in 1970 (Alexander and Eckland, 1973).[2]

In addition to problems of interpretation, the proportion of variance in educational outcomes commonly attributable to differences of any kind between schools has been relatively modest (e.g., see Coleman et al., 1966; Hauser, 1971; Jencks, 1972a; Smith, 1972) thereby precluding any dramatic effect of student composition, which of course is only one of numerous possible components of "between-school" differences (Hauser, 1969). Hence, in the aggregate, contextual variables will necessarily have small effects in the educational process. Recognizing that such influences are likely to be modest does not, however, mean that they must also be uninteresting or unimportant. On the con-

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[1] See Feldman and Newcomb (1969) for a thorough review of the literature and issues as they pertain to college environments. The recent study by McDill and Rigsby (1973) evaluates much of this research in elementary and secondary level institutions.

[2] The project has been designated "Explorations in Equality of Opportunity" and will be referred to by the initials "EEO" in the text.

trary, it may be essential to incorporate such influences into school process models if the complex dynamics of educational attainment are to be adequately understood.

A related problem, one by no means peculiar to the study of contextual effects in education, is that of interpretation in crossing from aggregate to individual units of analysis. It is a problem encountered whenever the research objectives are to explain differences among "naturally occurring" groups and to attribute causality to intercorrelated factors (the problem of multicollinearity). This difficulty is endemic to contextual research because composition effects are part of the between-groups effect in ANCOVA (Hauser, 1969; 1970).[3] Thus, we cannot unequivocally rule out alternative explanations for influences that we choose to interpret as contextual. Such alternatives could involve unmeasured individual level variables or structural characteristics of educational institutions.

The integrity of contextual effects can only be demonstrated within a causal model which controls for other theoretically relevant variables, both individual and structural,[4] in some hypothesized sequence of cause and effect. This will be our strategy, and, practically, it is the only appropriate strategy for the survey researcher, regardless of the level or levels of aggregation at which the problem is formulated.

We will proceed now to develop our substantive model of composition effects in educational institutions. As mentioned, it will combine insights from a number of separate sources. Finally, after outlining our research strategies, we will conclude by evaluating our model empirically.

THE CONTEXTUAL MODEL

The seemingly inconsistent conclusions[5] of the Coleman-Campbell Report that enrollment in high status schools depresses minority youths' academic self-conceptions while at the same time raising their absolute achievement (Coleman et al., 1966: 323-4 and 302-4) dramatically illustrates the need for refining the contextual effect model. Interpreting such results in terms of the "differential sensitivity" of deprived youth to the impact of educationally enhancing environments, as the authors did, may appropriately label these phenomena but does not explain them. Similarly, while it may indeed be fortunate that loss of self-esteem as a consequence of competition with more able students does not inhibit achievement gains, such knowledge sheds little light on the processes that determine these outcomes.[6] How, then, might aggregate student body characteristics, such as their status or ability composition, have such peculiarly different effects? The answer requires that such variables be incorporated into a model of educational attainment that explicitly identifies the intermediate linkages in that process.

In our model, contextual variables are treated as components of the institutional environment that affect the quality and character of more proximate interpersonal, subjective and structural determinants of school outcomes. The role of contextual variables in this process then is essentially an ecological one.[7]

Consider, first, Davis' (1966) "frog pond" or relative deprivation model of ability composition effects upon goal-orientations. Davis suggests that goal-orientations depend upon one's relative academic success, which is itself tempered by the collective competence of the student body. Simply put, a student

[3] Since contextual effects are but an element of the residual between-groups effects, sole reliance upon them as aggregate influences will underestimate the latter.

[4] The absence of such structural data and information regarding educational "hardware" and resources is a major limitation of the present inquiry.

[5] We are not interested at this point in assessing the validity of these conclusions. As is well known, the Coleman-Campbell report has received considerable critical attention (Bowles and Levine, 1968; Cain and Watts, 1970; Hanushek and Kain, 1972).

[6] Katz (1968) has provided us with a thoughtful consideration of these data and the processes that may underlie them. His interpretation is buttressed by the suggestive conclusions from his considerable experimental research.

[7] This conceptualization does not preclude more direct "climate" influences involving, principally, attributes of the normative milieu. See, in particular, the work of McDill and his associates (McDill and Rigsby, 1973; McDill, Rigsby and Meyers, 1969; McDill, Meyers and Rigsby, 1967).

with high ability in an academically selective setting tends to receive lower grades than he would if competing with less able students. Davis' conceptualization, then, implies a two-stage model of influence: with individual aptitude controlled, high ability contexts directly reduce one's relative performance; performance, in turn, is a major determinant of academic goals, evaluations of one's academic competence, and so forth.[8] Davis' conclusions, supported by a number of other studies (Drew and Astin, 1972; Meyer, 1970; Werts and Watley, 1969), are not inconsistent with those of the Coleman-Campbell Report because association and competition with more able students may actually enhance absolute academic achievement at the same time that it depresses relative performance, as measured by grades or class standing.

Although the "frog pond" model may account for the Coleman-Campbell results regarding self-conceptions, the positive consequences of enrollment in a high status school, which also are documented in a number of other studies (Boyle, 1966; Cleveland, 1962; Michael, 1961; Wilson, 1959), still remain to be explained.

Campbell and Alexander (1965) have proposed a two-stage process for such status influences that stresses the importance of interpersonal associations, with the school's status composition affecting the likelihood that any individual student will have high status or college-oriented friends. The former are likely to be in an academic curriculum (Heyns, 1974) and to have college plans regardless of any scholarly interest (Coleman, 1961). Peer influences, in turn, are known to affect the student's own academic orientation (Duncan et al., 1968; Herriott, 1963; Kandel and Lesser, 1969; Sewell and Hauser, 1972). Thus, the ability and social status dimensions of student body composition may both be hypothesized to affect self-conceptions and goal-setting largely indirectly through various interpersonal and social comparison processes *but in different directions.*[9]

A number of recent inquiries have recognized the possibility of such offsetting composition effects. Nelson (1972), for example, notes that high status levels depress performance scores but directly enhance educational plans. Drew and Astin (1972), on the other hand, attribute the same set of influences to school selectivity, or ability composition, in their college-level analysis. Neither of these inquiries, however, provides a reasonable model of the process to account for these offsetting influences. Moreover, the fact that each attributes the same consequences to different dimensions of student body composition suggests the need for clarification.

Our distinction between aptitude[10] and status dimensions of student body composition suggests a model with counterbalancing influences that would account for these inconsistencies: higher ability contexts modestly depress goal-setting, self-conceptions and perhaps actual attainments through lowering one's relative performance; higher status student bodies modestly elevate absolute achievement and goals through their implications for enrollment in college-oriented curricula and association with high achievement-oriented peers. Again, while such offsetting effects have been the subject of some speculation (Jencks, 1972b; Bain and Anderson, 1974) and inquiry (Drew and Astin, 1972; Hauser, 1969; Wilson, 1967), the model has yet to be fully developed and empirically assessed. We believe the closest anyone previously has come is Meyer (1970) who found that controlling for aggregate ability levels raises the correlation between average school social status level and college plans. This implies a suppressor effect for ability context corresponding exactly to the offsetting contextual effect attributed by Drew

[8] Davis' formulation assumes uniform grading distributions across schools. Employing class rank as our measure imposes this condition. In any event, assumptions regarding teacher behavior and grading policies are imbedded in this model. While formal evaluation procedures are certainly conceivable, that would perhaps invalidate this aspect of the model; more informal assessments of relative performance might have the same consequences.

[9] Unfortunately, the absence of absolute achievement data precludes a thorough evaluation of our interpretation of the Coleman-Campbell results. See Hauser (1971), Hauser (1969), McDill, Rigsby and Meyers (1969), McDill, Meyers and Rigsby (1967), and McDill and Rigsby (1973) for contextual analyses involving academic achievement dependent variables.

[10] The terms "aptitude" and "ability" will be used interchangeably throughout this report.

and Astin (1972) and Nelson (1972) to different dimensions of student body composition.[11]

The major features of our substantive model may be summarized as follows:

1. Ability and social status dimensions of student body composition should be distinguished, both analytically and empirically.

2. The consequences of each contextual variable for dependent variables, such as academic self-concept, educational expectations and their realization, are largely indirect, being mediated through more proximate determinants of these outcomes.

3. Ability composition operates largely through depressing relative performance scores, which, in turn, are major determinants of academic self-evaluation and college plans.

4. Social status composition affects the likelihood of being in a college preparatory curriculum and having college-oriented peers which, in turn, may affect academic performance and college plans.

5. Because the ability effect on performance is depressant and the status effect on peer plans is salutary, their influences will be largely offsetting.

The above hypotheses will be tested by incorporating measures of ability and status composition as additional exogenous variables into the school process model presented schematically in Figure 1. The model will be examined with path analysis, which is an appropriate technique for assessing composition effects (Feldman, 1970; 1971; Werts, 1968), and which, in this application, is identical to the statistical model of ANCOVA.

The model presented in Figure 1 is in the tradition of those developed by Sewell and his associates (Hauser, 1972; Sewell, Haller and Portes, 1969; Sewell, Haller and Ohlendorf, 1970; Sewell and Hauser, 1972). It is designed to explore some of the more interpersonal and subjective processes affecting educational attainment and their importance in mediating the effects of social origins and academic ability on more "tangible" status outcomes. Because we have treated the "context-free" version of this model elsewhere (Alexander and Eckland, 1973; 1974; and two forthcoming articles),[12] this report will focus mainly on the consequences of adding contextual variables to the analysis. The model will be analyzed with both males and females included, but with sex treated as a dummy variable. Any important sex differences or interactions will be noted in the text.

METHODOLOGY

The data set to be employed is part of a 1955 survey of public high school students conducted by the Educational Testing Service and a follow-up conducted by the Institute for Research in Social Science in 1970. The nationally representative sample originally included all seniors in 516 schools and all sophomores in 97 of these. The 1955 survey consisted of two major sections: a twenty item test of academic aptitude, measuring both verbal and mathematical ability, and a questionnaire eliciting self-reported information on such items as family background status, occupational and educational goals, and peer, parental and teacher influences on college plans. These data have already been employed in a number of inquiries (Meyer, 1970; Michael, 1961; Rogoff, 1962; Stice and Ekstrom, 1964).

The 1970 EEO follow-up survey was restricted primarily to the national sophomore sample. Schools constituted the basic sampling units. These were stratified by region and size, and to some extent by parental education, group test scores, and school dropout and college-going rates. The targetted sample consisted of 42 schools with 4,151 sophomores from all regions. More detailed descriptions of the EEO schools (Eckland and MacGillivary, 1972) and sampling procedures (Alexander and Eckland, 1973) are available elsewhere. Usable data were obtained for 2,077 of the 4,151 sophomores in the final sample, 1,130 females and 947 males. The fifty percent nonresponse consists of 32 percent refusals, 16 percent lost cases (for whom current addresses could not be located), and 2 percent known deceased. The

[11] While Hauser did obtain some such suppressor effects in his research (1971), Meyer's specific results were not duplicated.

[12] The analysis to be reported here is based on what are referred to as the unconstrained interitem correlations in these references.

major sample selection biases involve an underrepresentation of urban and large schools (partially an intended consequence of the sample stratification), and, not surprisingly, an underrepresentation of both low aptitude students and school dropouts. These sample biases, of course, temper the generalizability of results.[13]

In order to minimize sample attrition due to missing data on different items, procedures were employed for estimating missing data whenever it appeared empirically and logically sound to do so. Although the extent of missing data on individual items was generally moderate, usually in the 2-5 percent range, there was considerable independence in their occurrence across items and respondents. The cumulative effect of deleting all cases with missing data on any variable would have reduced our sample size by approximately 32 percent. By various estimation procedures, we were able to limit the loss to about 17 percent.

While no single strategy was relied upon exclusively, the procedure usually entailed assigning scores, when possible, from a comparably worded item from the same or a different schedule, e.g., father's education was included in both the 1955 and 1970 questionnaires. A comparison of means, standard deviations, inter-item correlations and parallel path analyses with and without missing data estimates revealed no notable estimation biasing effects. These procedures and their evaluation are reported elsewhere (Alexander and Eckland, 1973:92-7).

1. *Family background status.* The effects of four status background variables are examined in the analysis. Except for some missing data estimates, all were obtained from the 1970 schedule:

A. Mother's and Father's Education: a set of nine pre-coded response categories ranging from "none, or some grade school" to "Ph.D. or professional degree";

B. Father's Occupational Status: an open-ended item pertaining to father's occupation "while you were in high school" and assigned scores on Duncan's SEI scale;

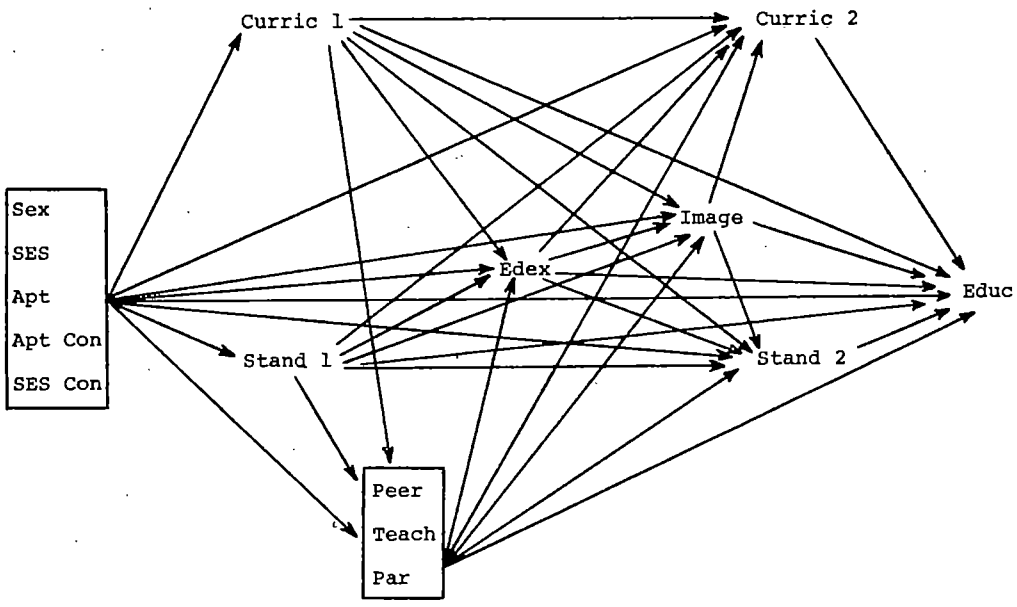
C. Acquisition Index: a factor weighted index of possessions in the respondent's high school household, using Maxwell's weighting scheme (Maxwell, 1971). The initial set of 22 household commodity items in the questionnaire was reduced to 13, with a reliability coefficient of .83.

Elsewhere (Alexander and Eckland, 1973) the effects of these four status background indicators were mediated through an unmeasured SES construct generated with a canonical correlation analysis (Hauser, 1972; Hauser and Goldberger, 1971). While that strategy entailed a modest loss of explanatory power as a consequence of disregarding linear covariance between the set of status items and all other variables in the model not subsumed under the first canonical correlation, it was used to achieve somewhat greater parsimony in the model.

In this analysis, however, the measures of status background are of interest principally as individual level controls in the evaluation of contextual effects. Hence, we are reluctant to incur even the modest reduction in explanatory power involved in the canonical technique and therefore will employ Heise's "sheaf" coefficient to provide a summary measure of the joint effects of status background. Computationally the sheaf coefficient involves

[13] If the primary objective of this study were to produce a set of descriptive statistics on college-going rates, dropout rates and the like, then these sample biases might present a more serious problem. We are not, however, interested in the marginals per se, but in the relationships among variables. It is such relationships that tell us something about the causal network involved in the attainment process. Thus, the critical question is: would the correlations among variables differ substantially from those we obtained had everyone in the original sample (or, ideally, the universe) been included in the study? What evidence we have suggests that these differences would not be great. There were eleven "status attainment" variables in the 1955 data for which information was available from the EEO respondents, the total EEO sample and the original ETS sample, the last data set including schools not in the EEO followup survey. Separate intercorrelation matrices for these eleven items were generated, by sex, for each of these three samples. The average difference in the magnitude of these coefficients for all possible comparison (n = 330) was only .023, although for a few comparisons the difference was as large as .08. In general, the coefficients tended to be slightly higher in both the Respondent and Full EEO samples than in the original ETS sample, indicating that the major source of bias, to the extent that it exists here, involves sampling error in the selection of follow-up schools and not low response rates.

Figure 1. High School Contextual Effects Model



NOTE: For ease of presentation, some variables have been "blocked" in the diagram. In the actual structural model, however, the separate variables effect one another within each block, including the four status background indicators of SES not shown in the diagram. Variable abbreviations used either here or in the tables are:

SES, sheaf coefficient for:
- Moth Ed, mother's education
- Acquis, household acquisitions
- Fath Ed, father's education
- Fath Occ, father's occupation

Apt, academic aptitude
Apt Con, aptitude context
SES Con, status context

Curric 1, sophomore curriculum enrollment
Stand 1, sophomore class standing
Peer, peer college plans
Teach, teacher contact about college plans
Par, parent contact about college plans
Edex, educational expectations
Image, academic self-concept
Curric 2, senior curriculum enrollment
Stand 2, senior class standing
Educ, educational attainment

summing the direct status regression coefficients for a given dependent variable and then correcting that sum to take into account the interrelationships among the status indicators themselves. Unlike canonical correlations, this procedure entails no loss of information.

2. *Academic aptitude.* Aptitude was measured with an untimed, twenty-item test administered by ETS during the 1955 survey, with an estimated reliability of .82 (Stice and Ekstrom, 1964).

3. *Peer college plans.* An index of peer college orientation was constructed by summing responses to two pertinent items from the 1955 schedule: the first, "What are the college plans of the friend you most like?" and the second, "Of the people your own age with whom you spend most of your free time, how many of them plan to go to college or are already going to college?"

4. *Sophomore and senior class standing.* The sophomore class standing measure was included in the 1955 schedule as a quartile self-ranking, while senior class standing is based on a quintile ranking obtained from school records in 1959 (Stice and Ekstrom, 1964).

5. *Sophomore and senior year curriculum enrollment.* Curriculum enrollment items for the sophomore and senior years were included in the 1955 and 1970 student questionnaires, respectively. Both items were dichotomized into college preparatory versus all other categories.

6. *Adult influences.* Two indicators of the influence of adult "significant others" were

obtained from the 1955 schedule. The first item pertained to school personnel and asked, "To what extent have you discussed going to college with the teachers or guidance counselors (advisors) in your school?" The other comparably worded item pertained to parents.

7. *Academic self-concept.* A factor analysis of items involving self-descriptions of academic performance and problems produced a nine-item scale with an estimated reliability of .90. The items, taken from the 1970 schedule, are retrospective and include such self-evaluations as "I usually did a bit more than the teacher required" and "I had trouble remembering what I read."

8. *Educational expectations.* Plans after high school were measured in 1955 with an item that asked: "What do you think you *will* do when you finish high school (suppose that you do not go into military service)?" The original eight response categories were reduced to a set of three: "college goers," a "possible college" group and "non-college-goers."

9. *Educational attainment.* This was measured with the same set of categories employed for the parental education items, except ranging only from "some high school" to "Ph.D. or professional degree."

10. *Contextual variables.* Aggregate school-level measures of aptitude and status background were computed on the ETS test of academic aptitude and an item from the 1955 schedule regarding father's education. The latter is the only "family status" variable for which usable 1955 data were available. Both aggregate measures are based on data for EEO respondents *and* non-respondents (i.e., all sophomores in EEO schools included in the original ETS study).[14]

[14] In the case of status context, it would have been possible to average scores on at least four family indicators from the 1970 data and possibly produce a more reliable measure. However, these data were available only for the respondents. Since the sample schools generally were below average in size (fully 14 of the 42 schools had sophomore classes with under 50 students) and since the response rate for many of these schools was less than 50 percent, it was assumed that greater reliability would be gained by having a single measure of status context based on more students (using both respondents and nonrespondents) than several measures based on a more limited number (respondents only).

Multiple regression and path analysis will be our basic analytic techniques. The coefficients to be reported may be interpreted as net, standardized effects. Patterns of indirect influence can be computed through the standard "tracing" rules of path analysis. The techniques and assumptions of path analysis are available in a number of sources (Duncan, 1966; Heise, 1969; Land, 1969; Finney, 1972) and need not be recapitulated here.

The analysis is based upon correlations uncorrected for attenuation, partly in order to retain comparability with other published analyses of these data and partly because we do not have reliability estimates for all variables in the model. However, we were able to correct the interitem correlation matrix for unreliability in three of our four individual-level status indicators, individual-level academic ability and academic self-concept. We then assessed the consequences of these corrections for the composition effects in our analysis, which is our primary substantive interest in this report. The corrected and uncorrected matrices produced nearly identical results in both magnitude and implication for the 72 possible comparisons which were involved. Thus, these corrections for unreliability would not effect our conclusions. In view of this and the other reasons mentioned above, the present analysis is based upon the observed interitem correlation matrix.

Originally, parallel analyses of the model in Figure 1 were performed for males and females. However, since only one significant interaction involving sex and the contextual variables was obtained (which we will note later), these groups have been combined and sex has been included as an exogenous variable in this presentation. Based on our scoring procedure, negative "sex" coefficients in the following tables indicate a negative consequence of being female (or a positive consequence of being male).

RESULTS

The means, standard deviations and interitem correlations from which the various models to be discussed were generated are presented in Table 1. The two contextual measures correlate .66, a value comparable to that obtained by Meyer (1970) for similar

Table 1. Means, Standard Deviations, and Interitem Correlation for Variables in the Contextual Effects Model (N=1523)

	Sex	Moth Ed	Acquis	Fath Ed	Fath Occ	Apt	Apt Con	SES Con	Curric 1	Stand 1	Peer	Teach	Par	Edex	Image	Curric 2	Stand 2	Educ
Sex[a]		-.019	.011	-.009	.000	.017	-.001	-.009	-.099	.134	.024	-.037	.053	-.018	.202	-.052	.252	-.199
Moth Ed			.452	.544	.345	.241	.196	.259	.293	.149	.332	.147	.261	.357	.138	.319	.159	.328
Acquis				.485	.432	.229	.306	.436	.330	.147	.364	.200	.322	.379	.112	.370	.163	.321
Fath Ed					.538	.285	.229	.344	.366	.203	.347	.153	.291	.391	.125	.371	.205	.372
Fath Occ						.214	.211	.383	.306	.119	.300	.150	.277	.317	.076	.332	.137	.314
Apt							.240	.190	.339	.461	.267	.105	.235	.281	.392	.410	.518	.417
Apt Con								.662	.170	-.015	.114	-.014	.075	.090	-.032	.200	-.017	.140
SES Con									.325	.012	.267	.087	.197	.202	.004	.310	.002	.236
Curric 1										.262	.396	.248	.389	.485	.174	.629	.252	.458
Stand 1											.267	.183	.267	.293	.421	.332	.611	.381
Peer												.279	.465	.554	.171	.407	.279	.397
Teach													.427	.256	.072	.194	.104	.183
Par														.499	.189	.393	.272	.326
Edex															.200	.493	.298	.462
Image																.221	.504	.233
Curric 2																	.370	.544
Stand 2																		.392
Mean	.544	3.54	.081	3.34	40.1	7.78	7.02	3.25	.424	3.19	5.07	1.63	2.29	1.95	1.01	.395	2.30	5.05
S.D.	.498	1.40	.915	1.85	22.0	4.06	1.11	.580	.494	.504	1.64	.612	.662	.874	.839	.489	1.34	1.51

[a]Negative coefficients indicate a negative correlation with being female. See note to Figure 1 for clarification of variable abbreviations.

Table 2. Reduced and Full Form Regression Coefficients Showing Structural Effects of Five Exogenous Variables

	Curric 1	Stand 1	Peer	Teach	Par	Edex	Image	Curric 1	Stand 1	Educ
			A. Reduced Form Coefficients							
Sex	-.100	.126	.024*	-.039*	.051	-.019*	.196	-.055	.243	-.202
SES	.275	.126	.355	.215	.326	.430	.083	.308	.130	.315
Apt	.240	.461	.161	.066	.148	.166	.404	.304	.520	.327
Apt Con	-.130	-.140	-.151	-.144	-.131	-.119	-.150	-.073	-.144	-.093
SES Con	.240	-.034*	.174	.067*	.100	.051*	.000*	.154	-.060	.090
R^2 (Con in)	.262	.258	.228	.063	.170	.248	.215	.300	.361	.321
R^2 (Con out)	.235	.235	.211	.051	.161	.240	.195	.288	.332	.315
			B. Full Form Coefficients							
Sex	-.100	.126	.029*	-.038*	.059	-.025*	.164	-.032*	.159	-.240
SES	.275	.126	.284	.158	.244	.198	.052	.107	.032	.131
Apt	.240	.461	.052	-.036*	.025*	.008*	.274	.126	.256	.078
Apt Con	-.130	-.140	-.106	-.103	-.079	-.013*	-.109	.019*	-.050	-.006*
SES Con	.240	-.034*	.128	.030*	.044*	-.068	.001*	.044*	-.067	.041*
R^2 (Con in)	.262	.258	.277	.102	.237	.464	.268	.502	.531	.475

*Coefficients are less than twice their standard errors.

variables with data from the 1955 ETS sample of seniors.

Part A of Table 2 presents the standardized effects of individual and aggregate aptitude and status background upon each variable in the school process model *without controlling* for the effects of intervening variables upon subsequent outcomes.[15]

A first point of interest is the small increment in explanatory power accruing from the addition of contextual variables to the various equations. The differences between the corresponding coefficients of determination computed with and without contextual variables included in the analysis all are statistically significant at conventional alpha levels, yet range from only .006 for educational attainment to .029 for senior class standing. This procedure, however, may underestimate the importance of the contextual variables since it attributes the explanatory power shared by the individual and aggregate measures (i.e., their covariance) to the former, a procedure which is not entirely defensible.

Referring still to Part A of Table 2, it appears that our expectations regarding the direction of influence for these composition variables were reasonably accurate. Every ability composition coefficient is negative, while seven of the ten status effects are

[15] In the "reduced form" model (Part A of Table 2), the dependent variables separately are regressed only on the five exogenous variables, while in the "full form" model (Part B of Table 2), the dependent variables are regressed on *all* prior variables in accordance with the path diagram shown in Figure 1. Table 3 differs from Part B of Table 2 in that the coefficients have been recalculated after the "trivial" effects are deleted (with the exception of those involving the individual-level exogenous variables) and, in addition, Table 3 shows the effects of the school process or intervening variables on any subsequent outcomes.

positive.[16] The difference between offsetting composition effects would result in an appreciable "net" contextual influence in only a few instances. In fact, the overall salutary effect of status context on educational attainment, .090, is almost exactly offset by the depressant effect of ability context, −.093. Thus, the composition effects are largely as anticipated: of modest magnitude, negative for ability context, positive for status context and, given their opposite signs and often comparable magnitudes, largely counterbalancing.[17]

It will be recalled that ability composition is expected to have its major direct impact on relative performance scores, affecting later outcomes mainly through this mechanism; status composition, on the other hand, should affect the likelihood of being in a college-preparatory program and acquiring college-oriented peer associates which, in turn, affect one's own goals and subsequent attainment.

These hypotheses are assessed in the models presented in Part B of Table 2 and in Table 3. In Part B, all patterns of influence portrayed in Figure 1 are estimated, but only reported for the exogenous variables. The primary purpose here is to demonstrate the extent to which the effects of the contextual variables on later outcomes are in fact being mediated by the school process variables in the model. Comparing the reduced form coefficients in Part A and the full form or structural coefficients in Part B, it will be noted that whereas the effect of status composition on educational attainment net of only the other exogenous variables is .090, the direct effect net of both these and the school process variables is .090, the direct effect net of both these and the school process variables is .041. Likewise, the effect of ability composition on educational attainment net of only the other exogenous variables is −.093, whereas the direct effect net of all other variables is −.006.

(The reader may wish to note that most of the effects of individual family status and individual ability on educational attainment also are being mediated through the intervening variables in this model. The sex effects, on the other hand, are not. In fact, the direct depressant effect of being female on educational attainment is larger in the structural model than the effect shown in the reduced form version. The reasons for this finding and its implications have been discussed at some length recently in this journal, see Alexander and Eckland, 1974.)

In Table 3, the full model is presented but with most "trivial" effects removed. That is, except for those coefficients involving the individual-level exogenous variables, the regression coefficients from the analysis in Table 2B (most of which were not reported therein) have been recalculated after deleting effects of less than .100.[18] Though often statistically significant at conventional levels, coefficients of this magnitude are deemed substantively unimportant. We will now consider in some detail the findings reported in this table.

Aptitude composition, as anticipated, modestly depresses sophomore class rank (with individual ability controlled), which, given the importance of class rank for subsequent outcomes, implies some indirect negative consequence. The largest such mediated effect is for senior rank at −.060, followed by a −.040 influence upon academic self-concept. The latter, though modest, is consistent with our interpretation of the Coleman-Campbell results discussed earlier.

However, we also had anticipated that low relative performance scores (class rank) would depress college plans, thereby mediating some of the negative effects of a high ability context on educational attainment. This was not the case. College plans do not appear to be directly affected by sophomore class standing nor, for that matter, even by individual academic aptitude. Rather, the direct determinants of college plans are family status background, .167; sophomore curriculum enrollment, .204; and both peer, .293, and parental, .217, influences.

It apparently is through its negative effects

[16] In view of the substantial correlation between status and ability composition measures, sizable same-sign effects would be quite unlikely.

[17] Although these same general results were obtained separately for males and females, the contextual effects of ability were uniformly somewhat stronger for females while the contextual effects of status were uniformly somewhat stronger for males.

[18] The individual-level exogenous variables are retained in all regression equations regardless of the size of their coefficients because of their importance as control variables.

Table 3. Standardized Regression Coefficients for Full Contextual Effects Model with "Trivial" Effects Deleted

	Curric 1	Stand 1	Peer	Teach	Par	Edex	Image	Curric 1	Stand 1	Educ
Sex*	-.100	.126	.024	-.038	.058	-.016	.163	-.009	.154	-.240
SES*	.275	.118	.284	.166	.241	.167	.064	.141	.035	.162
Apt*	.240	.462	.161	-.037	.010	.031	.284	.176	.247	.088
Apt Con	-.130	-.159	-.151	-.086			-.109			
SES Con	.240		.174							
Curric 1				.176	.256	.204		.429		
Stand 1				.131	.144		.259		.380	.111
Peer						.293				
Teach										
Par						.217				
Edex								.172		.153
Image									.212	
Curric 2										.251
Stand 2										.166
R^2	.262	.257	.228	.102	.234	.456	.265	.489	.513	.468

*Paths with coefficients below .100 were removed from the equations, with the exception of the three individual-level exogenous variables which were retained in all equations because of their importance as controls.

on early curriculum decisions and peer associations that ability context exerts part of its negative influence on later outcomes. The depressant effects of ability composition on sophomore curriculum enrollment are not necessarily inconsistent with the relative deprivation hypothesis, however, since the linkage here could involve the influence of earlier (but unmeasured) relative performance scores. In any case, as just noted, enrollment in a college-preparatory program and association with college-oriented peers are among the primary determinants of college plans which, in turn, is a primary determinant of actual attainment. Moreover, sophomore curriculum enrollment, as would be expected, is the primary determinant of senior curriculum enrollment which, in turn, has a substantially stronger direct effect on educational attainment than any other variable in the model, except for sex.

Although contact with teachers or counselors about college plans has no apparent consequences in this model and academic self-concept appears only to influence senior rank, both teacher contact and self-concept also are negatively affected by the ability composition of the school. Thus, while the depressant effects of a high ability context on relative academic performance were found here, ability composition has negative direct effects on a broader range of variables than expected, as well as negative indirect effects too numerous to have but briefly mentioned.

Turning finally to the positive influences of a school's status composition, we note only two main effects, both of which were hypothesized. The larger of these is for sophomore curriculum, .240, which incidentally more than offsets the negative influence of ability context on sophomore curriculum. The status composition effect on enrollment in a college preparatory program, moreover, is actually comparable in size to that of any of the other exogenous variables on curriculum enrollment in the model, including individual

ability and individual status background.

The other salutary effect of status composition is its influence on a student's direct association with college-oriented peers, .174. The sex difference in this particular instance, however, was significant when the model was examined separately for males and females.[19] The status context effect on peers was .202 for males and only .079 for females (although still statistically significant). It appears then that the dynamics of peer group formation may be different for females than that implied by the status-mediation theory outlined earlier.

For instance, the Campbell-Alexander model (1965) assumes that, within schools, peer relations are not unduly constrained by status background homophily or some correlate thereof.[20] However, female social networks may be less open to students of diverse background, perhaps because of greater "status consciousness" among female adolescents or a scarcity of activities for women, such as athletics, which induce mingling. Thus, the "random" contacts of low status women with their high status classmates in relatively high status schools might result in fewer friendships than would similar contacts among males. Women, then, tend to associate largely with their status "equals," *regardless* of the status composition of the student body as a whole. Because peer status background is presumed to link status composition with peer plans in our model, these sex differences in the dynamics of peer group formation, should they maintain, might account for our results.[21]

In any case, status composition operates, at least for males, in essentially the manner that was hypothesized. The effects are directly salutary for enrollment in a college-preparatory curriculum and for association with college-oriented peers which, in turn, positively affect a number of subsequent outcomes in the model, including educational attainment.

SUMMARY AND CONCLUSIONS

A mediation model of student body composition effects was developed that distinguished between ability and status dimensions of composition and specified the more proximate interpersonal and subjective mechanisms through which their consequences should operate. The model was evaluated by incorporating contextual variables into a basic school attainment model, using fifteen-year longitudinal data for a national sample of youth.

Ability composition was found to modestly depress all school process variables and educational outcomes in the model, while the effects of status composition were just the opposite. When the full model was evaluated, the contextual effects of ability were found to lower relative academic performance, as well as to be mediated through a number of other mechanisms in producing negative effects. Status composition, on the other hand, positively affected the likelihood of enrollment in a college preparatory curriculum and, at least for males, the likelihood of associating with college-oriented peers. The net results of the contextual variables were almost exactly offsetting in terms of the model's final outcome, educational attainment.

We do not presume to have examined all the dimensions of composition that might be pertinent to school processes—the absence of data on school facilities and organization in the present analysis is certainly one limitation. However, even including additional variables would offer no assurance that some theoretically and empirically important control

[19] While it might have been tempting to compare in detail the composition coefficients for men and women, such comparisons would reveal little of interest. In a covariance analysis (not reported in the text), only one of the thirty-six sex-context interaction terms for the reduced form and structural models was found to be statistically significant. Thus we find little evidence of differential context effects by sex.

[20] Rhodes, Reiss and Duncan (1965) have documented that the degree of status homophily in within-school peer relations somewhat exceeds that which would be expected under a "random relationship" model.

[21] Our data are reasonably consistent with this interpretation, but they clearly do not constitute a test of it. The *individual* SES regression coefficient for peer plans for women is .435, as compared with .299 for men. The respective coefficients for status composition are .113 and .248. Thus, individual status does appear to be somewhat more consequential for women, while aggregate status is so for men.

variable was not excluded. This qualification must apply to all research in this domain.

The final judgment regarding the implications of this research should be based upon the apparent cogency of the substantive model, the accuracy of its predictions and some assessment of the likelihood that those confirmed are not artifactual. We can only note that the research design employed here is probably the strongest possible for exploring between-group differences given the limitations of survey data.

We find the general model persuasive and the results supportive. We would encourage further refinement of the framework, particularly through the explicit measurement of school resources and, at the individual level, the utilization of such resources (Jencks, 1972a; Smith, 1972). Such data would complement our contextual measures and provide for a more comprehensive model of aggregate and structural effects and a fuller understanding of the intricate complexity of cross-level influences.

Recognizing the modesty of the influences examined in this analysis, we may put forth the following conclusions: with individual ability and status characteristics constant, a "benefit" accrues to students in educational institutions characterized by a high status-low ability student body in terms of increased likelihood of enrollment in a college preparatory curriculum, of involvement with college-oriented peers, and of enhanced academic self-concept, college plans and actual attainments. Competing with relatively low status-high ability peers, on the other hand, has the opposite effects. While we were able to distinguish between the status and ability dimensions of student body composition analytically, one can also envision some degree of independence between them in the "real world." A socially elite prep school with low academic standards would constitute the "ideal" educationally enhancing environment under this model. The "high ability-low status" school, perhaps less likely to occur in a natural setting, would be the most debilitating from the vantage point of the model evaluated here.[22]

[22] At a minimum, the availability of measures of absolute academic performance, in addition to grades, would be required to accurately assess the net results of such school influences.

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ITEMS *(Continued)*

Merrill, 1973) and (with co-editor Robert Laufer) *Youth, Generations and Social Change* (1974).

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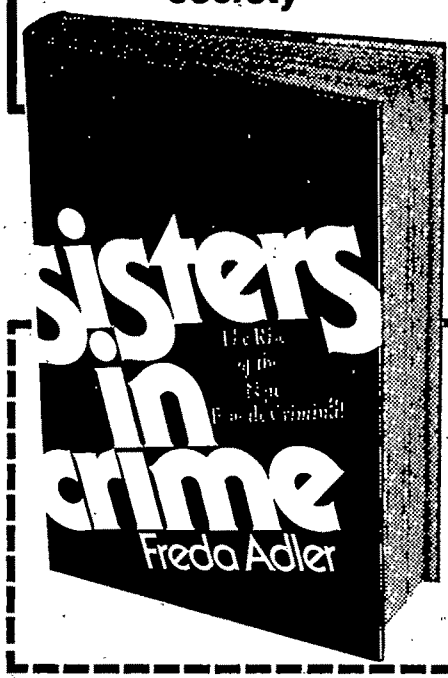

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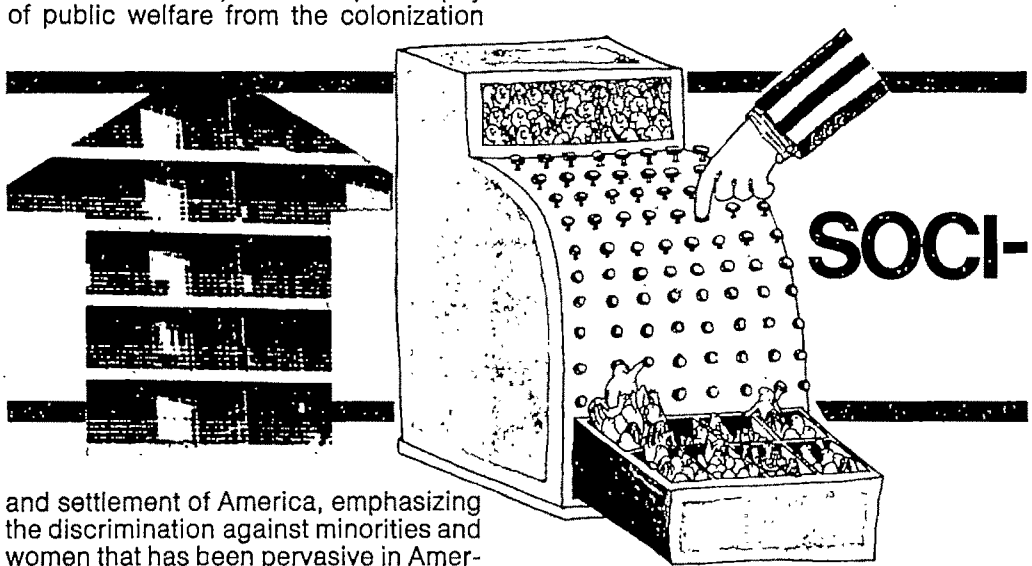

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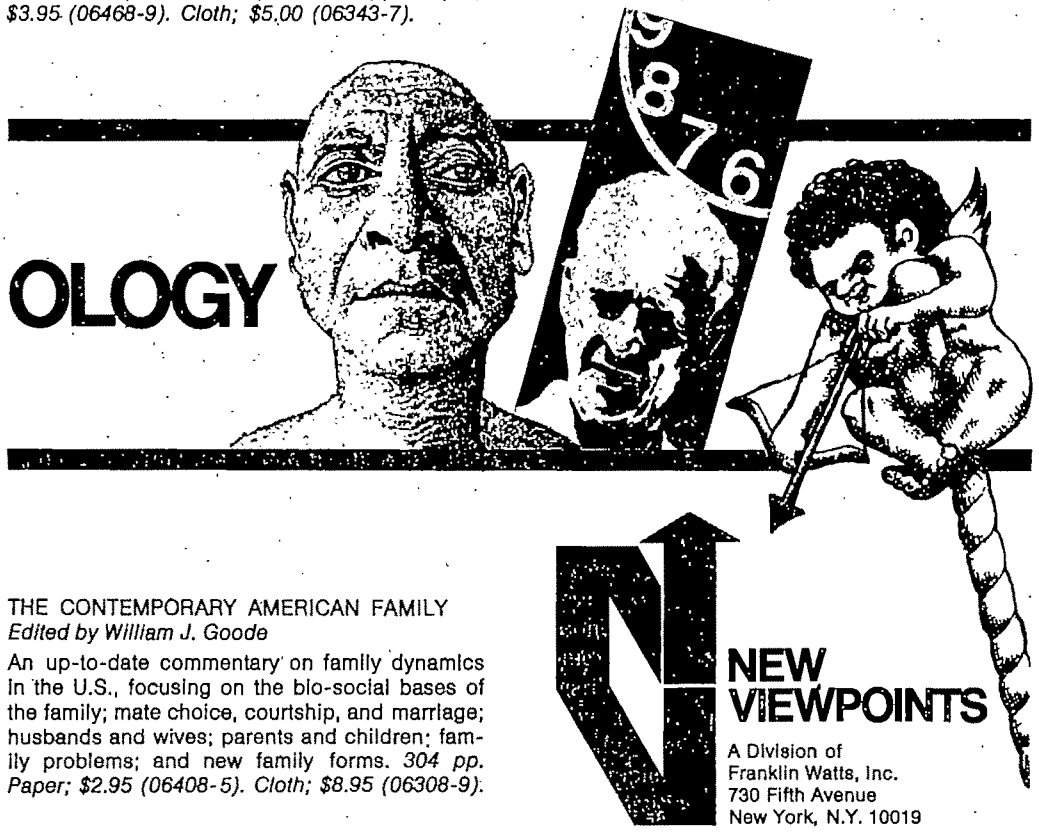

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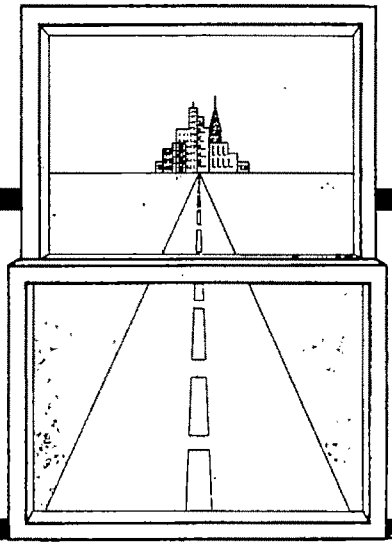

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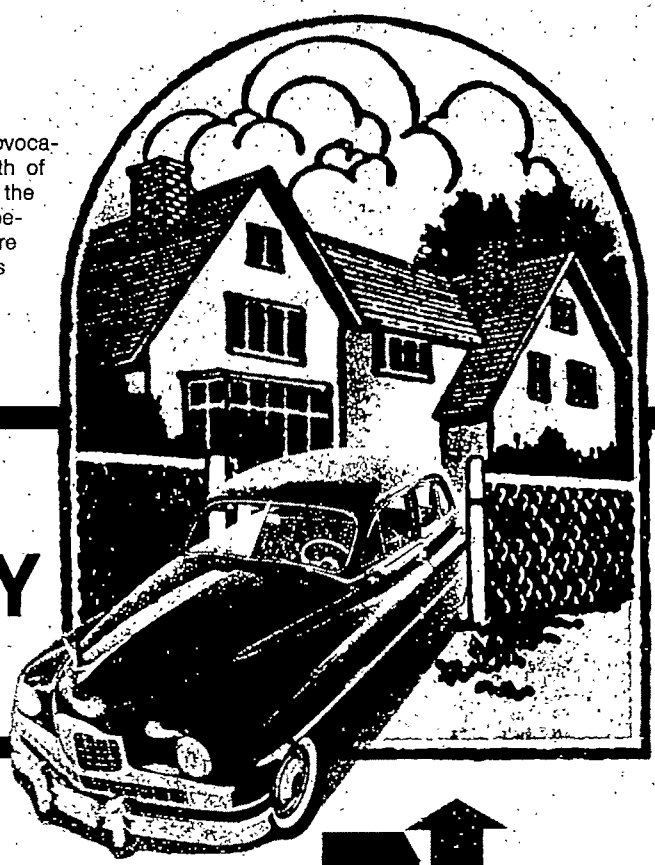

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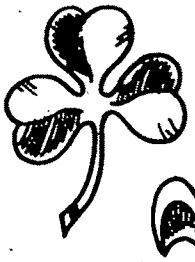

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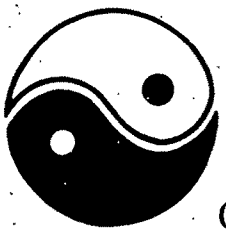

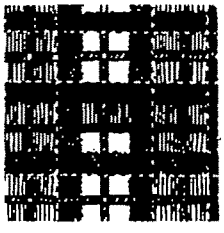

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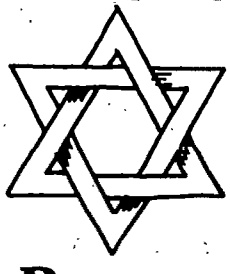

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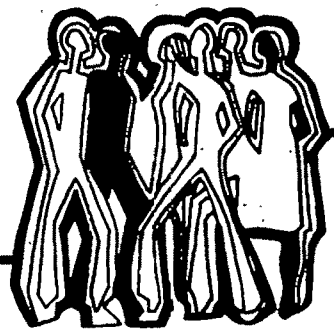

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The Divergence of Weber and Durkheim

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Marwell
Why Ascription?

Sørensen
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AUGUST, 1975 VOLUME 40 NUMBER 4

ARTICLES

On the Divergence of Weber and Durkheim: A Critique of Parsons' Convergence Thesis ..... WHITNEY POPE, JERE COHEN AND LAWRENCE HAZELRIGG 417

The Market, Tradition and Peasant Rebellion: The Case of Romania in 1907 ..... DANIEL CHIROT AND CHARLES RAGIN 428

Why Ascription? Parts of a More or Less Formal Theory of the Functions and Dysfunctions of Sex Roles ..... GERALD MARWELL 445

The Structure of Intragenerational Mobility ..... AAGE B. SØRENSEN 456

The Occupational Achievement Process, 1940-1949: A Cohort Analysis ..... ANGELA LANE 472

A Comparison of Men's and Women's Intergenerational Mobility in the United States ..... IVAN CHASE 483

Party Class Images and the Class Vote in Canada ..... RICK OGMUNDSON 506

"The Internal Stratification of the Working Class": A Reanalysis .... KENNETH I. SPENNER 513

Determinants of Career Plans: Institutional versus Interpersonal Effects ..... EPHRAIM YUCHTMAN (YAAR) AND YITZHAK SAMUEL 521

COMMENTS

"The Internal Stratification of the Working Class." A Reanalysis
Comment on Spenner ..... WILLIAM H. FORM 532

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August, 1975

▪ WHITNEY POPE (The Divergence of Weber and Durkheim) is Associate Professor of Sociology at Indiana University, Bloomington. He is preparing a monograph on Durkheim's theory of social integration, *Durkheim's "Suicide": A Classic Analyzed*, to appear in 1976, and is working on an analysis of de Tocqueville as a comparative social and political theorist. JERE COHEN is Assistant Professor of Sociology at the University of Maryland, Baltimore County. His present interests involve research on psychological modernity, adolescent subcultures and peer groups, and small group decision-making. LAWRENCE E. HAZELRIGG is Associate Professor of Sociology at Indiana University, Bloomington. He is working on a cross-national analysis of relationships among economic growth, inequality, and occupational status change and is coauthor, with Joseph Lopreato, of *Class, Conflict, and Mobility*.

▪ DANIEL CHIROT (Peasant Rebellions) is Assistant Professor at the Center for Studies in Macrosociology at the University of Washington, Seattle. The present paper and a forthcoming book, *A Social History of Wallachia: Agrarian Relations in a Peripheral Society*, reflect his interest in the problems of development in the modern world system. He is preparing a special issue of *Social Forces* on recent trends in historical sociology. CHARLES RAGIN is Assistant Professor of Sociology at Indiana University, Bloomington. His recently completed doctoral dissertation at the University of North Carolina, Chapel Hill, dealt with the problems of regional political integration in Great Britain. His current work includes the expansion of his dissertation topic to cover advanced societies in general.

▪ GERALD MARWELL (Why Ascription?) is Professor of Sociology at the University of Wisconsin-Madison. He is primarily interested in the organization of collective effort, whether through institutionalized role structures, social movements or spontaneous cooperation. He is coauthor of the recently published *Cooperation: An Experimental Analysis*, with David R. Schmitt, and of *Dynamics of Idealism: White Activists in a Black Movement*, with N. J. Demerath and Michael T. Aiken.

▪ AAGE B. SØRENSEN (Intragenerational Mobility) is Associate Professor of Sociology at the University of Wisconsin-Madison. His ongoing research includes study of the occupational achievement process with emphasis on the interplay between the structure of inequality, personal occupational resources and occupational careers. In addition, he is pursuing research on the academic achievement process and change in the structure of informal groups.

▪ ANGELA LANE (A Cohort Analysis of Occupational Achievement) is Assistant Professor of Sociology at Indiana University, Bloomington. Social stratification and gender roles are her principal research interests. Her current projects include

(Continued on page 536)

AMERICAN SOCIOLOGICAL REVIEW

AUGUST, 1975 VOLUME 40 NUMBER 4

ON THE DIVERGENCE OF WEBER AND DURKHEIM: A CRITIQUE OF PARSONS' CONVERGENCE THESIS*

WHITNEY POPE

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LAWRENCE E. HAZELRIGG

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American Sociological Review 1975, Vol. 40 (August):417–427

The main thesis of this paper is that Parsons' claim that the works of Durkheim and Weber substantively converged on a voluntaristic theory of action is inaccurate and that his alleged demonstration of such convergence is grounded in serious distortions of the Durkheimian and Weberian positions. We first examine basic differences between Durkheim and Weber and then consider Parsons' claim in the light of those differences.

Parsons recently (1972:102) noted that it was while working on *The Structure of Social Action* (1949) that he first perceived, beneath their outward differences, a convergence on the voluntaristic theory of action in the works of Weber, Pareto, Durkheim and Marshall (see also Parsons, 1949:719-20, 682). Parsons felt that this convergence constituted a major revolution in social theory and that his book would be the first to demonstrate it.[1] He (1972:104) has judged his demonstration of that thesis to be the major accomplishment of *The Structure*, and even today expresses (1972:102, 104, 106; 1974a; 1974b) no reservations about either the thesis itself or the adequacy of his demonstration. We argue, however, that this demonstration often conflicted with a faithful interpretation of the theorists (regarding his interpretations of Durkheim and Weber see Pope, 1973, and Cohen et al., 1975, respectively). Indeed, a re-examination of the central juxtaposition in Parsons' comparative analysis—the comparison of Weber and Durkheim—reveals major differences that overwhelm the similarities. While a total recounting of differences resists brief explication, the most important relate to their different conceptions of the social and of sociological explanation. We first examine basic differences in perspective between Durkheim and Weber. We then consider Parsons' argument for convergence on the theory of action in the context of this evidence. Our concern is not only the question of whether Durkheim and Weber converged at those points where Parsons claims to find such convergence, but also the question of other equally important areas of possible convergence or divergence.

*We acknowledge the editorial assistance of Carolyn J. Mullins. We also wish to thank R. Stephen Warner and Norbert Wiley for sharing with us their reactions to this paper.

[1] In an invidious comparison, Parsons (1972:149) has observed that in contrast to *The Structure* "the most widely used compendium in the field in the 1930s [Sorokin's *Contemporary Sociological Theories* (1928)] treated Pareto, Durkheim and Weber as belonging to entirely different schools."

BASIC DIFFERENCES IN PERSPECTIVE

The Social and Society

Durkheim (1950:102) insisted that relative to its individual members, society was a *sui generis*, emergent phenomenon. "When the individual has been eliminated, society alone remains. We must, then, seek the explanation of social life in the nature of society itself." In contrast, Weber rejected any conception of society as an emergent phenomenon. While noting that the sheer convenience of locution almost necessitated their use, he argued for the need to remember that such holistic concepts as "state" or "society" refer only to the probability that *individuals* will act in certain ways under given conditions. "When reference is made in a sociological context to a state, a nation, a corporation, a family, . . . or to similar collectivities, what is meant is . . . *only* a certain kind of development of actual or possible social actions of individual persons" (Weber, 1968:14, also 13). Thus, while Durkheim insisted that sociology must focus on emergent social phenomena, particularly society itself, Weber felt that sociology must be couched in terms that have reference ultimately to the behavior of individual actors. Insofar as Weber's stance can be characterized as one of methodological individualism, Durkheim's views formed a contrary stance.[2]

Furthermore, whereas Durkheim often analyzed society as a structural-functional system of forces forming an equilibrium, Weber (1968:15) warned specifically against such a functional mode of analysis: its "cognitive value" is not to be "overestimated" or "its concepts illegitimately 'reified' "; it is useful only "for purposes of practical illustration and . . . provisional orientation." For these and other reasons, Weber (1968:17-8) limited functional analysis to a "preliminary" role in sociological analysis.

Causation and Explanation

The two theorists' approaches to social causation and sociological explanation mirror their views on the logical status of the concept "society." Weber (1968:12) stated that behavior must be understood in terms of individual motives and subjective meanings. "A correct causal interpretation of a concrete course of action is arrived at when the overt action and the motives have both been correctly apprehended and at the same time their relation has become meaningfully comprehensible." This may be contrasted with Durkheim's approach, exemplified in *Suicide* (1951:151; emphasis added), where he proposes to explain even such a seemingly subjectively inspired, private, individual act as suicide by "*disregarding* the individual as such, *his motives and ideas*" in order to seek directly its social causes. Throughout his work Durkheim identified numerous grounds on which sociological explanation must reject appeals to the subjective states of individual actors, grounds that frequently placed his views in direct opposition to Weber's.

First, subjective states were considered insufficiently accessible to scientific observation to be legitimate objects of scientific analysis (Durkheim, 1950:27-8). "Intent is too intimate a thing to be more than approximately interpreted by another. It even escapes self-observation" (Durkheim 1951:43). But Weber felt that the ability to discover intent was one of the great advantages of the social (as opposed to the natural) sciences. He (1968:15) argued that while we cannot put ourselves in the place of biological cells in order to discover why they behave as they do, we can do so in the case of human beings.

Second, Durkheim (1950:27-30; 1951:36, 148-9) felt that because subjective states were changeable by mere acts of will, they lacked the objectivity of scientific

---

[2] In a recent commentary, Parsons (1974b:56) observed with reference to his convergence thesis that:

> In taking Durkheim's line of thought as seriously as I did I think I was able to remain completely true to Weber's famous "methodological individualism" with respect to which he maintained that action in a proper sense occurs only through the agency of individual human beings and that the "intentions" of these human beings, the meaning of their actions, and of their consequences to them "subjectively" are of the essence of the Weberian method. I have never been able to see that this Weberian point of view, to which I wholeheartedly subscribe, was in any serious conflict with that of Durkheim in spite of the important differences of their approaches.

phenomena. Third, Durkheim (1951:43, 67) rejected analysis based on the ends pursued by actors. "An act cannot be defined by the end sought by the actor, for an identical system of behavior may be adjustable to too many different ends without altering its nature." Moreover, if "historic development took place in terms of ends clearly or obscurely felt, social facts should present the most infinite diversity; and all comparison should be almost impossible" (Durkheim, 1950:94). Given Durkheim's (1950: 125-40) appreciation of the comparative method as the sociological method par excellence, such a circumstance would undermine the very possibility of establishing a scientific sociology. Weber, on the other hand, not only acknowledged a degree of diversity in the goal orientations of people; he also argued that behaviors meaningfully and understandably vary in relation to goal directedness. Indeed, one of his categories of social action (*zweckrational*) includes a deliberate emphasis on the importance of goal directedness (Weber, 1968:24-6).

Fourth, Durkheim viewed sociologically relevant subjective states as the product of social causes. They "are like prolongations . . . inside of individuals" (Durkheim, 1951:287, see also 151) of the social causes on which they depend. They may enter sociological explanation as effects, but never as causes. Appeal to subjective states as causal agents, according to Durkheim, threatened the legitimacy of sociology's claim to scientific status by reducing it to psychology. As is commonly recognized, Durkheim developed a massive and multifaceted case against appeal to subjective states in sociological explanation.[3] Weber, in contrast, felt that adequate understanding of subjective states was indispensable to sociological explanation.

Central Theoretical Dynamic

Durkheim viewed society as a force or power. Further, he (1960:130; 1951:319, 335; 1964:325-40; 1965:29, 237-8, 297-8) formulated the relationship between the social and his concept of an unsocialized individual as an opposition of forces. This hypothesized opposition provided his theory with its central dynamic. Weber, on the other hand, rejecting appeal to emergent social phenomena, did not conceive of society as an independent force that stands in opposition to an unsocialized individual. Rather, the central dynamic of his theory consists in his conception of competitive struggles for advantage among individuals and groups of individuals as each seeks to realize its own ideal and material interests. To Weber "irreconcilable conflicts are an endemic feature of society" (Bendix, 1962:263). Durkheim (1960:70-110 and *passim;* 1965) discounted intrasocietal conflict in premodern societies; in modern societies he (1960:353-95) considered it pathological and, except for those "moments of collective ferment" out of which new harmonies emerge (1953:91; see also 1965:475-6), not central to an understanding of societal dynamics.

The Explanation of Social Order

Parsons has long identified the problem of order as the basic question for sociological theory. From this viewpoint, differences between Weber and Durkheim culminate in

[3] Though his more dogmatic, polemically inspired statements sometimes create a different impression, Durkheim's rejection of reductionist explanations of social phenomena does *not* mean that all psychological, individual or subjective phenomena were to be considered irrelevant to sociological explanation. Rather, it means that insofar as such phenomena are appropriate materials for sociological explanation, they are effects and never causes. For instance, *Suicide* contains eloquent passages describing the distinctive subjective states that incline individuals toward either altruistic, anomic or egoistic suicide, and *The Elementary Forms* includes numerous accounts of the subjective states experienced by Australian aborigines as they participated in religious ceremonies. However, by treating all individual, psychological or subjective phenomena as effects only, Durkheim permitted them to enter sociological analysis while still adhering to his conception of the emergent nature of social reality. Even in one of Durkheim's most extensive treatments of subjective orientations, a chapter entitled "Individual Forms of the Different Types of Suicide," he (1951:277-94) continues to emphasize the social causes of suicide. Thus he quickly by-passes individual variations in situation and psychological temperament and classifies acts of suicide only according to the portions of their subjective meanings which are direct reflections of their social etiologies.

their different explanations of social order. Consistent with his view of the human divorced from society as an antisocial, immoral and egoistic being, Durkheim felt that persons' pursuit of their own individually defined and therefore egoistic, immoral interests led to social disorder. Hence, he (1950:1-13) invariably sought the cause of social uniformities in strictly social constraints, i.e., the moral rules embodied in occupational associations (1960:1-46; 1951:378-84), the state (1958), schools (1961), political, religious, familial and military society (1951:152-276), or society as a whole (1965). This perspective contrasts with Weber's (1968:30) observation that "many of the especially notable uniformities in the course of social action ... rest ... entirely on the fact that the corresponding type of social action is in the nature of the case best adapted to the normal interests of the actors as they themselves are aware of them." He considered both religious ritual and order in the marketplace to be social regularities generated in this way (see below), and he saw structures of domination as supported in varying degrees by self-interest (1968:37, 212-4, 941-3, 946-7).

In sum, Durkheim's dictum (1950:110) that "*The determining cause of a social fact should be sought among the social facts preceding it and not among the states of the individual consciousness*" (Durkheim's italics) may be contrasted with Weber's (1968:15) assertion that "subjective understanding is the specific characteristic of sociological knowledge." Weber would have rejected Durkheim's "social realism" as reification, while Durkheim would have rejected Weber's approach as psychological reductionism and, therefore, not truly sociological.

CONVERGENCE ON THE VOLUNTARISTIC THEORY OF ACTION?

In short, we must question Parsons' general assertion of convergence and his specific claim of convergence on the voluntaristic theory of action. Parsons' explication of this theory stressed two points. First, the theory of action is predicated on recognition of the importance of subjective states; everything is seen from the viewpoint of the actor (Parsons, 1949:46). Second, Parsons (1949:44-5) emphasized the importance of norms: not only does voluntarism depend on actors' adoption of a normative orientation (1949:81-2) but, indeed, "there is no such thing as action except as effort to conform with norms" (Parsons, 1949:76-7). Clearly Weber and Durkheim cannot be said to converge on a voluntaristic theory of action unless they agree on the place of subjective states and normative elements in sociological theory. We have already seen that Durkheim rejected the use of subjective phenomena in sociological explanation; thus he cannot be considered an action theorist, voluntaristic or otherwise.

Furthermore, Weber and Durkheim accorded normative elements different places in their respective social theories. Durkheim's work can be characterized generally as the sociology of the moral life. He saw moral rules as powerful social forces regulating individuals. But whereas Durkheim saw shared norms as decisive influences on behavior, Weber (e.g., 1968:25-6, 30, 214, 312, 316) sharply circumscribed the importance of normative elements in actors' subjective orientations, stressing instead the importance of habits (e.g., 1968:25, 29, 31, 792) and self-interests (e.g., 1968:30).

Parsons cited numerous examples of allegedly parallel conceptions within the two theories. Of these, he (1949) seemed to consider similarities on the issues of market behavior, moral respect and compliance, and common values as the most important support for his thesis. Yet it can be shown not merely that these examples fail to demonstrate convergence but also that they underscore the fundamental theoretical differences just noted.

Market Behavior

In the case of market behavior, Parsons (1949:529) argued that both theorists considered ethical discipline necessary in an individualistic economic order. Noting that Durkheim believed the market to be socially regulated by moral norms, Parsons (1949:529, 660-1) stated that Weber's notion of the universal fair play of Calvinist sectarians was equivalent to the noncontractual elements of contract posited by Durkheim. But this equation exaggerates convergence, principally because Weber viewed the influence of norms on

market behavior as highly variable and even denied the importance of norms in the typical case.

Weber identified Calvinism as an example of the *exceptional* instance in which ethics made a strong impact on the market. He felt that among those religions he analyzed, universal fair play had attained ethical status only in Christianity (see, e.g., 1968:599, 614-7). But in addition, ethical discipline in economics is necessary only before a market system is dominant (1958:181-2), and economic discipline was significantly strengthened by religious ethics only in pre-Enlightenment, ascetic Protestantism (1958:178-82; see also 1946:307-9, 313, 319-21). Thus, Weber (1958:181-2) distinguished between a period when the Protestant Ethic had an influence and the market had none, and a period including and following the Enlightenment when, because of the dominance of market institutions, modern economic behavior needed no religious stimulus. He considered direct religious influences on economic action to be minimal during the latter period.

When we set aside this exceptional case, we find that Weber's analysis of market behavior stressed individual interests. Observing that much social action is "not determined by orientation to any sort of norm," Weber (1968:30) added that "this is above all true of economic action, for example, the uniformities of price determination in a 'free' market." Elsewhere he (1968:202) argued that "all economic activity in a market economy is undertaken and carried through by individuals acting to provide for their own ideal or material interests." Dealers attempt to obtain satisfaction of their own self-interest while expecting that others will do likewise. This shared orientation to self-interest produces regularities and uniformities of the market place, "which are often far more stable than they would be if action were oriented to ... norms and duties" (1968:30). Orientation to constellations of interests brings about results comparable to those that norms seek to enforce, often in vain (1968:30). In short, mutual self-interest effectively regulates the market.

All of this is in direct opposition to the views expressed by Durkheim, who (1960:200-29) vigorously attacked explanations of order in the market (or in society generally) based on efficient self-interest. Durkheim argued that such a view was totally inadequate because it ignored the "noncontractual" elements that undergird and make possible these exchanges, and because it relies (illegitimately) on individual and psychological facts for sociological explanation. If people were guided primarily by individually defined interests, the inevitable result would be a complete breakdown of the social order. This argument was the core of his critique of utilitarianism (particularly as it was formulated by Spencer; see, for example, Durkheim, 1960:200-6; 1950:122-3), a critique that Parsons (1949:310-5) has warmly endorsed. Therefore, we might have expected that when Parsons encountered Weber's explanation of order in the market stated in terms of efficient self-interest, he would have treated it as just another version of a type of explanation that he and Durkheim rejected. On the contrary, Parsons claimed that the two explanations were *similar* on the grounds that both appeal to norms!

In thus finding convergence, Parsons misrepresented both men. He claimed that self-interested Weberian actors made choices on the basis of "norms of efficiency." Yet, Weber's discussion clearly and explicitly asserted that market action is oriented *not* to norms but rather to interests. Even if one allows Parsons to read "efficiency norms" into Weber, Durkheim's "noncontractual elements of contract" are moral rules which express social necessities and interests and as such are the opposite of "efficiency norms" in terms of which actors pursue individually defined self-interest.

Moral Compliance

Although Parsons (1949:652) perceived a common attitude of moral respect in Weberian legitimacy and Durkheimian moral obligation, close investigation shows that normative elements do not occupy the same place in the two concepts. In Durkheim's theory, moral obligation stems from an awe of the sacred and of society's overwhelming power. Moral sentiment, often awakened by ritual ceremonies, commands dutiful obedience. For Weber, on the other hand, a legitimate order holds no more than equal footing with self-interest in creating behavioral regularities. Since legitimate orders are not necessarily ethical in nature and not common to society as a whole,

they generally lack the moral power of Durkheim's collective sentiments and moral rules.

Of Weber's three types of legitimate domination, charismatic authority most nearly matches the content of Durkheim's moral obligation; yet even this authority does not typically convey the same high degree of moral force as does awe of the sacred in Durkheim's theory. Even though "it is the *duty* of those to whom he addresses his mission to recognize him as their charismatically qualified leader" (Weber, 1946:247; also 1968:244-5, 412-3, 432, 437-9, 445, 465-6), and the leader convinced of his charisma treats those who resist or ignore him as "delinquent in duty" (Weber, 1968:242), Weber nonetheless portrays the lay response to charismatic leaders as largely self-interested and practical (1946:249; 1968:492, 498-9).

For example, the magician is set apart from the layman by reason of his "capacity to achieve the ecstatic states which are viewed, in accordance with primitive experience, as the preconditions for producing certain effects in meteorology, healing, divination, and telepathy" (Weber, 1968:400). Similarly, if the charismatic leader is for long unsuccessful, "above all, if his leadership fails to benefit his followers, it is likely that his charismatic authority will disappear" (Weber, 1968:242). Lay acceptance of the ethical prophet's authority generally "in practice meant magic" (Weber, 1968:440). "This usually means that he is a magician, in fact much greater and more powerful than other magicians, and indeed that he possesses unsurpassed power over demons and even over death itself. It usually means that he has the power to raise the dead, and possibly that he himself may rise from the grave. In short, he is able to do things which other magicians are unable to accomplish" (Weber, 1968:467). The exemplary prophet appeals for related reasons. He, "by his personal example, demonstrates to others the way to religious salvation, as in the case of the Buddha. The preaching of this type of prophet says nothing about a divine mission or an ethical duty of obedience, but rather directs itself to the self-interest of those who crave salvation, recommending to them the same path as he himself traversed" (Weber, 1968:447-8).

Certainly charismatic legitimacy produces "complete personal devotion" to the charismatic leader. This effect is similar to the complete respect identified by Durkheim (1965:237): "When we obey somebody because of the moral authority which we recognize in him, we follow out his opinions, not because they seem wise, but because a certain sort of physical energy is imminent [sic] in the idea that we form of this person." Despite this similarity, however, it is also true that Durkheim (1965:238, 237; see also 1961:30-1) understood moral respect as affecting actions without "deliberation or calculation" or "*regard for any consideration relative to their useful or injurious effects*," while charismatic devotion rises instead "out of enthusiasm, or of despair and hope" (Weber, 1968:242). Charismatic devotion and obedience, although connected with duty, also depend strongly on the practical magical interests and personal religious needs and ideal interests that, Weber believed, motivate religious action. These entail pursuit of self-interest, a circumstance that contrasts with Durkheim's conception (1965:237) of the moral awe of society as leading individuals to deny individual ends in favor of the pursuit of social ends.

The two other types of legitimacy in Weber's (1968:212-301) typology of legitimate domination are even less connected to moral obligation. Conformity to traditionally legitimated orders is frequently habitual and unreflective, and actors, "in a very large proportion of cases," are "not even aware how far it is a matter of custom, of convention, or of law" (Weber, 1968:38). Conformity is partly upheld by "vested interests" (Weber, 1968:37) and, when actors submit, their obedience is *not* primarily normative but depends on "the fear of magical evils," other magical consequences, and "general psychological inhibitions" against change (Weber, 1968:37). These latter factors are inimical to Durkheim's concept of moral obligation, which excludes the consideration of practical self-interest (Durkheim, 1965:237-8).

Legal legitimation of an order, "today the most common form of legitimacy," enhances people's readiness to conform because enactments "are *formally* correct and . . . have been made in the accustomed manner" and it inspires conformity based on more than "fear or . . . motives of expediency" (Weber, 1968:37). However, laws "may have been established entirely on grounds of expediency" and may not "be regarded as belonging to

the realm of 'ethics'" (Weber, 1968:36). Since Weber distinguishes between law and ethics, stating that not all "legally guaranteed forms of order . . . claim the authority of ethical norms" (Weber, 1968:36), legal legitimacy does not necessarily imply moral obligation in the Durkheimian sense. Durkheim (1961:96) defined as one element of morality the good, i.e., "morality conceived as a desirable thing that attracts our wills to it spontaneously."

In sum, the only type of Weberian legitimacy that stresses ethical duty at all is charismatic legitimacy,[4] and even in this case various religious and ideal self-interests often contribute to obedience; this position contrasts strongly with the less egocentric morality described by Durkheim (1961:59), "the object of which is *society*." Whereas Weber saw legitimacy as one of several operative forces affecting conformity, one claim among many on behavior, Durkheim (1961:31) saw morality as holding an *absolute* claim: all "calculation is forbidden. . . . If duty speaks, there is nothing to do but obey."

Moral rules are powerful, Durkheim argued, because they are held in common by individuals who think the same way; the rules represent society and social interests even though they are spoken by individuals (1965:59, 238). This theme contrasts with Weber's (1968:37) contention that "it is very common for minorities, by force or by the use of more ruthless and far-sighted methods, to impose an order which in the course of time comes to be regarded as legitimate by those who originally resisted it." Those who resist an imposed order "give way," but often "the agreement underlying the order is not unanimous." Legitimacy does not imply agreement in the sense of Durkheimian morality, nor is obedience to a legitimate order necessarily a function of wholehearted support.

Common Values

Since Parsons emphasized common values in his own theory of action, it is not surprising that he (1949:670, 676n, 680-1; 1965:174) emphasized the importance of the common-value element in both Weber and Durkheim. He was certainly right in holding that Durkheim stressed the importance of common moral rules and ideals. However, the notion of values common to an entire society is rather foreign to Weber, who more typically analyzed the *differences* in ideas and ideal interests of different social, political and economic strata within a society. For example, Weber (1968:486-518) devoted much of his work on religion to explicating the systematic differences in religious ideas and interests among different status groups within a society. Intellectuals prefer sophisticated and relatively rationalized religious conceptions, nobles and knights prefer religious beliefs consistent with self-estimation of their own elevated honor and dignity, and peasants prefer magic and religions that help them to cope with immediate problems in this world and that promise them a better life in the next. In contrast to Parsons' stress on the similarity of religious conceptions of the members of a single society, Weber frequently noted similarities between groups similarly located in different societies, and differences between those differently located within the same society. Although he did not deny that similar religious concepts can sometimes be found throughout an entire society, he did not

[4] Parsons (1949:664-5) had noted that charisma is transferred upon the death of a prophet to the "objective system of rules," which thereupon acquire sanctity, legitimacy and "legality"; he (1949:663-5) therefore stated that charisma is the source of *all* legitimacy in Weber's scheme, including bureaucratic legality. He (1949:665, 669) asserted that it best engendered an attitude of respect and implied that this respect is transferred to the other types.

Although charismatic authority can become transformed in traditional and/or legal-rational directions (often for practical reasons; Weber, 1968:246), Weber (1968:252) made it clear that traditional or legal-rational authority derived from charisma is distinct from traditional or legal-rational authority that has not been charismatically derived: "A hereditary monarch by 'divine right' is not a simple patrimonial chief, patriarch, or sheik; a vassal is not a mere household retainer or official." When authority is not a routinized form of charisma, traditional authority and bureaucratic authority are legitimated by principles that are sufficient in themselves and "sharply opposed" to the charismatic type (1968:244). Since it appears that either type of noncharismatic authority may be legitimated without having a charismatic history, the kind of duty associated with a charismatic mission must remain, in Weber's scheme, a somewhat atypical characteristic of obedience to a legitimate order. This type of duty by no means characterizes all legitimate authority.

present this situation as typical. Although status groups and structures based on domination often involve numerous people similarly oriented toward an order, in Weber's portrayal an order is more likely shared by a corporate group (1968:48-53) than by a whole society.

Whereas Durkheim believed that in primitive societies, "everything is common to all" (1965:18) and "there can be no society which does not feel the need of upholding and reaffirming . . . the collective sentiments and the collective ideas which make its unity and its personality (1965:474-5), Weber saw no such social unity. He analyzed values (as in *Wertrationalität*) as a type of individual orientation rather than as a holistic property of society. Adopting the collective values of religious orders was considered by Weber to be an intellectual sacrifice (1946:155), while an ethic of individual responsibility represented the most rational form of value commitment (1946:127).

Clearly, then, the normative occupies a very different place in Weberian as compared with Durkheimian theory. Whereas Weber saw in actors' subjective orientations large measures of habit and self-interest intermixed with normative orientations, Durkheim saw moral elements—whether the collective conscience, collective sentiments, collective representations or moral rules—as powerful forces controlling the socialized individual. Beyond that, while Weber saw norms as social—they were typically developed and perpetuated in social contexts—he nonetheless held that norms refer ultimately to the subjective orientations of individual actors. In contrast, Durkheim viewed norms as a social, and therefore emergent, phenomenon; as such, they embodied the power of society itself, acting to control and direct the potentially disruptive outbursts of unsocialized personality, and providing the ultimate basis of social order itself. Durkheim would have felt that Weber failed to recognize the emergent nature of normative elements and their function as the very basis of social order; Weber, in turn, would have rejected Durkheim's view of norms as reification.

ADDITIONAL CONVERGENCES DISCUSSED BY PARSONS

To this point we have been discussing convergence with specific reference to the theory of action. However, in addition to his effort to demonstrate such convergence, Parsons also attempted to show the convergence per se of specific concept pairs (see 1949:682n, 720). As we shall see, the different theoretical orientations of these two theorists (demonstrated above) affected their conceptualizations; the pairs of concepts, embedded in contrasting theoretical contexts, differ in fundamental ways. Two instances of this alleged "translatability" were particularly important to Parsons: (1) Weberian "stereotyped tradition" and Durkheimian "ritual" (Parsons, 1949:676) and (2) "charisma" and the "sacred" (Parsons, 1949:665-9, 674, 717; 1947:75-6; 1967:50; see also 1954:32). Close examination shows that the differences within each pair are manifold and sometimes fundamental, reflecting basic divergence of the two theoretical approaches.

Stereotyped Tradition versus Ritual

Whereas Durkheim emphasized the collective nature of ritual, Weber (e.g., 1961:261) often associated tradition with individual self-interest and saw stereotyped adherence to tradition as being motivated by the practical, magical outcomes of adherence. Since Weber's explanation of stereotyped tradition suggests that the social fact of regular, collective ritual observance serves uniform individual ends, it qualifies for Durkheim's criticism of such an explanation. Not only did Durkheim feel that individual coincidences of motivation were unlikely and ephemeral, he also did not believe that individual desires for such social regularities could bring them about. "The need we have of things cannot give them existence, nor can it confer their specific nature upon them" for a social fact is not "a product purely of mental effort" that "may be produced at will whenever we find it necessary" (Durkheim, 1950:90). Consistent with these general perspectives, in his classic analysis of primitive religion Durkheim spoke of "a whole group of [religious]ceremonies whose sole purpose is to awaken certain ideas and sentiments, to attach the present to the past or the individual to the group. Not only are they unable to serve useful ends, but the worshippers themselves demand none." Furthermore, beliefs attributing "physical efficaciousness to the rites are wholly accessory and contingent" (Durkheim, 1965:423). Only col-

lective moral factors give the rite its character and being; clearly, this viewpoint is opposed to Weber's analysis of stereotyped tradition in terms of individual self-interest.

Charisma versus the Sacred

For Durkheim, the term "sacred" referred to things set apart and forbidden: there is a strict interdiction against contaminating sacred objects with contacts from the profane world (1965:52-6). There also is a strong respect or fear of the sacred. Individuals displace their feelings toward society onto religious symbols; therefore, sacred symbols are accorded the awe that society itself inspires. Since individuals see society's effects all around them and directly experience the pressure it exerts, they find it easy to accept the power of the sacred. Beyond social experience, which reinforces collective sentiments, the individual requires no proof or miracles in order to believe.

When Weber spoke of charisma, he meant first a person's special gift of extraordinary endowment, which, in the religious case, is a special magical or supernatural power (1968:241-2, 400-1; 1946:245-7). This may take the form of an ability to control the spirits magically (1968:427, 464), an ability to achieve states of religious faith or inspiration (1968:401, 566-7), the possession of divine grace (1968:572-3; 1958:110, 121-2) or, in the case of prophetic charisma, the ability to proclaim "a religious doctrine or divine commandment" (1968:439).

Several important differences between these concepts are apparent. While awe of the sacred comes from the power of society, charisma usually refers to properties of individuals (as defined by certain others). Sacredness normally resides in symbolic objects; charisma resides in individuals or in blood lines, or, having been separated from its individual sources, charisma may become immanent in organizational offices or in institutions—in every case, concrete social objects.

Since Weber did not hold that attitudes toward charismatic individuals or institutions are derived from attitudes toward an all-powerful society, attitudes of respect and fear are not necessarily extended to charismatic individuals. While Weber associated leadership and authority with magical and prophetic charisma, he made no such association with the other forms of charisma (1968:425, 440, 563). For example, in the case where charisma takes the form of the ability to experience constant faith, it engenders the power to rely perfectly upon God rather than on man (Weber, 1968:566-7). Where charisma does lead to the exercise of authority, obedience is only partly based on attitudes of respect. Although respect for charismatic leaders may be demanded (1946:246-7; 1968:562), the Biblical prophets cited by Weber did not enjoy widespread respect, nor did he claim that they did. Even saints may be regarded disrespectfully (thus, Weber's [1968:401]observation that nothing "has succeeded in deterring a south European peasant from spitting in front of the statue of a saint when he holds it responsible for withholding a favor"). While the sacred, as described by Durkheim, readily inspires respect, the charismatic leader must perform new miracles "constantly" in order to prove continuance of his charisma (1946:248-9; 1968:242-3). Finally Weber (1968:427) frequently portrayed the public as ready to turn away from (or against) magicians, prophets, saints and other charismatic figures.

Because Durkheim understood society itself as the source of sacredness, he viewed the relevant symbolic objects as sacred for everyone in the society. Consequently, everyone is expected to engage in the appropriate behavior with respect to symbolic objects, and individuals will find it difficult to resist the pressure to do so. The failure of any significant portion of the population to acknowledge the appropriate sacred objects and comport themselves appropriately is a sure sign that society is losing its power and suffering the malaise of insufficient integration. In contrast, within a given society, some individuals may respond to the charisma of one leader, others to that of another and some may not respond at all. In addition, whereas religious objects are sacred to everyone, the response to charisma is differentially spread throughout a given society.

Since society is the ultimate source of sacredness, the existence of society insures that sacredness and the symbolic objects embodying it will likewise be present. Charisma, on the other hand, is the unusual; there is no guarantee that charisma will necessarily be present in any given setting. A charismatic

irruption is possible in any social setting, but this possibility may or may not be realized. In modern, secular, legal-rational nations, Weber perceived few if any important embodiments of charisma, and in fact posited a decline of charisma in the modern world. As a quality tied to and derived from the special qualities of an individual, charisma itself is exceptional.

One final difference: as the very representation of society itself, Durkheim's "sacredness" exists as a fundamentally integrative force. In *The Elementary Forms* Durkheim made quite clear his view that sacred objects play an essential role in the ritual gatherings through which society, quite literally, recreates itself. In contrast, while the charisma of a leader serves to integrate his movement, the movement itself exists as a radical and divisive force.

CONCLUSION

Our intent has not been to argue against the value of synthetic theoretical constructions. Generally, and indeed in this particular case involving Durkheim and Weber, it is not only possible but also desirable to undertake the creation of theory after closely examining previous theoretical efforts. In making such examinations, one may visualize a construction capable of subsuming two or more previous constructions that are disparate in specific content and even in basic perspective. One may even argue, in a given case, that the encompassing construction was in some sense already implicit in the preceding constructions. However, it is inappropriate to read the later construction into its predecessors, as if to explicate their original meanings, because such an act can create (1) an insidious confirmation of the later construction (insofar as its predecessors had gained already a measure of acceptance) and (2) serious distortions of differences among the predecessors and excisions of, or redistributions of attention to, their various perspectives, forms and contents.

Our contention is that Parsons' analysis of the work of Durkheim and of Weber, culminating in his thesis that the two converged in important ways, contains serious misrepresentations of each; therefore, as a description of the works themselves, the convergence thesis is simply wrong. First, the vast difference in basic perspective separating Weber from Durkheim belies the thesis that they converged upon any single explanation of social behavior. Specifically, asserting convergence upon the voluntaristic theory of action implies that they shared basically similar views on normative elements in the explanation of human behavior; in fact, they disagreed sharply on the role of norms and values in human behavior. Second, Parsons' convergence thesis holds that both men employed theories of action; in fact, Durkheim emphatically rejected use of the action frame of reference for explaining social phenomena. Third, as part of his attempt to demonstrate convergence, Parsons sought to establish the translatability of certain concepts; far from demonstrating convergence, however, each of the proffered examples simply illustrates the major differences in theoretical perspectives. In short, not only does Parsons' thesis discount or ignore the gaps separating Weber and Durkheim as social theorists; in addition Parsons fails to demonstrate convergence at specific points.

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THE MARKET, TRADITION AND PEASANT REBELLION: THE CASE OF ROMANIA IN 1907*

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Two models of peasant rebellion are examined using historical census data relevant to the Romanian peasant rebellion of 1907. One model synthesizes the arguments of Wolf, Moore, Hobsbawm and Tilly. The other is Stinchcombe's. Each model is operationalized and tested using multiple regression techniques on county level data. The first synthetic model works well and shows that the most important variable explaining the intensity of the rebellion is the interactive effect of peasant traditionalism and the penetration of market forces in agriculture.

Some historians (particularly Mousnier, 1970) have claimed that every peasant uprising must be explained in purely particularistic terms. But actually there is some agreement among recent theoreticians about the general causes of peasant rebellions that have taken place in the last several centuries. Hobsbawm (1959), Tilly (1967), Moore (1967), Wolf (1969) and Stinchcombe (1961) have developed explanations that have a great deal in common.

Hosbawm (1959:67) writes:

> The irruption of modern capitalism into peasant society, generally in the form of liberal or Jacobin reforms (the introduction of a free land-market, the secularism of church estates, the equivalents of the enclosure movements and the reform of common land and forest laws, etc.) has always had cataclysmic effects on that society. *When it comes suddenly . . . its effect is all the more disturbing* (our emphasis).

Though Tilly's *The Vendee* is not exclusively about peasants, his general thesis is the same. The Vendee revolted against the Jacobin reforms of the French Revolution because it was an area that had been subjected to relatively rapid and recent commercial market forces, chiefly through the growth of a textile industry. This came in a region that had less commercial agriculture than neighboring regions, and therefore it had a profoundly disturbing effect. Neighboring regions that had long been engaged in commercial agriculture and had well-established commercial towns were not upset by the post-1789 changes and did not rebel against the Republican government (Tilly, 1967: particularly 114-9).

Moore, taking a much broader historical view than either Hobsbawm or Tilly, agrees with their general interpretation and rejects another widely held view, that the "revolution of rising expectations" had much to do with peasant unrest, at least in many of the most important cases of rebellion. He argues (Moore, 1967:474):

> In any event, one of the greatest dangers for an *ancien regime* during the earliest phases of transition to the world of commerce and industry is to lose the support of the upper crust of the peasantry. One common explanation is a psychological one, to the effect that limited improvements in the economic position of this stratum leads to greater and greater demands and eventually to a revolutionary

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outbreak. This notion of a "revolution of rising expectations" may have some explanatory power. It will not do as a general explanation. For both Russia and China, even in the twentieth century, it strains the evidence beyond recognition.... The timing of changes in the life of the peasantry, including the number of people simultaneously affected, are crucial factors in their own right. I suspect that they are more important than the material changes in food, shelter, clothing, except for very sudden and big ones. Economic deterioration by slow degrees can become accepted by its victims as part of the normal situation. Especially when no alternative is clearly visible, more and more privation can gradually find acceptance in the peasants' standards of what is right and proper. What infuriates peasants (and not just peasants) is a new and sudden imposition or demand that strikes many people at once and that is a break with accepted rules and customs.

Wolf generally agrees with Moore and takes up the theme of what he calls "middle peasants" (Moore's upper crust of the peasantry, middle in relation to large landowners). He stresses the fact that sudden creation of a large-scale market in land has profoundly unsettling effects on peasants long used to considering land as community or family property whose utilization ought to be subject to many social ties and requirements. Since impersonal market forces strip the land of these social obligations, and consequently render life insecure for many of the peasants, "... capitalism necessarily produces a revolution of its own" (Wolf, 1969:277-8). But at the same time, "The poor peasant or landless laborer who depends on the landlord for the largest part of his livelihood or the totality of it, has no tactical power" (1969:290). He is therefore less likely to initiate a rebellion. Wolf feels it is the middle peasant who is both able to initiate rebellion and who is most frightened by the growth of market forces. Further, the "... middle peasant forms a ... culturally conservative stratum ... which ... most depends on traditional social relations of kind and mutual aid between neighbors." It is the middle peasant's attempt "... to remain traditional which makes him revolutionary" (1969:291-2).

Stinchcombe reaches similar conclusions, but by a different route. He explains peasant unrest ahistorically in terms of comparative rural stratification. His basic thesis is that among all types of land tenure systems in agrarian societies in which there exist markets for agricultural produce, those systems characterized by family-sized tenancy will prove most volatile. The family-size tenancy system is one in which much or most of the land is owned by a relatively small number of owners who do not cultivate their lands as unified estates. Rather, small peasant tenants sharecrop or lease plots from landlords and farm them as they would ordinary small peasant plots. Thus, small peasant techniques and habits prevail in a society dominated by large landlords. The situation is volatile for five reasons.

First, there is a clear conflict of interest between the peasant and the *rentier* capitalist who tries to squeeze as much rent as possible from his tenants. Second, there is a conflict over risks to the peasant. Third, there tends to be little social contact between the landlord, who usually lives in a town, and the tenants who live in peasant villages. The landlord is thus an outsider. Fourth, there tends to develop a small class of prosperous, independent peasants. Though they do not suffer from the prevailing system as much as their poorer fellow peasants, they see the *rentier* class blocking their upward mobility, and being members of the village community, they form a class of natural leaders within the village against the urban landlords. Fifth, the peasants perform their work without supervision, and generally, without landlord investment or managerial skills. They therefore know that if the landlords could be excluded, there would be no loss of needed capital or technology. The peasant would simply get to keep a much higher proportion of his produce (Stinchcombe, 1961). This combination of facts makes family-sized tenancy agrarian systems exceptionally prone to peasant rebellion, and Stinchcombe documents this quite well, citing, among other examples, the case of Romania before World War I.

Thus, two general arguments exist: an historical argument (synthesizing Hobsbawm, Tilly, Wolf and Moore) stressing the survival of peasant traditionalism following the intrusion of capitalist market forces; and a structural argument (Stinchcombe) emphasizing comparative rural stratification. Both arguments stress the importance of the middle

peasantry, but in the more historical argument, the concern is with middle peasants as a bastion of traditionalism rather than as a class.

The major elements of the first argument include (1) the rapid penetration of market forces and (2) strong residual peasant traditionalism. This may be contrasted with the second argument to see which explains peasant rebellion better. We will also argue that a proper synthesis of Hobsbawm, Tilly, Wolf and Moore (for lack of a more precise term, we can label this the "transitional society" argument) must take into account the interaction between these elements, not simply the additive effects of all of them together. None of the authors would claim that any one element alone, no matter how strong, can explain peasant rebellion. A highly traditional peasantry in a non-market economy will not be more prone to rebellion than a somewhat traditional peasantry in a somewhat commercialized economy. It is the combined effects of traditionalism and of market forces that produces the potential for rebellion. In other words, the multiplicative interaction effects must be taken into account in order to test the theory.

In order to test these arguments, we shall look at one particularly intense but brief peasant uprising which took place in Romania in March, 1907. In a few weeks some 11,000 peasants were killed by the army (two of every 1,000 rural inhabitants). Though brief, the intensity of the uprising traumatized Romania and eventually became a major factor in producing a land reform some ten years later (Roberts, 1951:3-4, 21; for rural population in 1907, see Ministerul Industrii, 1909:40).

PRELUDE TO REBELLION

The agrarian history of Romania is quite complex; for our purpose it can be outlined very briefly.

In the middle ages Romania consisted of two distinct principalities, Moldavia and Wallachia. Rural inhabitants were agro-pastoralists who relied more on their animals than on the limited amounts of farming in which they engaged. The states were ruled by the princes and court aristocracies who derived most of their revenues from taxes they levied on the international trade that transited through the principalities. Thus, villages were taxed quite lightly; they were also free (there were few serfs); and they were communal (land belonged to the community, not to individuals, and it was not alienable to outsiders). The situation changed in the 16th century with the Turkish conquest of Romania and the decline of the international Black Sea trade. The states needed huge new revenues to pay the Turkish tribute and they had to tax the villages to get them. Flight from the villages became endemic to avoid taxes, and to stop it, serfdom was decreed. But not all villages were affected. In some areas, a free peasantry persisted. (For details on early Romanian states and their decline see Chirot, 1974.) It proved impossible, however, to turn a largely pastoral population into serfs. Flight remained common. Finally, in the mid-18th century serfdom was abolished, but villagers continued to have to pay six days of corvee obligations to lords. This was, however, a very light load. By comparison, in mid-18th century Russia the peasant corvee amounted to two or three days per week (Blum, 1964:445). Even later, when Romanian villagers were obliged to pay 12 days of corvee labor, this was hardly a crushing burden. Further, it was generally paid in kind or cash, rarely in labor. And in some areas, villagers retained many of their old freedoms (Mihordea, 1971:202-37).

The situation changed quite dramatically with the conversion from a primarily pastoral to a primarily cereal economy in the 19th century. In 1829 the Black Sea was opened to international commerce and Romania shifted to a wheat export economy. The lords gradually became true landowners instead of mere tax collectors; the villagers became cereal growing peasants rather than pastoralists. Further, because the land was so lightly populated and because there was a consequent labor shortage, the peasants were turned into serfs. By the middle of the 19th century, the peasants owed an average of 56 days of corvee labor per year. From the early 1830s to the mid-1840s wheat production and planted hectares of wheat went up some two and one half times. (Corfus, 1969:302-79; Emerit, 1937:229-36; for a discussion of the labor-scarcity theory of serfdom, see Domar, 1970 and Chirot, 1975.)

In 1859 the Romanian principalities were united *de facto* into a single country that became fully independent as a monarchy in

1878. In 1864 there occurred a land reform which ushered in the age of full capitalist agriculture and serfdom was abolished once more. Land was turned into private property and the communal village that had persisted in many ways since the middle ages was destroyed. The peasants received their own land, but the lords received the best lands and the peasants were left with too little on which to survive. They were therefore obliged to enter into sharecropping arrangements with the lords, and in this way the vital wheat exports could be produced (Garoflid, 1938:578-9). Tracing the growth of wheat production and exports in Romania reveals the impressive nature of the transformation which took place. (If precise figures existed from 1830, the change would seem even more dramatic.)

In the 1860s some 700,000 to 900,000 hectares of wheat were cultivated each year. By 1901-1904 the annual average was 1,600,000 hectares, and in 1905 and 1906 it reached 2,000,000 hectares. From 1880 to 1885 an annual average of 342,000 tons of wheat were exported. In 1905 and 1906 over 1,700,000 tons were exported (60% of the total wheat crop in 1905 and 55% in 1906). In the period from 1900 to 1904, wheat took up an average of 31% of all land cultivated in Romania. In 1905 and 1906, that proportion went up to about 39% of all cultivated land (Ministerul Industrii, 1909:146-7, 159). In 1906, cereals (mostly wheat) formed 82.5% of the value of Romania's total exports – the rest consisted almost entirely of lumber, petroleum, meat and vegetable products (Ministerul Industrii, 1909:495). It is quite clear that the intrusion of capitalism was both abrupt and intensive. The production of the key export crop rose dramatically, and the rate of growth seems to have taken an even sharper upturn in the early years of the 20th century. This had important consequences on the peasant way of life and on the entire rural social structure. Hobsbawm's description of the "irruption of modern capitalism into peasant society" might as easily have been written about Romania as about rural Spain or Italy.

The agrarian system that evolved has been called neo-serfdom (Mitrany, 1951:27). The unit of cultivation was chiefly the peasant family, not the large estate. Peasants owned land, but on the average, they did not have enough of their own land and they had to sharecrop the lords' lands to survive. Peasants were generally required to work first on the lord's portion, then on their own portion from which they took their share of the produce. This meant that in times of intense labor needs, particularly at harvest time, they had to work directly for the lord's benefit, and with whatever time was left, on their own portion before a frost or severe rain damaged the harvest (Creanga, 1907). The peasants used their own tools, took most of the risks and usually had to borrow cereal for food and seed in the spring. This was done at usurious rates such that peasants tended to fall farther and farther behind, never quite able to catch up to the previous year's debts (Roberts, 1951:14-6).

In 1905, 0.6% of all landowners owned 48.7% of the land, while 95.4% of the owners owned 40.3% of the land (Jormescu and Popa-Burca, 1907: Table 11, Part III). In other words, a few thousand families, the crown and monasteries owned close to 50% of the land while the million or so small peasant households owned 50%. By 1900 a system very close to that described by Stinchcombe prevailed. Indeed, Romania was characterized by a typical "family-sized tenancy system."

There were other aspects to the system. A large number of estate owners leased their estates out to estate farmers who paid a global rent for the property and then tried to recover their investments by squeezing a maximum out of the peasants. All of the crown lands and most monastery lands were also handled in this way. Thus, true "capitalists" (in the sense of being motivated purely by market forces and the profit motive) were in charge of much of the best estate lands. By 1902, 59% of all the arable lands in estates were being managed by these estate farmers (Jormescu and Popa-Burca, 1907:Table 9, Part III). To make matters more explosive, a large portion of these farmers were not Romanian. In Moldavia 40% were Jews (the use of "Court Jews" and other aliens in a somewhat different, but analogous context has been discussed by Coser, 1972) and about 10% were "other foreigners" (Jormescu and Popa-Burca, 1907: Table 9, Part III).

The final complicating factor was that throughout the 19th century the peasant population increased rapidly as new lands were turned from pasture to cereal. From 1800 to 1860 the population probably dou-

bled, and from 1859 to 1899 it increased 54% (Jormescu and Popa-Burca, 1907:18). In 1899, 81% of the population was rural (Ministerul Industrii, 1909-23). Thus, along with rapid commercialization of agriculture there was also overcrowding of the land. This played into the hands of the landowners who could thereby extract harsher tenancy contracts from the peasants.

By and large, Romanian peasants were still highly traditional. Among rural males over seven years old, the literacy rate was 26% in 1899. Among females, it was 4.5%. In the cities, by comparison 60% of all males and 38% of all females were literate (Ministerul Industrii, 1909:34–5). Even in the 1930s sociological studies of Romanian villages revealed great adherence to old traditions (Warriner, 1965:116–20). The crucial thing is that there existed a large gap between the rapidly changing economic circumstances and the continued adherence of the rural population to old ways of perceiving society. The low rate of literacy indicates a slow rate of change in that perception. Thus, while Romanian agriculture was being drawn ever more tightly into the world market and while demands on the peasants were increasing, the outlook of the peasantry remained bound to old patterns. This is a perfect example of Moore's statement about the nature of "new and sudden" impositions that seem to be "a break with accepted rules and customs."

The growth of the market, the retention of traditional attitudes, the rapid rise in demands placed on the peasantry, the transformation of property relations that created full private property in land and destroyed the commons, and finally, the emergence of a "family-sized tenancy system" should have provoked a peasant rebellion. It did. But it should be emphasized that the traits predicted to provoke rebellion were not evenly distributed throughout the country. Generally, wheat grew best in the plains, not in the hills and mountains. The large estates predominated in the plains. By and large, the plains peasants were also less traditional than those in the hills. But the dichotomy between hill areas and plains areas is a gross oversimplification, since these traits were not that simply distributed. In some hill areas there were rich valleys with cereal cultivation. Some plains areas did have highly traditional villages. Some plains areas had a more egalitarian distribution of land than some of the hill counties even though this was not generally the case. The concentration of middle peasants varied a great deal as well. There is therefore no overwhelmingly clear geographic pattern that explains these differences particularly when the country is examined on a county by county basis.

The intensity of the rebellion of 1907 also varied greatly from county to county. Some counties had virtually no outbreakes, while in others there was a great deal of violence. Nor was the pattern neatly constrained by geographic area even though, on the whole, the plains districts were more prone to rebellion. Still, some plains districts experienced no rebellion and some hill districts did. This high degree of variation from county to county is fortunate for our purposes since it makes a comparative analysis possible.

THE REVOLT OF 1907

There were local peasant uprisings in 1888, 1889 and 1900. But in March, 1907, a massive rebellion occurred (Roberts, 1951:4). In describing peasant violence in Andalusia, Hobsbawm has written (1959:79):

> . . . social movements tended to reach peak intensity during the worst months of the year—January to March, when farm labourers have least work, March-July, when the preceding harvest has been exhausted and times are leanest.

The same was true in Romania. The revolt began in Northern Moldavia and was first directed primarily against Jews who controlled over 40% of all estate lands and about 25% of all the land (Jormescu and Popa-Burca, 1907:Table 9, Part III). The government was caught unprepared. The revolt spread quickly and became focused against absentee landlords and estate farmers. The army was sent in, but to little effect. Bands of peasants threatened the cities. Soon, all Moldavia was in uproar, the hill counties as well as those in the plains, even though the intensity of protest was smaller than in the northern counties where the rebellion began. In all this, few people were killed even though houses were set on fire, Jews were mistreated and the peasants called for more land (Rosetti, 1907:611-4).

In Bucharest, the government was dismissed by the king because of its failure to crush the rebellion. One hundred twenty thousand men were placed under arms and the capital was ringed with troops. In Moldavia the rebellion soon lost steam. But as things were quieting down in that part of the country, in the south, in Wallachia, a more dangerous situation arose. Whole armies of insurgent peasants engaged in pitched battles with the army. In one county, not very far from Bucharest, a poorly armed peasant army of 10,000 was mowed down and dispersed. In whole counties the majority of villages arose, and flying columns of regular troops with cannons systematically demolished rebellious villages. In other counties, the rebellion was much more localized, but where it erupted, there was also bloodshed. After reaching a paroxysm of violence in the southern and western parts of Wallachia, the rebellion was brought under control (Rosetti, 1907:614-21; Hurezeanu, 1962:355-9; Seton-Watson, 1963:385-8).

All of those who have studied this rebellion (including Tucker, 1972 and Eidelberg, 1974) agree that in Wallachia it was far more violent than in Moldavia. In part, this may have been because the army was better organized and the government firmer by the time the rebellion spread there from Moldavia. But even at the very start, there had been some minor bloodshed. And in Wallachia, those counties that rebelled weakly also experienced only minor bloodshed. There were other factors at work, and in many counties of Wallachia the peasants seemed better organized than their Moldavian counterparts as well as more purposeful in destroying estates and murdering lords' agents. It was in these counties that there was the most severe repression and the most killing by both sides (Eidelberg, 1974:1-2).

After the rebellion, records were destroyed by the government to conceal the extent of bloodshed. It is therefore impossible to get a precise breakdown of deaths and property damage. But reliable accounts have been put together from county records, from newspaper accounts of that time and from telegrams sent to Bucharest by county administrators. From 1945 to 1967, Romanian historians put together all of the existing documents to produce the most comprehensive account of the events (Otetea et al., 1967).

MEASURING THE INTENSITY OF REBELLION

The intensity of rebellion varied according to the extent of its diffusion and according to the degree of violence associated with the rebellion.

The spread, or extent, of the rebellion in each county could be measured either by counting the number of rebellious events, or by counting the proportion of villages in which events took place. Both methods are problematic. If events are counted, there is the problem of villages in which multiple events took place and where, in many instances, separating events is very difficult, and somewhat arbitrary. If arrest records were available, this might solve the problem, but they were largely destroyed by the government and many arrests were never recorded at all. On the other hand, simply counting villages is somewhat misleading since in some villages the entire population was involved, while in others, only a few individuals were involved. Further, all of the data are not available. What is available is a compendium of events, narrated on a county by county basis, put together by 16 of Romania's leading historians (Otetea et al., 1967). We found counting events an impossible task because it often proved impossible to separate one event from another. But counting the villages in which events took place was not problematic. The Otetea account is extremely thorough and it includes an index of villages in which events took place (1967:868-907). The 1909 Statistical Abstract provides a count of the number of villages per county (Ministerul Industrii, 1909:11). The proportion of villages involved varied from 4% to 46%. It is clear that the Otetea account does not include all villages in which events took place and it describes some events that were so trivial as to be marginally relevant. Nevertheless, this single source is the best available, and the best that will ever be produced given the destruction of many documents by the old government.

The amount of violence associated with the rebellion is harder to measure. But there are approximate numbers of deaths recorded for each county. In particular, the Otetea group scanned all newspaper accounts and all available government reports and it provides many specific accounts of deaths. In most counties, there were fewer than 25 reported deaths. In

these counties, local newspapers reported most if not all of the deaths. In one county 74 deaths were reported. In six counties, there were hundreds of deaths, and here, newspaper accounts are not much help since even local news reports lost track of the number of people killed. But the Otetea account (1967:155-562) states that in four of the counties, there were many hundreds of deaths, and more probably over 1,000 per county. In two others, there were somewhat fewer deaths, but well over 100. A logical coding system therefore presented itself:

Code	*Deaths*
1	less than 25 (25 counties)
2	25 to 100 (1 county)
3	between 100 and 500 (2 counties)
4	over 500 (4 counties)

There is no absolute proof that this coding is perfect. But aside from the most widely accepted estimate of 11,000 deaths, there are a number of other estimates by reliable observers that permit some sort of comparison between the coding scheme and the total number of deaths. The French Embassy in Bucharest in 1907 estimated that there were between 10,000 and 20,000 deaths, and the Austrian Embassy estimated that there were from 3,000 to 5,000 deaths (Eidelberg, 1974:1). Taking our coding scheme into account, the lowest possible number of deaths would have been on the order of about 2,500. The highest possible number (assuming 4,000 deaths in each of the four principal centers of rebellion, 500 in the two other counties with intense rebellion, 75 in the county labelled "2," and 25 each in the other 25 counties) of deaths would have been a bit under 18,000 deaths. A more reasonable estimate, using the mid-points offered in the coding scheme, estimating about 2,000 deaths in each of the four main counties, yields about 9,000 deaths. Since greater precision is impossible and since this procedure yields reasonable results, we used it.

In defense of our procedure, we might point out that these estimates are based on the best judgment of 16 historians, not on the basis of a single author. They also correspond very closely to the subjective evaluation of the seriousness of the rebellion on a county by county basis in the other leading accounts (Eidelberg, 1974; Tucker, 1972; Seton-Watson, 1963:385-8; Hurezeanu, 1962:355-73; Rosetti, 1907:611-23).

Granting that the coding procedure is historically reasonable, however, does not explain why we used the values 1, 2, 3, 4 instead of the actual estimated number of deaths. There are two reasons for this. First, the estimates, while grossly correct, are not sufficiently reliable to justify a claim that the numbers should be used as if this were a continuous variable. Second, and more important, raw numbers of deaths are not adequate expressions of violence. Most of the killing, after all, was done by the army, not by the peasants. Most killings occurred where the peasants rebelled more strongly, but the act of repression magnified the number of deaths in these counties. The army did not create violence where there was none to begin with, but where 1000 peasants were killed, it is unlikely that the rebellious peasants were 100 times more violent than where 10 peasants were killed. (What does 100 times more violent mean in any case?) It is only certain that where 1000 were killed, the peasants were *more* rebellious than where 10 or 100 were killed. On these grounds, our coding system seems like the only way of quantifying violence, at least in this particular case.

To measure the overall intensity of the rebellion, we added the measures of spread and violence (after standardizing them).

THE ANALYSIS OF THE INTENSITY OF THE REBELLION

Rather complete census data exist for Romania because a series of nationwide censuses were taken between 1896 and 1905. Jormescu and Popa-Burca (1907) report the results of the various agricultural censuses (land distribution, amount of cultivated land by crop, etc.) while the Statistical Abstract of Romania for 1909 (Ministerul Industrii, 1909) reports some of the same data as well as others relating to population, literacy, foreign trade, agricultural production, etc. The availability of this data, organized at the county level, makes a statistical study of the various models of rebellion possible.

The counties are relatively homogeneous ecologically (that is, they are similar with respect to size and population and all but one are predominantly rural); the possibility of spurious ecological correlation is therefore

minimal. County size and population were entered into all the regressions tested and no significant biases were discovered. The small number of cases, 32 counties, does make statistical examination awkward, but not impossible. We could either assume that we had the universe of Romanian counties and that all statistical results would be significant, or we could apply the usual tests of significance. While we did have data on the universe of counties, we decided to apply the usual tests of significance to help us judge the relative importance of the various explanatory variables. Also, given the probability of measurement error in historical census data, this procedure contributes to the strength of our argument.

The best available indicator of traditionalism is the rural literacy rate. Many authors who have dealt with modernization have stressed the crucial nature of rising literacy in breaking down traditional ways of viewing the world and in promoting a more impersonal and rational outlook toward the modern economy and society. (In particular, see Levy, 1966:758-60.)

Literacy is not always a good measure of traditionalism. In highly literate societies, a low literacy rate within a particular segment of the population may simply indicate poverty. In some societies, where the literacy rate is almost zero, comparative literacy rates mean very little. But in transitional societies, where the literacy rate (particularly the rural literacy rate) remains low even while it is increasing, this rate is a good indicator of the relative penetration of new ideas (Huntington, 1968:32-3, 72-3).

The indicator of the spread of capitalist market forces is perfectly straightforward. Since wheat was by far the main cash crop, we measured this construct by taking the percent of the cultivated land in each county over a five-year period (1900-1904) devoted to wheat (Jormescu and Popa-Burca, 1907:152).

The relative strength of middle peasants was measured by taking the percent of the rural households that owned between 7 and 50 hectares. This fits with the economic analysis available for Romania (Warriner, 1965:117-8) which shows that an average household needed anywhere from three to five hectares of land to survive without leasing extra land. Seven hectares provided a safe margin. Fifty hectares was the conventional sized unit used in Romanian censuses to distinguish between peasants and small landlords. Peasants owning 20, 30 or 40 hectares were quite prosperous indeed (Jormescu and Popa-Burca, 1907:Tables 1 and 2, Part III).

The relative strength of the family-sized tenancy system can be represented by a Gini index of inequality of landowning. This was calculated according to the formula provided by Duncan and Duncan (1955:211). The number of households and the amounts of arable land held by them were computed for the categories 0-3 hectares, 3-7, 7-50, 50-100, and 100+ hectares from the census data (Jormescu and Popa-Burca, 1907:Tables 1, 2, 10, 11, Part III). The Gini coefficient represents the relative predominance of a certain type of tenurial system. In counties where inequality of landownership was greater, there were more peasants who held insufficient land of their own to survive. These were the peasants who were partly or entirely tenants, sharecropping landlord estates. In counties with a more egalitarian pattern, there were relatively fewer peasants who were partly or entirely dependent on sharecropping as more peasants had enough land of their own. (This indicator works for Romania, where there was little or no real plantation land. In plantation societies, a high Gini coefficient would indicate the relative preponderance of plantation land and of salaried workers, not of sharecropping tenants.)

OPERATIONALIZED TESTS OF THE TWO MODELS

The first, or "transitional society" model, synthesizes Wolf, Moore, Tilly and Hobsbawm. The basic argument is that peasant rebellion is most likely when there has been extensive, recent commercialization of agriculture in a traditional peasant society. Three versions were tested using the three dependent variables, Intensity of rebellion (I), Violence of rebellion (V) and Spread (S). One more variation was added. The models were first tested with Commercialization (C) and Traditionalism (T) entered additively. Then, the interaction of the two (T multiplied by C) was added. The expectation was that if this model worked, C and T would have a strong effect in the additive formulation, but this would be due to their joint presence. Adding the interactive term, the true predictor of rebellion, would greatly weaken or negate the separate

Table 1. Data by County

Northern Romania (Moldavia)	Rural Population in 00's	C	T	M	G	S	V	I
Bacau	1340	13.8	86.2	6.2	.60	9	1	-1.37
Botosani	1039	20.4	86.7	2.9	.72	32	1	0.67
Covurlui	629	27.6	79.3	16.9	.66	46	1	1.90
Dorohoi	1084	18.6	90.1	3.4	.74	23	1	-0.13
Falciu	612	17.2	84.5	9.0	.70	15	1	-0.84
Iasi	863	21.5	81.5	5.2	.60	26	1	0.13
Neamtu	980	11.6	82.6	5.1	.52	19	1	-0.48
Putna	948	20.4	82.4	6.3	.64	15	1	-0.84
Roman	753	19.5	87.5	4.8	.68	22	1	-0.22
Suceava	953	8.9	85.6	9.5	.58	16	1	-0.75
Tecuci	839	25.8	82.2	10.9	.68	22	1	-0.93
Tutova	727	24.1	83.5	8.4	.74	7	1	-1.55
Vaslui	797	22.0	88.3	6.2	.70	19	1	-0.48
South Central Romania (Muntenia or East Wallachia)								
Arges	1472	24.2	84.9	6.1	.62	7	1	-1.55
Braila	680	30.6	76.1	1.3	.76	19	1	-0.48
Buzau	1510	33.9	85.5	5.8	.70	12	1	-1.10
Dambovita	1560	28.6	84.2	2.9	.58	11	1	-1.19
Ialomita	1344	36.5	78.1	4.3	.72	11	1	-1.19
Ilfov	2004	40.9	84.4	2.3	.64	15	1	-0.84
Muscel	796	6.8	76.3	3.6	.58	9	1	-1.36
Olt	1058	41.9	89.7	6.6	.66	25	4	2.83
Prahova	1890	25.4	83.2	2.5	.68	13	1	-1.02
Ramnicu-Sarat	955	30.5	80.2	4.1	.76	32	2	1.59
Teleorman	1543	48.2	91.0	4.2	.70	42	4	4.33
Vlasca	1488	46.0	90.5	3.7	.68	36	4	3.80

Table 1. (Continued)

South West Romania (Oltenia or West Wallachia)	Rural Population in 00's	C	T	M	G	S	V	I
Dolj	2415	45.1	85.5	5.1	.64	36	4	3.80
Gorj	1303	12.5	83.8	7.2	.50	5	1	-1.72
Mehedinti	1789	39.3	85.6	4.9	.60	13	3	0.84
Romanati	1417	47.7	87.6	5.2	.58	33	3	2.61
Valcea	1376	15.2	87.3	10.8	.42	15	1	-0.84
Eastern Romania (Dobrodgea)								
Constanta	833	11.7	82.3	81.7	.42	4	1	-1.81
Tulcea	645	25.6	80.1	68.4	.26	4	1	-1.81
Mean	1175	26.3	84.3	10.2	.63	19	1.53	0
Standard deviation	447	11.9	3.8	17.4	.31	11	1.08	1.78

Explanations for Table 1:

C = commercialization of agriculture, measured by % of arable land devoted to wheat

T = traditionalism, measured by % of rural population illiterate

M = middle peasant strength, measured by % of land owned in units of 7 to 50 hectares

G = gini coefficient of inequality of landownership

S = spread of rebellion, measured by % of villages in which rebellious events took place, as reported in Otetea et al.

V = violence, measured by number of deaths, with 1 = 0-24 deaths, 2 = 25 to 100, 3 = between 100 and 500, 4 = 500+

I = intensity, measured by adding standardized S and V

effects of C and T, but the interaction term would be very strong, and the total amount of variance explained would rise above that explained by the simple additive formulations. The inclusion of the multiplicative interaction term is justified on theoretical grounds (it is the effect of both C and T that influence rebellion, not the separate effects of each alone) and on methodological grounds (see Blalock, 1969:156-62). The equations can be represented as follows (where A = a constant, and e = error term):

(1) the "transitional society" model

(a) $I = A_1 + \beta_{11}C + \beta_{12}T + [\beta_{13}CT] + e_1$

(b) $V = A_2 + \beta_{21}C + \beta_{22}T + [\beta_{23}CT] + e_2$

(c) $S = A_3 + \beta_{31}C + \beta_{32}T + [\beta_{33}CT] + e_3$.

The second, or "structural" model is based on Stinchcombe (even though elements of Stinchcombe's model are also found in the writings of the other theorists). In this model the emphasis is on class relations. The stronger the tenancy system (that is, the greater the inequality of land tenure) and the stronger the middle peasants, the greater the tendency to rebel. The model was operationalized as follows (where M = Middle peasant strength and G = Gini coefficient):

(2) the "structural" model

(a) $I = A_4 + \beta_{44}M + \beta_{45}G + e_4$

(b) $V = A_5 + \beta_{54}M + \beta_{55}G + e_5$

(c) $S = A_6 + \beta_{64}M + \beta_{65}G + e_6$.

The expectation was that M and G would have strong positive effects.

Since the two arguments actually approach each other obliquely, we also tested a combined model which included both the "structural" and the "transitional society" model. It is possible, for example, that the variables relating to class relations have a much greater effect when tested in conjunction with the variables relating to the process which takes place in transitional societies. The full model can be operationalized as follows:

(3) the "full" model

(a) $I = A_7 + \beta_{71}C + \beta_{72}T + \beta_{73}CT + \beta_{74}M + \beta_{75}G + e_7$

(b) $V = A_8 + \beta_{81}C + \beta_{82}T + \beta_{83}CT + \beta_{84}M + \beta_{85}G + e_8$

(c) $S = A_9 + \beta_{91}C + \beta_{92}T + \beta_{93}CT + \beta_{94}M + \beta_{95}G + e_9$.

A test of the full model allows a more definitive statement of the relationship between the two perspectives.

Multiple regression techniques were used to test the different models. The raw data are reported for each county in Table 1. The zero order correlation matrix for all variables (except population, which is added only for the information of the readers) is reported in Table 2. The summary of the regression analysis is reported in Table 3.

RESULTS OF THE ANALYSIS

The "transitional society" model works quite well and the "structural" or more static model works poorly.

Several facts explain the failure of the Stinchcombe model. First, middle peasants were strongest where the tenancy system was least widespread (R = −.685). Second, wheat cultivation tended to be more widespread where the landlords were strongest, that is, where the tenancy system was more widespread (R = +.315). It follows from this that the strength of middle peasants was not correlated with more extensive wheat cultivation (R = −.222). Third, the counties in which the middle peasants were strongest were not the most traditional counties (R = −.206). Since in Romania the middle peasants were stronger where the market *and* tradition were weaker, the "structural" model is the direct antithesis of the "transitional" model *for this case*, and the failure of one is explained by the success of the other. In some ways this is unfair to Stinchcombe since he actually stresses the presence of a tenancy system more than the presence of middle peasants. But even the strength of the tenancy system is a much poorer predictor of rebellion than the degree of commercialization and tradition. G correlates well with the spread of rebellion (+.409), but weakly with the degree of violence (+.137); and overall, G and M together explain 7% of the variance in the intensity of rebellion, none of the variance in the degree of violence and 14% of the variance in the spread of rebellion within each county. (All corrected for small N, see Table 3; correction according to McNemar, 1962.)

Stinchcombe may well have misspecified his variables. (An analogous argument was given by Paige, 1972.) The very existence of a tenancy system that extracts a high proportion of the peasant's produce implies that the landlord can market his share. Given a market

Table 2. Zero Order Correlation Matrix

	I	V	S	T	C	CT	M	G
Intensity	1.000	.889	.889	.454	.714	.776	-.268	.307
Violence		1.000	.582	.497	.756	.852	-.158	.137
Spread			1.000	.310	.514	.528	-.322	.409
Traditionalism				1.000	.294	.766	-.206	.116
Commercialization					1.000	.787	-.222	.315
CT						1.000	-.234	.211
Middle peasants							1.000	-.685
Gini coefficient								1.000

structure, it is in a tenancy system that peasant traditionalism remains strongest because the peasant continues to operate in a traditional family and village setting. Thus, it is quite correct that such systems produce unrest, but not primarily because of the nature of the land tenure system. Presumably, a tenancy system that has been established for a long time and in which the degree of traditionalism is quite low while commercialization of agriculture is quite high would lead to weak unrest. Only where the tenancy system is new, where the market is strong and where tradition remains strong would there be a high degree of unrest. Or, to generalize further, our case study suggests that tenancy systems are highly politically unstable where they are accompanied by high, new demands on the peasants. This was obviously the case in Romania in 1907. But because the tenancy system was an incidental variable, and commercialization and tradition were the key variables, the Stinchcombe model is a far weaker explanation of what happened than the "transitional" model.

The "middle peasant" argument cannot be dismissed so lightly. Both Moore and Wolf stress the importance of this class as a recruiting ground for peasant leaders. There is evidence that in Wallachia, where the rebellion was most intense, middle peasants were in the forefront of the rebellion (Garoflid, 1938:579). But the hypothesis is really meant to apply to individual actors, not to counties or even villages. We have shown that the percent of the land owned by middle peasants (an excellent indicator of their presence and strength) had no influence on the intensity, spread or violence of the rebellion, but county level data simply give no indication of what sorts of individuals were most active. Without individual level data, the hypothesis remains untestable, and while Stinchcombe, Moore and Wolf may be right in stressing it, the argument fails when transformed into an ecological proposition.

At this point, it is worth stressing what has been implicit all along. We have exaggerated the dichotomy between the two models for heuristic purposes. Each model is distinct, but the authors we quoted are not as tightly bound to one or another model as we have suggested. The arguments presented support the notion that a "static class" or "structural" model is not as strong as a "transitional society" model, but both models are basically "materialistic." In this respect, they both have much in common.

The "transitional" model proved quite satisfactory. The degree of commercialization of agriculture correlates highly with the degree of spread of the rebellion (+.514), with the degree of violence (+.756) and with the overall intensity of the rebellion (+.714). Tradition correlates more weakly with these, but when the effects of both are added, 56% of the variance in intensity, 64% of the variance in violence and 27% of the variance in spread are accounted for (corrected for small N, see Table 3).

Including the interaction term CT significantly improved the explanation of intensity (63% of the variance explained) and violence (78% of the variance explained). On the other

Table 3. Standardized Regression Coefficients

Regression Equation #	T	C	CT	M	G	R^2	R^2 corrected for small N
1a, additive	.267	.636				.575	.561
1a, interactive	(-.349)	(-.011)	1.053			.651	.627
1b, additive	.301	.668				.654	.643
1b, interactive	-.534	(-.209)	1.426			.793	.779
1c, additive	(.174)	.464				.292	.269
1c, interactive	(-.087)	(.189)	(.446)			.306	.258
2a				(-.108)	(.233)	.101	.072
2b				(-.114)	(.059)	.026	(-)
2c				(-.079)	.355	.171	.144
3a	-.419	(-.124)	1.162	(-.012)	(.142)	.670	.622
3b	-.515	(-.177)	1.395	(-.002)	(-.042)	.794	.764
3c	(-.230)	(-.044)	(.672)	(-.020)	(.294)	.388	.297

Significance level at .05; non-significant terms in ( ).

Equations 1a, 2a, 3a predict intensity; 1b, 2b, 3b violence; 1c, 2c, 3c spread.

1 refers to the "transitional" model; 2 to the "structural" model; 3 to the "full" model.

hand, it did not improve the proportion of the variance explained in the spread of the rebellion. It failed to raise the R^2 by very much, and in correcting for the small N, the inclusion of an extra variable caused an overall drop in explained variance. This is somewhat suggestive because it was only in explaining the spread (but not the amount of violence or the overall intensity) that Stinchcombe's model worked best. Though we do not want to push the argument too far, it may be that "spread" and "violence" of rebellion are really distinct attributes of the phenomenon. In measuring the spread of the rebellion, we included villages in which the only activity was vigorous petitioning of the authorities in order to redress the worse aspects of the tenancy system, namely high rents. But in many counties, such activity led to little or no violence as the peasants petitioning for a redress of wrongs were easily dispersed by police and army troops. Peasants may feel upset because rents are high, or because too large a surplus is being extracted from them, but they are far more likely to resort to violence if they are fully convinced that their claims are legitimate. In areas where tradition was strongest, the legitimacy of the new agricultural system was weakest. Thus, fortified by their stronger claim to a redress of wrongs, the peasants in such areas pushed their demands to the point of violence. The argument should not be overemphasized because even in explaining spread, C and T did better than M and G.

In equations 1a, 1b and 1c the inclusion of the interaction term in the first set of equations made T and C negative or insignificant. This was because CT accounted for such a large portion of the variance in the dependent variables. This fits the "transitional society" model which suggests that if C or T alone explain much variance, this is an essentially spurious correlation due to the fact that the rebellion will be strongest where both are present. In a peasant society in which C and T were not somewhat positively correlated with each other, one would not expect serious peasant rebellions.

As normally happens in interaction terms, both C and T were highly correlated with CT (Blalock, 1972:463-4). But because the theory suggests that the interaction term should be the most important term and because its inclusion significantly raised the explained variance for intensity of rebellion, we were convinced that this did not raise serious problems.

In the "full" model the results were substantially the same. CT remained the most important predictor variable and the inclusion of G and M did not make very much difference. Only in predicting the spread of rebellion did the land tenure system make much difference, and in no case did the relative strength of middle peasants help at all. The total R^2 increased a bit in all equations, but after correction for small N (see Table 3), the "full" model explained intensity and violence less well than the simpler "transitional society" model.

THE ISSUE OF REPRESSION

Snyder and Tilly (1972) and Tilly and Shorter (1974) have recently stressed two major aspects of collective violence which we have ignored. Both of these are related. One is that collective violence is primarily a political act, carried out to gain certain political ends. The second is that those in authority will meet violent political action with force, and that the use of police and troops will diminish the long term likelihood of violence. In other words, effective repression makes a difference.

In the case of Romania in 1907, *repression certainly made an important difference since it crushed the rebellion* and prevented the overthrow of the entire political and economic system. But did it make any difference in any particular county that was different from the effect in other counties? Two facts are important. First, the entire rebellion lasted only a few weeks. It was not a matter of long term organization, protracted civil war, or even long term, active violent class warfare. Secondly, from the very start the government used the army to repress the rebellion, and from the very start there were deaths. Romania is not a large country, and even in 1907 it had a good railway and telegraph system. Troops were shuttled back and forth across the country, sent to the areas of greatest need. Therefore, in a sense, repression was evenly applied throughout the country, though of course there were local variations dependent on the officers in charge of particular detachments (Otetea, 1967:523-53). Most deaths were caused by the army, not by the poorly organized peasants. But all indications are that

extremely violent repression took place in areas where the peasants had begun murdering estate owners and their agents, and the massive killings in Southern Romania occurred where the peasants fought back against the army (Eidelberg, 1974:223, 227). The army did not create violence where there was none, nor did it prevent violence where the peasants were most rebellious. When the government changed in the midst of the rebellion, this did not signal a major policy change. The group in power at the start of the rebellion found itself "... unequal to the task of restoring order ... and the king summoned the Liberals" (Seton-Watson, 1963:386). Even after the change, some counties remained relatively quiet, while in others the rebellion spread and became more violent. It is correct that most of the deaths occurred under the new government, but that was primarily because the rebellion spread to the south after it had started in the north, and in the south the peasants were more violent than in the north.

As for the long term conclusions about the organization of collective violence and its repression in Snyder and Tilly and Tilly and Shorter, these are basically irrelevant in explaining spontaneous outbursts by relatively disorganized peasants. On the other hand, had the rebellion of 1907 not been crushed and had it lingered on as a peasant "war of liberation," then, certainly, long-term political acts and the pattern of repression would have had to be entered into any explanation of events. In that case, it is likely that our original formulation would have remained valid (that is, those counties that had the most intense outbreaks of rebellion would have become the centers of the peasant war), but further variation in rebellious activity would have occurred according to various political and military factors.

SOME GENERAL CONCLUSIONS

We should begin by noting what we have not done. We have made no attempt to explain the exact timing of the rebellion (why 1907 and not 1906 or 1908). Given the fact that the counties in Romania were contiguous, that they existed within a single polity, and given that the rebellion started in one particular county, it is easy to account for its spread throughout the country, and it is possible to explain why the intensity varied from county to county. But with the data at hand, an explanation of why it started when it did is not possible. Certainly, the "transitional" model would predict that such a rebellion could not have begun before the spread of wheat cultivation. Nor could it have begun after traditional peasant ways had been eradicated. But this still leaves decades during which rebellion would have been likely. As stated at the start of the paper, there were, in fact, sporadic rebellions throughout the late 19th century. Why the rebellion of 1907 was so much more serious than the rebellion of 1888 remains an open question until someone discovers data from 1888 that pertain to the extent of commercialization of agriculture at that time, and to the extent of literacy. Unfortunately, no such data are likely to turn up.

We cannot overgeneralize from our single case study. At a minimum, however, the study is a solid confirmation of some of the leading theories of peasant rebellion. At a more general level, it specifies the most important variables that must be studied to explain the phenomenon of peasant unrest. As such, it is at least suggestive.

Our general conclusion is that in a rapidly modernizing society there is an optimal period in which peasant rebellion may occur, before traditional peasant ways and attitudes are destroyed. Once that point has passed, the likelihood of peasant rebellion decreases markedly. The theoretical explanation of this point lies at the very heart of our argument, and it therefore requires fuller explanation.

The issue of peasant traditionalism has led to numerous debates. In Romania a traditional outlook made the peasants expect a certain economic structure that was relatively undemanding, that is, that extracted a relatively low surplus from their production. It also led them to expect a certain amount of security. In the centuries preceding the intrusion of capitalist forces, that security existed in the form of a large reservoir of common lands that could be used by those who needed it. Peasants who remained more traditional held these expectations more strongly (Stahl, 1969). The commercialization of agriculture contradicted these expectations. Common farmland, forest and pasture vanished as landlords gained private property rights over the land and as cereal farming took over increasing amounts of land. A highly commercialized

agrarian economy did not produce unrest by itself, any more than did traditionalism by itself. It was only when these two were present together, where the effects of one magnified the effects of the other, that there was a high degree of unrest. This is not an argument about "relative deprivation" because it has little to do with actual wealth. The gap was one between expected level of security, and the actual high insecurity of a rural economy in which world prices and the impersonal market determined the availability of land and the actual reward received by the peasant producers.

This does not answer the question of why certain areas remained more traditional than others. We are not in a position to explain that without going into a county by county history of social change from at least 1700 to 1900. The varying levels of traditionalism were connected to the varying patterns of lord-villager interaction, to the position of various counties relative to trade routes, with migration patterns and with the geographic nature of each county. Specifying all these variables, and finding out how they interacted, is work for a whole monograph on Romanian rural society.

In a sense, then, our use of the concept "traditionalism" may be misleading. We are not suggesting that Romanian peasants were "primitive," but only that the rate of economic change was much more rapid than the rate of change in the peasantry's outlook on society in certain counties. Presumably, along with low literacy levels there also existed a host of other attitudes perpetuated by a certain kind of social structure. It would have been interesting had we been able to measure other relevant attitudes more directly, particularly those relating to feelings about the propriety of heavy rents and the alienation of common lands. But data were not available. It would also have been interesting had we been able to describe the kinds of village institutions which perpetuated old attitudes.

Our theoretical argument leads to a paradoxical conclusion, but one that has been prominent in the development literature (e.g., Huntington). The quicker the rate of economic change, the more difficult it will be for social forms and attitudes to change at the same rate. Thus, the danger of peasant unrest will be particularly acute. But at the same time, if economic change is very rapid it will destroy old social forms and attitudes more quickly, and the period in which peasant unrest might occur will be shorter. If economic change is slow, the period of potential unrest will be correspondingly longer because old social forms and attitudes will persist longer. But the danger of unrest will be less acute. That seems to be the choice, a short period of extreme unrest or a long period of low but persistent unrest. A brief outline of Romanian agrarian history after 1907 will illustrate this conclusion.

After World War I there was a land reform. Land was more equitably distributed, and even though population growth kept the peasants poor, the pressure of the market greatly decreased (Mitrany, 1930; Warriner, 1965:153). By 1934, only 1,300,000 hectares of wheat were being cultivated in "old Romania" (that part of post-war Romania that had formed the pre-1918 kingdom). This was only 65% of the surface devoted to wheat in 1906. In 1935, the number of hectares of wheat rose to 1,600,000 hectares, but this was still considerably below the highs of 1905 and 1906. Yields per hectare fell from pre-war levels as peasants were no longer forced to produce for the lords. Exports never attained pre-war levels (Institutul Central de Statistica, 1936:182-4, 312-5). For all the poverty in rural Romania between 1920 and 1944, there were no peasant uprisings (Roberts, 1951).

Since World War II Romanian agriculture has been extremely commercialized (by socialism rather than capitalism). But collectivization, massive industrialization, a large scale literacy campaign and migration to the cities has broken the traditional Romanian village (see Montias, 1967). The peasants may have many reasons to be discontented, but the probability of a peasant uprising is minimal. As in the rest of Europe, the *jacquerie* is a subject of historical analysis, not a present threat to contemporary stability.

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WHY ASCRIPTION? PARTS OF A MORE OR LESS FORMAL THEORY OF THE FUNCTIONS AND DYSFUNCTIONS OF SEX ROLES.*

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An analysis of the functions of sex-role ascription is based upon the assumption that many societies are organized around the conjugal family as the basic economic unit and that there are skills which each family must possess to be "successful." Formalization of the analysis concludes that when most families consist of male-female pairs (with or without children), when mating is voluntary, when the number of skills needed for successful family maintenance is large and when most people are capable of acquiring the skills, the utility of ascription is maximal. However, when there is some scarcity of inherent aptitude and when societies are capable of identifying those individuals with aptitude, the functionality of ascription diminishes, and ascription eventually becomes dysfunctional. *It is argued that the latter set of conditions are precisely those which tend to characterize modern industrial societies.*

Consider the following statement: "Man is born free, yet everywhere he is in chains." For a sociologist the statement contains a paradox. It combines an observation of empirical regularity—the second clause—and a negative moral judgment regarding that regularity. But the sociologist might ask, given that the negative moral judgment itself arises from the culture, why doesn't the culture adapt its empirical characteristics to satisfy its morality, or *vice versa?*

To answer his own paradox the sociologist generally chooses one of two stances: (1) Some version of conflict theory, in which he points out that morality isn't important in cultural development; that only the working out of competing interests determines the path of history. For the specific case, he would usually argue that the group in the culture which profits from man's chains is historically very strong, so that it has been able to exercise its will. (2) Some version of functionalism, in which he insightfully shows that morality isn't really important in cultural development, at least not as important as some other, usually "latent" (un-obvious) need(s) of the culture (functions) which is (are) served by the presence of chains.

This paper is of the second type. It takes the question of sex-role ascription and asks why it exists. Ascription is usually defined as the extent to which individuals are assigned certain behaviors, rights, etc., on the basis of their membership, usually by birth, in certain categories such as race, sex or religion. Even before the women's liberation movement, American social science rendered a generally negative moral judgment on ascription as the basis for role allocation. Consider our discussions of caste. Or racism. Or feudalism. When one looks at sex roles, however, we find a glaring empirical regularity. They exist everywhere, if not in identical form. In every culture women and men are assigned disparate functions and patterns of behavior well beyond those dictated by clear physiological differences. Why?

The answer contained in this paper is simply that ascription can be useful under certain, historically dominant, conditions. Not just to some sub-group within the society, but to society as a whole. In other words, ascription can be functional, in that it solves in an efficient way certain kinds of problems with which society must cope. I might add

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that the argument is restricted to certain kinds of societies and delimited by a set of assumptions, but then, what reasonable argument isn't?

This paper is *not* concerned with specifying *what* roles are assigned to which sex. Men may even be called upon to bear children for all this theory cares. The roles in question are abstract, and it is only with their creation—the fact of differentiation—that I am concerned. Who does what may be explained best by some additional functional theory, or some historical argument. In fact, I would not argue that conflict does not play a role in generating sex roles. But I have always wondered why we do *not* have ascription by eye color, but do by sex, and in a way that is what this paper is about.

A final preliminary note. This paper has a somewhat peculiar format. It begins with a conversational presentation of the basic argument regarding the sources of ascription. The next section presents a formalized version of parts of that argument and contains a few additional points which grow out of the formalization. This is followed by some further discussion and suggestions for elaboration of the theory.

Mating: Similarity, Complementarity and the Search Process

My interest in the problem of ascription began with a remotely connected topic historically central to the family literature: what are the effects of similarity and complementarity on mate selection? The usual cute way of introducing this problem is with the question "Which aphorism is right, birds of a feather flock together, or opposites attract?" It didn't take long for some Solomon to quite reasonably conclude that both were right—it just depended on what characteristics you were considering. In the area of values, for example, similarity surely was the key. Democrats would marry Democrats, not Republicans. Two "progressive education" believers were better off together than either was with a McGuffey reader. On the other hand, the argument went, when "needs" were considered, complementarity was the thing. The Marquisa de Sade had better be a masochist, not a sadist like her husband, if she wants to have a happy marriage. Assertive people need dependent mates. Talkers need listeners—and *vice versa.* Two straight men make a very bad act.

In research, however, the theories work out only partially. There is little doubt about the similarity hypothesis. From Kerckhoff and Davis (1962) onward, there is plenty of evidence that similarities attract. Not only in values, but in all kinds of background characteristics. In part this arises from the effects of propinquity, and background working through propinquity, in part from the functional difficulties of dissensus in a highly intimate group.

Support for the complementarity hypothesis, however, has been much more difficult to find. A recent text by an author who has worked on this problem flatly states that "research does not indicate a consistent pattern of this sort" (Udry, 1971:210).

But there is something wrong with the notion that complementarity doesn't work. We can still predict best who will marry whom from a complementarity characteristic—sex. Trivial, you say. But is it really? May it not be that other complementarities don't predict *because they are so highly correlated with sex?* In other words, may they not be so important in the selection of mates that society virtually guarantees that *any* couple will be complementary by tying the relevant characteristics to sex roles?

To make this proposition clearer, consider a society in which all men are aggressive, and all women passive. Within each group there is some variation, but no woman is as aggressive as the least aggressive man. Research on complementarity would indicate no relationship between the degree of aggressiveness on the part of a given woman and the aggressiveness of her male mate. The most aggressive male would be as likely to marry the most as the least aggressive female. Complementarity does not work. Yet it does. Each couple contains one member from the upper half of the total aggressiveness distribution, and one member from the lower half.

Let us assume that this strange distribution of aggressiveness between the sexes is in fact the product of socialization, not genetics, and that this pattern of socialization exists for some reason. What could that reason be? Perhaps the institutional arrangements of that society are such that the units we here call families work most effectively if they possess a certain amount of aggressiveness coupled

with a certain amount of passiveness? If these traits were randomly distributed in the population regardless of sex, we could in no way be sure that any given couple would be appropriately matched—unless we took strong measures to control mating patterns. If we wished to have voluntary mating, on the other hand, we would almost certainly fail in a large number of cases to form families with the desired distribution of traits. All the evidence we have on voluntary mating indicates that factors other than passiveness/aggressiveness tend to control decision-making by prospective mates. These factors are rarely identical to those which contribute to felicitous marriages.

One inference from the above scenario is that the most important culturally specified complementarities between the sexes may be those which we are least able to perceive. Like many very basic cultural norms, they do not arise for conscious consideration because they constitute such strong assumptive givens. Even without this stronger argument, however, the notion that complementarity of characteristics other than organic do not strongly influence mating is unproven. It does not consider the question of characteristics culturally specified to be part of sex roles themselves.

What Characteristics—The Functions of the Family

Where should we look for the characteristics which might be expected to require complementarity in marriage? A functional analysis might provide the requisite suggestions. By looking for the societal functions currently performed by the family, we might identify the factors supporting sex-role training to assure the requisite skills and values within the family relevant to those functions.

Most functional analyses of the family start with the question "Why is there such an institution—what are the 'irreducible' functions which only it can perform?" Kingsley Davis has a clear answer:

> Replacement of the societal membership . . . (involving) the physical reproduction of new individuals, the nourishment and maintenance of these individuals during infancy and childhood, and the placement of these individuals in the system of social positions. . . . Like any other organized group the family in any given society will be found to perform, at least in part, certain other social functions such as economic production, care of the aged, political control, and physical protection. But its role in performing these other functions is usually a by-product of its primary role of reproduction. (1949:7-8)

But this relative ordering of the "functions" of the family doesn't seem appropriate for an understanding of the way the contemporary family actually operates. We married folk spend relatively little of our time "procreating." We spend some more time socializing children. Over the long haul, including pre- and post-children phases of life, we probably spend as much time entertaining and giving emotional support to each other. And most of all we spend an enormous amount of time and energy on what may be described as economic functions.

The American nuclear family, after all, is a bastion of socially approved communism in an avowedly capitalist economy. Generally it consumes as a unit, sharing the income and productivity of its members without much referent to who produced what. From each according to his skills, to each according to his needs is a rough approximation of the prevalent norms.

Most importantly, the production functions of the nuclear family include not only income-producing work performed for persons or agencies outside the family, but a dazzling array of instrumental actions which contribute directly to the standard of living and way of life of all family members. These often incorrectly termed "housewifely" labors could include building a summer cottage in the woods, finding a cheaper price for a car, taking out the garbage, dropping in at Chicken Delight on the way home, as well as the standard mowing of the lawn, cooking and cleaning.

A basic tension between the women's liberation movement and Good Housekeeping magazine seems to revolve around the perception of the meaningfulness, difficulty and distribution of these jobs. The former talk about repetitiveness, loneliness and dreariness which leads to lack of growth and pride. The latter points to the management functions and responsibilities of creative homemaking, and

the direct relationship between labor and consumption which often leads to gratification. For the moment, however, the important point is the enormous *potential* number and kinds of these behaviors. By our own labors we may substantially change the quality of our lives—and the effectiveness of these labors depends on our skills.

The above is part of the answer to the question with which we began this section—complementarity on what characteristics? We have distinguished four somewhat distinct areas which seem to have special functional importance for the family:

(1) procreation
(2) socialization of the young
(3) husband-wife nurturance and "tension-release"
(4) economic productivity and management.

Until now, research on "complementarity" has focused almost completely on the third of these. Yet sociological role theory would predict that it is precisely in *instrumental* areas that the division of labor is most likely to increase efficiency. (In "A theory of marriage," 1973; 1974, the economist Gary Becker has developed this argument in a related, but somewhat different manner.) We can see that the area of procreation is clearly dominated by such a division of labor. Historically, it has been argued that socialization and economic functions in the family have also been areas of high specialization. It is my position that such specialization is not only efficient historically, but that it retains its utility today, when even more different decisions and jobs may be done by individuals for themselves. More importantly, I believe that such specialization requires training—both of specialized skills such as cooking, and of attitudes towards certain jobs, such as cleaning sewers or wiping noses. Not everyone will have in their past the requisite skills and attitudes. And training everyone to them is hardly efficient, considering the other skills we also consider necessary.

Beyond the actual activities which husband and wife engage in together, they enact a perhaps even greater number of activities relevant to their roles as members of the nuclear family which are performed separately. And the allocation of these activities, the family division of labor, is part of their role relationship. Reiss describes the typical American solution to the division of labor as follows:

> Tasks seem to be divided rather sharply, and role specialization between husband and wife seems present... such a division of labor indicates that these are the traditional ways tasks are allocated because of reasons that used to, and in some cases still do, have roots in the differences between the sexes in physical and cultural training. Males are still trained more to be handy with mechanical tasks and tasks that take muscle power and females are still trained to know more about cooking and cleaning, and that seems the essential basis of the division of tasks in the family. (1971:72)

This description, however, does not do justice to the variety of skills necessary to the fulfillment of contemporary standards of living. The following passage from Udry gives a somewhat more graphic picture:

> When the wife becomes sick, family life usually undergoes a shocking rearrangement. The proverb states, "many a fallen woman has been forgiven, but never for falling ill." Husbands frequently discover this for the first time at the birth of the first child, but the strain is even greater when there are already young children. Suddenly the husband has to do everything. Child-care has to be provided first. Meals have to be prepared. Groceries have to be bought, dishes done, beds made, pets fed, bills paid, and all this must be added to the husband's employment schedule. Many families will have a hard time avoiding the feeling that "it was a dirty trick for mother to get sick and leave us all in the lurch." (1971:326)

And what of father/husband? Well, says Udry, everything can go on pretty much as usual.

Perhaps it can, but it might be too easy to overinterpret that fact. The real division of labor within the family gives to the husband (traditionally) the job of earning the external income. This is a major part of family labor. Consider, however, a farm family. Father in bed does *not* mean that things go on pretty much as before. No organization compensates for his loss—instead it is the family which must somehow milk the cows or perform those routine chores which await daily

attention. It is this character of the assignment of routine, repeated tasks within the household to the wife/mother which makes her contributions to the collective effort so apparent. The *management* work, coping with emergencies and improvements in living standard which are a large part of the traditional father's role may be delayed—but wives without husbands must learn to perform these tasks as well as earn a living, and life becomes much harder.

In summary, the collection of skills needed to be father/mother–husband/wife may be fairly impressive. There are obvious technical skills such as cooking, cleaning and maintenance work; there are attitudes toward cleanliness, physical labor, risk-taking and socialization; there are social skills concerning training of the young, coping with the marketplace, the use of threats and cajolery; the understanding of culturally specified aesthetic and normative sensibilities; and so on. It may be difficult for the society to pay the price of training everyone to all of these, or for anyone to spend the time and effort to acquire them all. It may also be necessary for all or most of them to appear in a family for the family to work.

A FORMALIZATION

The somewhat meandering discussion to this point contains the key elements of a much more precise statement of the sources of sex-role ascription. In this section such a formal model is presented, partially as a summary statement of the foregoing, but perhaps more importantly to clearly specify the assumptions upon which the previous assertions rest. As usual in such models, it is the weakness of such assumptions which provide the clearest basis for attack on the model and conclusions based upon it.

Some Definitions

For simplicity, let us begin with a population (Z) in which there are only adult males (M) and females (F) and in which each individual is a member of one and only one family unit (U) composed of one male and one female. That, is the number of males (N_M), females (N_F) and family units (N_U) are all equal, i.e.,

$$N_M = N_F = N_U = N_Z/2.$$

Let S be a skill distributed in the population. For illustration, provisionally define the skill as either present or absent for any individual. Let P_Z be the probability of any individual *not* possessing S, and P_M and P_F the probabilities of not possessing the skill for any male and female, respectively.

Since $N_M = N_F = N_Z/2$

$$P_Z = (P_M + P_F)/2.$$

If we assume that S is not *genetically* distributed by sex, we may now define *ascription* (w) in terms of the above probabilities. A *totally* ascribed S would be one in which all members of one sex, but none of the other sex possess S,

i.e., $P_M = 1, P_F = 0$

or $P_M = 0, P_F = 1.$

Under these conditions, individuals are being assigned behaviors on the basis of their membership in a given sex category. In contrast, when $P_M = P_F$ we know that *no* ascription by sex could be operative. Thus, let us take as our definition of ascription:

$$w = |P_M - P_F| /2.$$

In other words, ascription is defined as the extent to which members of one sex differ from members of the other in their acquisition of some skill (divided by two for convenience in subsequent calculations). Note that with this definition maximal w can only obtain when $P_Z = .5$.

Let us now define P_U as the probability that a family unit will not possess S—that is, P_U is the probability that *neither* the male nor the female member of a unit possesses S. As indicated in the previous discussion, we are postulating that it is "functional" for society to *minimize* P_U. The major questions in the following sections concern the relationship between w (ascription) and P_U under various conditions.

The Simplest Case

Given that society will train a specified number of individuals in S, how should these be distributed between M and F? That is, when P_Z is a constant, what is the optimal level of w for minimizing P_U?

P_Z and w have several important properties:

$$0 < P_Z < 1$$
$$w < P_Z$$
$$0 < w < .5$$
$$P_Z + w < 1$$
$$P_Z < w < 2P_Z.$$

In the simplest case, let us assume independence for any unit's male and female possessing S, i.e., where R is the correlation between P_M and P_F,

$$R_{P_M P_F} = 0.$$

Then, $P_U = P_M \cdot P_F$.

Let us arbitrarily set $P_M > P_F$, i.e., a higher proportion of males than females do *not* have the skill.

Then $P_M = (P_Z + w)$,
and $P_F = (P_Z - w)$.
So $P_U = (P_Z + w)(P_Z - w) = P_Z^2 - w^2$

and $\frac{\partial P_U}{\partial w} = -2w.$

Since P_Z has been set as a constant and w is positive, P_U decreases as w increases. Thus, *the greater the ascription (w) the less the chance any given family will not possess S (P_U)*—our basic point on the potential functionality of ascription.

Selective Mating

Obviously, this is not the only way to minimize P_U in the "real world." An alternative would be to mate M's and F's on a "matching" basis. M's with S would marry F's without S, and *vice versa.*

Define R, the *correlation* between P_M and P_F, as a measure of selective mating. As R approaches 1, M's and F's *with* S marry one another.

The definitions of P_U, P_M and P_F correspond to the cells shown in Table 1.

This simple 2X2 table allows us to define R in terms of the Phi coefficient, so that

$$R = \text{Phi} = (P_U(1-P_F-P_M+P_U) - (P_M-P_U)(P_F-P_U))/(P_F P_M (1-P_E)(1-P_M))^{1/2}$$

Solving for P_U

$$P_U = P_M P_F + R(P_F - P_F^2)^{1/2}(P_M - P_M^2)^{1/2}$$

$$= P_Z^2 - w^2 + R(P_Z^4 - 2P_Z^3 + P_Z^2 + (2P_Z - 2P_Z^2 - 1)w^2 + w^4)^{1/2}$$

$$\frac{\partial P_U}{\partial w} = -2w + R((2P_Z - 2P_Z^2 - 1)w + 2w^3)/(P_Z^2(1-P_Z)^2 + (2P_Z - 2P_Z^2 - 1)w^2 + w^4)^{1/2}$$

The term by which R is multiplied may be shown to always be less than or equal to zero (assuming the positive square root of the denominator).

Therefore, when R is negative (i.e., persons are marrying *dissimilar* spouses) it has the *opposite* effect on P_U than does w, and mitigates the effect of ascription. However, when R is positive—and, according to the similarity hypothesis this will be most of the time—the term including R becomes negative, and its effects supplement those of the first term. Since the magnitude of this second term also increases with w, ascription is seen to become more important for P_U as R increases.

Number of Skills

It would be unrealistic to suppose that only one S is important to the family. In fact,

Table 1. 2 x 2 Definitions of Probabilities

		MALE		
		Yes S	No S	Total
FEMALE	Yes S	$1-P_F-P_M+P_U$	P_M-P_U	$1-P_F$
	No S	P_F-P_U	P_U	P_F
	Total	$1-P_M$	P_M	1

we have argued above that the number might be reasonably large.

Let us define V as the sum of the probabilities of not possessing S's in a family unit;
i.e., $V = P_{1U} + P_{2U} + \ldots P_{jU}$, where j is a positive integer.

We now wish to minimize V.
Since

$$P_{jU} = P_{jZ}^2 - w_j^2,$$

$$V = \sum_1^j (P_{jZ}^2 - w_j^2) = \sum_1^j P_{jZ}^2 - \sum_1^j w_j^2$$

From this equation it is clear that V increases with j, since P_{jZ} is always greater than w_j.

More importantly, we can see that if the P_Z's are constants, V will be more sensitive to the average size of w as j increases. This becomes clear if we assume all S's have the same w. Then

$$\sum_1^j w_j^2 = jw^2$$

$$V = \text{constant} - jw^2$$

and $$\frac{\partial Z}{\partial w} = -2jw.$$

In other words, the greater j is, the more an increase in w decreases V. A higher mean level of ascriptiveness is therefore more "functional" as the *number* of relevant S's increases.

One-Adult "Families"

The simplification which has all unit families contain both a male and a female does not reflect a world in which many adults live either alone or together with children but without an adult partner. If such units are as important as "complete" families, the utility of ascription might be substantially undermined.

Let N_{MU} and N_{FU} be the number of unit families containing only one adult—a male or a female, respectively, and N_{DU} the number of families with both a male and female adult.

Then, $N_U = N_{MU} + N_{FU} + N_{DU}$.
Recall that is assumed that $N_M = N_F$. This implies that

$$N_{FU} = N_{MU}$$

Then, $N_U = 2N_{MU} + N_{DU}$

$$N_M = N_F = N_{DU} + N_{MU} = N_{DU} + N_{FU}$$

$$N_U \geqslant N_M$$

and

$$N_U \geqslant N_F.$$

If we assume that individuals are *randomly* distributed to single-adult or paired units within sex, then, by definition, (and again assuming $P_M > P_F$)

$$P_{FU} = P_F = P_Z - w$$

$$P_{MU} = P_M = P_Z + w.$$

From previous analysis, assuming the correlation between P_M and P_F is zero,

$$P_{DU} = P_M \cdot P_F = P_Z^2 - w^2.$$

Then,

$$P_U = \frac{N_{DU}}{N_U} P_{DU} + \frac{N_{MU}}{N_U} P_{MU} + \frac{N_{FU}}{N_U} P_{FU}$$

$$= \frac{N_{DU}}{N_U} (P_Z^2 - w^2) + \frac{N_{MU}}{N_U} (2P_Z) \text{ and}$$

$$\frac{\partial P_U}{\partial w} = -2w \frac{N_{DU}}{N_U}.$$

Again, assuming P_Z constant, we see that the effect of w on P_U varies *directly* with the proportion of units which are pairs (N_{DU}/N_U). In other words, as we might expect, ascription is *less* "functional" the smaller the proportion of married persons.

Selective Marriage

Just as we postulated selective mating, we might consider the possibility that remaining single or getting married is itself related to possessing S. In this case, two important possibilities exist: (a) the effect is in the *same*

direction for males and females or (b) the effect is in opposite directions.

For simplicity, let us assume that the *magnitude* of the effects of possessing S on males' and females' probabilities of marriage are equal. Then, in the first case, let h be an addition to P_M (beyond w) and P_F for being single. So that

$$P_{MU} = P_Z + w + h$$

$$P_{FU} = P_Z - w + h$$

$$P_{DU} = (P_Z + w - h)(P_Z - w - h)$$

$$= P_Z^2 - w^2 - 2P_Zh + h^2.$$

$$\text{Then, } P_U = \frac{N_{DU}}{N_U}(P_Z^2 - w^2 - 2P_Zh + h^2) + \frac{N_{MU}}{N_U}(2P_Z + 2h)$$

$$\text{and again, } \frac{\partial P_U}{\partial w} = -2w\frac{N_{DU}}{N_U}.$$

In other words, although the level of h affects P_U, it does not affect the degree to which w affects P_U.

In the second case, however, the results are different. If h is equal *and opposite* for males and females, and added to w for males,

$$P_{MU} = P_Z + w + h$$

$$P_{FU} = P_Z - w - h$$

$$P_{DU} = (P_Z + w - h)(P_Z - w + h)$$

$$= P_Z^2 - w^2 + 2wh - h^2.$$

Then,

$$P_U = \frac{N_{DU}}{N_U}(P_Z^2 - w^2 + 2wh - h^2) + \frac{N_{MU}}{N_U}(2P_Z)$$

and

$$\frac{\partial P_U}{\partial w} = \frac{-2N_{DU}}{N_U}(w - h).$$

Alternatively, if h were added to w for females,

$$\frac{\partial P_U}{\partial w} = -\frac{2N_{DU}}{N_U}(w + h).$$

Thus we see that h affects the degree to which changes in w will produce changes in P_U. The direction of this effect depends on the relationship of h to w. If the sex less likely to possess S (i.e., the "+w" sex) is also selected into marriage on the basis of possessing S (i.e., the "+h" sex) the effect of w on P_U is decreased (top set of equations, above). If the sex more likely to possess S is selected into marriage the effect of w is increased.

The Distribution of Genetic Aptitude

In a sense, all of the foregoing is a special case in which every individual has the capacity to be trained for S. However, part of the argument against ascription is based directly on the question of its efficiency when individuals inherit different capacities.

Let G be the probability of not possessing the genetic aptitude for acquiring a skill S and T the probability of not getting the training given by society toward the acquisition of S. Assume that both aptitude and training are necessary for acquiring S and (provisionally) that they are independent.

$$P_i = T_i + G_i - T_iG_i, \text{ for } i = Z, M, \text{ and } F.$$

$$\text{Then } P_Z = 1 - (1 - T_Z)(1 - G_Z)$$

$$= T_Z + G_Z - T_Z \cdot G_Z.$$

We should now *redefine ascription* (w) in terms of the society's decision to train (T). In our original definition of ascription, we noted that it depended on the degree to which society *assigned* people to roles on the basis of sex. To this point we have looked only at *outcomes*, i.e., who fills the role. But women are not *ascribed* childbirth, which is a function of aptitude. With the concept of

training we can more properly define ascription in terms of the process of assignment to training. Therefore,

$$w = |T_M - T_F|/2.$$

Note that when all individuals possess G (i.e., when $G_Z = 0$), w defined in terms of T becomes identical to w defined in terms of P. Then, following our previous procedures,

$$T_M = T_Z + w$$

and $T_F = T_Z - w$.

Let us further make the anti-sexist assumption that males and females do *NOT* differ on genetic aptitude for the S's with which we are concerned. Then $G_Z = G_M = G_F$.

Using all the above, plus the assumption of no selective mating, we can now write:

$$\begin{aligned} P_U &= P_M \cdot P_F \\ &= (T_M + G_M - T_M G_M) \\ &\quad (T_F + G_F - T_F G_F) \\ &= (T_Z + w + G_Z - T_Z G_Z - G_Z w) \\ &\quad (T_Z - w + G_Z - T_Z G_Z + G_Z w) \\ &= T_Z^2 + 2T_Z G_Z - 2T_Z^2 G_Z - \\ &\quad 2T_Z G_Z^2 + T_Z^2 G_Z^2 + G_Z^2 + \\ &\quad 2w^2 G_Z - w^2 G^2 - w^2. \end{aligned}$$

Hence,

$$\begin{aligned} \frac{\partial P_U}{\partial w} &= 4wG_Z - 2wG_Z^2 - 2w \\ &= -2w(1 - 2G_Z + G_Z^2) \\ &= -2w\ (1 - G_Z)^2. \end{aligned}$$

Since $0 < G_Z < 1$, $(1 - G_Z)^2$ also varies between zero and one. Therefore, the greater G_Z is, the less the effect of w on P_U, with the entire expression approaching zero as G_Z approaches 1. The higher the proportion of persons *without* genetic aptitude for S, the less "functional" ascription is. Interestingly, ascription is most "functional" when everyone has the native ability to acquire S (i.e., $G_Z = 0$). The "Scarce Talent" argument is supported.

The Effects of Search

The above assumes that society is completely inept in deciding who will and who will not get trained—T is random with respect to ability (G). A more realistic model would have T related to G, although far from perfectly. In this case, following previous methods,

$$P_Z = 1 - [(1 - T_Z)(1 - G_Z) + r(1 - T_Z - (1 - T_Z)^2)^{1/2}(1 - G_Z - (1 - G_Z)^2)^{1/2}]$$

where r = the correlation between T and G, and is assumed equal for males and females.

Then,

$$\begin{aligned} P_M &= T_Z + w + G_Z - T_Z G_Z - wG_Z \\ &\quad - r(G_Z - G_Z^2)^{1/2}\ (T_Z - T_Z^2 \\ &\quad - 2T_Z w - w^2 + w)^{1/2} \end{aligned}$$

and $P_U = P_M \cdot P_F$ = a very complex combination of the above.

The term which results from this product is not easily amenable to solution for the effect of changing r on the relation between w and P_U. Plotting solutions to the equation for various levels of T_Z, G_Z, r and w, however, reveals a complex set of relationships, as the examples in Figure 1 indicate.

Based on these plots, however, several conclusions may be reached. First, if training is not correlated with ability, increasing w decreases P_U, as found previously, i.e., ascription is functional. But if training and ability are "sufficiently" positively correlated, increasing w increases P_U. Thus, ascription is dysfunctional if talent is likely to be detected and trained.

It should be noted that when G is 0 "search" can have no effect on P_U, i.e., when everyone has the basic aptitude the notion of search is meaningless.

Summary of Results

The foregoing formalization argues for the following major conclusions:

1. Other things being equal, increasing levels of ascription (w) reduce the probability that a family unit formed by random mating will not possess a given skill (P_U).
2. Selective mating on S *may* reduce the functionality of ascription. However, if selective mating produces positive correlations between husband and wife on possession of S, the functionality of ascription is *increased.*
3. The greater the number of skills important to the family, the greater the functionality of ascription.
4. The greater the proportion of unmarried adults, the less the functionality of ascription.
5. The higher the proportion of persons with genetic aptitude for S, the greater the functionality of ascription.
6. The greater society's success in identifying and training those persons with genetic aptitude for a skill, the less the functionality of ascription. When this societal success is high, ascription becomes dysfunctional.

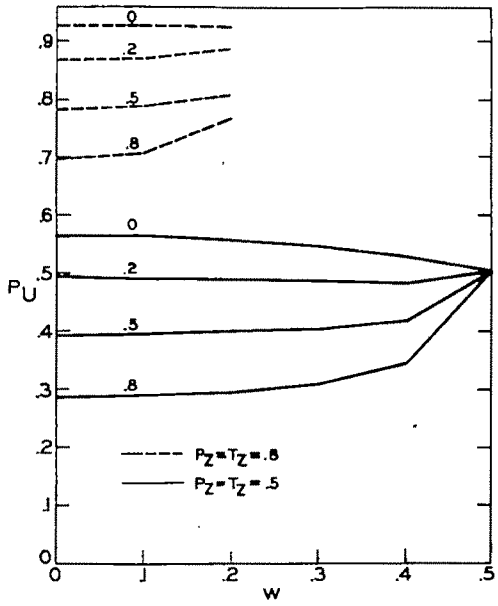

Fig. 1. Relationship Between P_U and w for Selected Levels of r, T_Z and P_Z.

Discussion–A Functionalist "History"

This paper has been "blue-sky" enough perhaps to warrant some further speculation regarding the implications of the foregoing model(s). In particular, I am impressed that whereas the paper begins with a search for the "positive" aspects of sex-role ascription, the final results probably argue for its demise. More specifically, it seems that ascription may be a phenomenon of historically specific functionality, whose day may finally have passed (or may have passed several centuries ago, only we were too culturally lagged to notice).

Consider the following "history" of the functionality of sexism. In the beginning–in so-called primitive societies–technology was minimal and everyone had the aptitude to learn any task. In other words, G = 0. Whenever G is common, ascription may be functional–for the reasons we previously cited. In addition, training costs may be moderate–no general educational system is used, but the tasks to be learned are not very complex. If "moderate" means that about half of all people get trained, this would again maximize the utility of ascription.

The development of technology produced more specialized jobs requiring (perhaps) some skills relevant to these jobs. This increased G. On the other hand, some system of education more efficient than that previously used was implemented. This compensated for the increasing costs of training produced by increasing task complexity. So T remained about the same, and the functionality of ascription, although reduced, was not abridged.

Finally, however, the costs of training for a large number of tasks were substantially reduced through general education. "Literacy" might be society's code-word for a population with the ability to learn more specialized skills fairly cheaply. At the same time, better means for identifying persons with the capacities to profit from training were developed. Universal education might have had the effect of bringing everyone to the attention of "the system" and screening each of them through an elaborate eight-year (or four-year, or whatever) "test."

As can be seen in Figure 1, these characteristics minimize the likelihood that ascription is "functional." If few people have appropriate aptitudes for several highly specialized tasks (say G = .8) and few will be trained (say T = .8), then regardless of the

capacity for search the ability of w to increase P_U is small. At most reasonable levels of r, on the other hand, w *increases* P_U. Ascription is dysfunctional.

And all of this without considering the "morality-based" dysfunctions of ascription in a generally individualistic culture. No word of resentments or feelings of inequity because of ascription, and the predictable reduction in effectiveness which accompany them. Of course, the model does assume that there are no inherent differences between males and females in G, and that assumption may be far from true. But I would be willing to bet on it for most skills.

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ERRATUM

In the article "Temporal Change in Occupational Mobility: Evidence for Men in the United States" by Robert M. Hauser et al. (June, 1975:pp. 279–297), a block of text has been repeated on page 286, column 1, lines 4–24.

THE STRUCTURE OF INTRAGENERATIONAL MOBILITY*

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This paper analyzes mobility rates for black and white men using life-history data on intragenerational mobility. Mobility is linked to the process of occupational achievement. It is argued that job mobility is generated by persons' attempts to maximize their status and income. Since opportunities for better jobs will be fewer the higher the occupational achievement already attained, the rate of mobility will depend on time in the labor force. The mathematical formulation of this time dependency is derived from a simple change model for the occupational achievement process. With this formulation, a redefinition of time is possible and job shifts in the redefined time scale can be described by a Poisson process. One component in the formulation of a realistic and theoretically meaningful stochastic model of mobility is then obtained. The empirical analysis indicates that the proposed model describes the observed change in mobility rates over time reasonably well.

INTRODUCTION

The distinction between intergenerational and intragenerational mobility is commonplace in research on social mobility. It is a distinction that apparently does not separate two equally important forms of mobility. Studies of father-son mobility clearly dominate the field. This emphasis seems easily justified. Intergenerational studies speak to a dominant concern of Western societies: the concern for equality of opportunity. The degree of "openness" of societies, as measured by father-son mobility rates, is generally held to be a major characteristic of societies. Svalastoga (1965) even constructs a typology of stratification systems based on the rate of intergenerational mobility.

Despite the prevalence of intergenerational studies, it can be argued that it is essential to study intragenerational mobility in order to achieve the objectives of mobility research. This is so even given the descriptive purposes that dominated early mobility research. The positions of fathers and sons in the social structure that are compared in measurement of the "openness" of societies are not fixed attributes of individuals over their lifetime. Rather all available evidence indicates a considerable mobility over age in industrial societies. Furthermore, a person's position seems to be systematically related to his age. The results of intergenerational mobility studies will therefore depend on the ages of fathers and sons used in the comparison. Some attention to patterns of intragenerational mobility is needed in order to obtain precise measurement and ensure comparability between different studies.

The need for intragenerational mobility data is even more apparent when the objective of research is causal analysis of mobility. A traditional distinction separates two sources of mobility: structural and individual. Research has rather sharply diverged according to which set of causal forces are analyzed. However, the causal analysis of both profits from the use of intragenerational data. This is to some extent already recognized in research on the effect of individual attributes on status that has come to be known as status attainment research. The causal models so successfully employed in this research use status obtained as the dependent variable. This status depends on a person's age, and already Blau and Duncan's (1967) pioneering

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study introduced status of first job in their analysis, and some analysis was done on age-variation in the process using synthetic cohorts. This work has subsequently been extended by Kelley (1973) and Featherman (1971), and in the research by Sewell and associates (for example, Sewell and Hauser, 1975) on a panel of Wisconsin youth. Although status attainment research shares the concern for equality of opportunity with traditional intergenerational mobility research, it is apparent that the research questions asked are intragenerational in nature, for they are questions about what determines a person's status (with special emphasis on the influence of the origin), and a person's status usually varies over time.

The analysis of the occupational achievement process found in status attainment research does not satisfy those who insist that there are important structural features of society that influence the process by which a person obtains a certain position in society. The existence of "barriers" to mobility and the influence of supply and demand for positions in the social structure are largely ignored in status attainment research. Direct analysis of mobility patterns seems needed.[1] Again the proper data source for an analysis of these structural causes of mobility would seem to be intragenerational data. Intergenerational data do not present the actual mobility experiences of any real cohort, nor do they enable the location in time of acts of mobility. Intragenerational data, in contrast, may permit the investigator to identify single acts of mobility precisely and to link those to structural characteristics of society. Although analysis of intragenerational mobility patterns are very scarce, the usefulness of such data for analyzing structural sources of mobility is apparent. The most successful attempt at analysis of intragenerational data probably is White's (1970) analysis of vacancy chains.

The analysis of intragenerational data presents problems. First of all, such data are difficult to obtain. They will either be prospective, in which case a panel is the most likely design (cf. the ongoing study of Wisconsin youth), or the data will be retrospective. Panel data may not provide complete information on mobility or status changes, often only the occupational position at discrete points in time will be observed—points that for reasons of economy may be quite widely spaced. Retrospective life-history data in contrast may enable the observation of every act of mobility undertaken by respondents over the period of observation. The main drawback of these data is possible errors of recall. Retrospective life-histories of a cohort of 30-39 year old men are the data source for the empirical analysis to be presented in this paper.[2]

The major problems with intragenerational data are, however, problems of analysis. Since these data are change data, well-established methods of analysis are less available. Although the age variation in status is recognized in status attainment research, direct analysis of change in status typically is not attempted. Rather, the analysis focuses on the level of status attained at particular ages, a procedure that cannot fully capture the dynamic nature of the process being investigated (Sørensen, 1974a). The use of causal analysis of mobility patterns based on intragenerational data presents the problem of how to summarize the wealth of information on mobility in a suitable mathematical form. With the exception of White's (1970) vacancy chain model, little work has gone beyond the attempts to fit observed mobility patterns to a stochastic model. The typical choice of model has been the Markov model. However, this

[1] Boudon (1974) thus returns to the analysis of mobility to establish the importance of education for inequality of opportunity, because he wants to incorporate social structural variables in the analysis. Such analysis was rejected by Blau and Duncan (1967), because of the serious methodological problems present in causal analysis using mobility as a dependent variable. However, the strategy of using status obtained as the dependent variable, adopted in status attainment research, does not give answers to the kind of questions Boudon poses.

[2] The Life History Study dealt with the occupational, educational, familial and residential experiences from age 14 to time of interview. The universe is the total population of males 30-39 years of age, in 1968, residing in households in the United States. Two samples were drawn: (a) a national sample and (b) a supplementary sample of blacks. The total number of interviews obtained was 1589: 738 blacks and 851 whites. The completion rates were 76.1 percent for sample (a) and 78.2 percent for sample (b). The Life History Study was initiated by James S. Coleman and Peter H. Rossi, then of the Department of Social Relations, The Johns Hopkins University.

model in its simplest form never has been found to fit observed mobility patterns very well. A number of modifications have been made, in most instances using *ad hoc* procedures to improve the fit of the Markov model. These modifications have most often been statistical in nature and have not been derived from an explicit theory of the forces that produce mobility.

It is the purpose of this paper to analyze intragenerational mobility rates using a Markovian model of mobility as the framework. An attempt will be made to modify the simple Markov model in a way that both improves the fit of the model and derives from a reasonable set of assumptions about the process of mobility. These assumptions will be derived from considerations of the occupational achievement process, that is, the process of attaining status and income.

It will be shown that these considerations lead to a model that mirrors the interplay between structural and individual characteristics that traditionally has been held to generate mobility. Comparison of mobility rates for blacks and whites will be used to demonstrate how this interplay works.

THE MARKOV MODEL OF MOBILITY

In this section, I shall outline a stochastic model of mobility to be further specified in subsequent sections. The final product will technically be a nonstationary discrete state process in continuous time. Mobility is conceived of as taking place in discrete steps: individuals spend some time in a job and then move to another job. That moves take place in discrete steps does not imply a discrete time Markov chain model. Rather, moves can take place at any point in time, so the process is conceived of as a continuous time process. Furthermore, in the final model, the rate of mobility is assumed dependent on time, so the process is nonstationary. However, as in a Markov chain, individuals move in a discrete state space formed by job categories (in this application, occupational groups).

The elementary act of mobility is conceived of as a job shift. The analysis of such shifts can be broken down into two components: (1) the analysis of the occurrence of shifts and (2) the analysis of the outcome of shifts given that they occur. This distinction is basic for the remainder of this paper. A model for mobility may be derived by making certain assumptions about the two components of the process. A simple model, useful as a point of departure, is obtained by making two sets of assumptions.[3]

First, job shifts are assumed to occur according to a Poisson process, that is, they occur randomly in time with an intensity λ, where this parameter is assumed constant over time and across individuals. This means that the probability p(s) that a person who entered a job at time 0, will not have moved by time s,[4] will change according to the differential equation:

$$\frac{dp(s)}{ds} = -\lambda p(s). \qquad (1)$$

Equation (1) is the defining equation for a Poisson process.[5] It implies that the number of job shifts in a period T will be Poisson distributed, and the waiting times between shifts will be exponentially distributed, a property to be used later.

The second assumption is that once a move occurs, its outcome will be given by a set of conditional probabilities m_{ij}'s that give the probabilities of moving from a job in some occupational category, or state i, to a job in state j, where i may equal j. These "direction" probabilities are assumed conditional on the state of departure only, not on any previous state the individual has occupied—an assumption often referred to as the Markov Property.

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[3] The imagery conveyed by the representation of the Markov model given below seems especially useful in applications to mobility. I am indebted to Seymour Spilerman for introducing me to this formulation. For an excellent survey of various formulations of this model and a number of estimation problems, see Singer and Spilerman (1974).

[4] A number of different symbols for time will be used in this paper. The quantity s is used to denote an arbitrary time interval measured from when a person enters a job. The quantity t is used to denote time spent in the labor force, measured from entry into the labor force. The quantity d is used to denote observed durations in a job. The time of entry into a job will be denoted t_1 (for which s = 0), and the time of exit t_2, so $d = t_2 - t_1$. In the redefined time scale, to be introduced, v(t) corresponds to t, and s^*, d^*, t_1^*, and t_2^* correspond to s, d, t_1 and t_2. All quantities are assumed to be nonnegative.

[5] The solution to (1), to be used later, is $p(s) = e^{-\lambda s}$ with $p(0) = 1$.

Again, these probabilities shall be assumed identical for all individuals and constant in time. These two assumptions and the parallel ones for the rate of job shifts shall be referred to in the sequel as the assumption of homogeneity among individuals and the assumption of the processes being stationary in time, respectively.

With these assumptions, consider the probability $p_{ij}(s)$ that a person will be in state j at time s given that he was in state i at time 0, where 0 can be taken as an arbitrary time point because of the stationarity assumption. The rate of change in this probability will be a function of the rate at which job shifts occur, λ, and the direction probabilities, the m_{ij}'s. This change can be described by a differential equation:

$$\frac{dp_{ij}(s)}{ds} = -\lambda(1 - m_{jj}) \cdot p_{ij}(s) + \sum_k \lambda m_{kj} p_{ik}(s). \quad (2)$$

If the $p_{ij}(s)$ quantities are arranged in a matrix P(s), and the quantities m_{ij} in a matrix M, then this differential equation can be written as:

$$\frac{dP(s)}{ds} = \lambda(M - I)P(s) \quad (3)$$

where I is the identity matrix. Equation (3) has the solution:

$$P(s) = e^{\lambda(M - I)s} \quad (4)$$

keeping the assumption P(0) = I.[6]

Since the rate of moves is assumed to be governed by a Poisson process, it follows that waiting times between moves are exponentially distributed; that is, durations of jobs, d, have the density:

$$f(d) = \lambda e^{-\lambda d} \quad (5)$$

with mean

$$E(d) = \int_0^\infty \lambda u e^{-\lambda u} du = \frac{1}{\lambda} . \quad (6)$$

This expression for the mean suggests estimating λ as the inverse of the mean duration of jobs.

Even though (4) is a useful representation of a Markov model of mobility, it is no more valid than other formulations found to be a poor description of mobility data,[7] for these formulations can be derived from (4) without any change in assumptions.

The lack of fit calls for either some modification of (4), if the basic framework is to be kept, or for rejection of the Markovian framework altogether. The latter strategy has been advocated for conceptual reasons by White (1970) and has also been adopted by Boudon (1974). Both are concerned about implementing a basic axiom of mobility theory—that mobility is at least partly determined by social structure. White (1970) argues that this implies that the analysis of mobility of men as a stochastic process is unreasonable, for the movement of one man will depend on the movement of others. This violates a fundamental assumption needed to formulate a model for the process: the assumption of stochastic independence of the elements.

White (1970) analyzes mobility in organizations where there are few vacancies and many men, and where the assumption about independence of the movement of men obviously is false. The life-history data used here deal with the movement of only one cohort in a much larger occupational structure, so that it does seem justified to analyze them as though they represent independent

[6] This formulation is sometimes referred to as a Generalized Poisson process, or a continuous time Markov chain (see Singer and Spilerman, 1974). A more general formulation assumes that λ depends on the state, so that the scalar λ is replaced by a diagonal matrix Λ with elements $\lambda_1, \lambda_2, \ldots \lambda_i, \ldots \lambda_n$. Further generalization is obtained by letting the m_{ij}'s depend on time. Introduction of state specific λ_i's would be a reasonable modification of (4) for use on mobility data. However, if differences among occupational groups in the λ_i's are caused by a variation in the desirability of occupations, the formulation of the dependency of λ on time to be introduced should remove this variation.

[7] The best known test of the Markov model on intragenerational mobility data is probably the one given by Blumen et al., 1955. They tested a Markov chain, but the failure of this model also implies that the representation (4) will provide a poor fit to the data.

moves of individuals, and thus keep the framework of a stochastic process for mobility of men.

This paper then shall keep the basic framework suggested by (4) and attempt to modify it in order to obtain a more realistic model. From a statistical point of view the failure of the Markov model, if moves are assumed stochastically independent, may be attributed to failure of one or both of the basic assumptions of stationarity and homogeneity of the parameters. The separation of the process into two components implies that the model may be modified with respect to both the assumptions concerning the rate of mobility and the assumptions regarding the direction probabilities. Only the first task shall be attempted here, that is, the assumption that job shifts occur according to a stationary Poisson process shall be modified. The result will be the formulation of a nonstationary Poisson process. Analysis of the direction probabilities is in progress.[8]

THE RATE OF JOB SHIFTS AS A TIME DEPENDENT POISSON PROCESS

The simple Markov model (4) was derived from the assumption that job shifts occur according to a Poisson process with a parameter λ, constant over time and identical for all individuals. This assumption is both simple and convenient as it leads to an estimation of λ as the inverse of the mean duration of jobs. However, the assumption does not represent a reasonable approximation of reality nor can it be derived from commonly held beliefs about what generates mobility.

The assumption does not represent reality for it is well known that older persons are less likely to shift jobs than younger persons. On the life-history data, it was found that the correlation between the duration of job and a person's labor force experience is .43 for whites and .44 for blacks. This suggests that λ should be a function of time in labor force $\lambda(t)$. Furthermore, the rate of job shifts may differ among individuals.

The observation that rates of job shift should vary with time and among individuals could be incorporated directly into the model on an *ad hoc* basis. Models where $\lambda(t)$ is specified as a declining function of time are discussed by McGinnis (1968), Ginsberg (1971) and Mayer (1972). Spilerman (1972) has shown how an assumption of λ being gamma distributed among individuals can be incorporated into the model (4) and a reasonable empirical fit obtained. Such devices then can be used to improve the fit of a model to observed data. However, when these devices are justified mainly by statistical concerns, they are of rather small help in understanding the forces that generate mobility.

A theoretical rationale for specifying $\lambda(t)$ and/or its variation among individuals cannot be obtained directly from a body of well-specified theory of mobility, for such a body of theory does not exist. The traditional conceptualization of mobility states that mobility is the outcome of an interplay between structural and individual characteristics. This is a very vague and general statement that must be made considerably more specific in order to be of any help in modeling the variation in rates of job shifts.

The point of departure for this specification is the observation that job shifts can represent either a voluntary act on the part of the job holder or represent an involuntary dismissal from a job. In both cases, structural characteristics interact with individual ones in producing the shift. If the shift is voluntary, there must be some vacant job available that the person can move to, and the individual must have the qualifications needed to obtain the available job. If the shift is involuntary, it often represents the elimination of a job, and it is likely that employers will first dismiss those employees who are most expendable to the firm. The two types of job shifts cannot be described by the same process. In the sequel the analysis will concentrate on voluntary shifts for which an explicit formulation of their dependence on time can be given. However, involuntary shifts are of importance in the empirical analysis to follow.

A person may be assumed to shift jobs voluntarily if he can obtain a better job. This shift may be seen as the outcome of a process where job opportunities become known to the individual and create impulses to leave a job.

[8] The basic idea is to conceive the m_{ij}'s as determined by the availability of jobs, denoted c_j in an occupational group, and the distance or affinity between occupational groups, denoted d_{ij} so that $m_{ij} = c_j \cdot d_{ij}$. The parameters c_j and d_{ij} can be estimated using the techniques developed by Goodman (1970).

Denote the rate at which impulses to leave arrive in time by z(t), where t is measured from time of entry into the labor force. Assume further that a person has a constant probability λ^* of acting on any one of these impulses. Assume that a person enters a job at time t_1 and consider the probability he will not have left the job after an interval of length s by time $t = s + t_1$. This probability can be written, in analogy to the simple Poisson process (1), as:

$$\frac{dp(t)}{dt} = -\lambda^* z(t)p(t) \tag{7}$$

or, slightly rearranging:

$$\frac{dp(t)}{p(t)} = -\lambda^* z(t)dt. \tag{8}$$

Define a quantity v(t) by the relation dv(t) = z(t)dt, so that:

$$v(t) = \int_0^t z(u)du. \tag{9}$$

It is useful to conceive of v(t) as a new time scale[9] defined by the rate at which impulses to leave arrive, which in turn reflects the rate at which opportunities for better jobs appear. In this new time scale, the process will be Poisson, as can be seen by substituting dv(t) for z(t)dt in (8). The intensity λ^* of leaving jobs therefore may be estimated as the inverse of the duration of jobs in v(t). Formally, the duration of a job in the new time scale will be:

$$d^* = t_2^* - t_1^* \tag{10}$$

where t_2^* and t_1^* denote values of v(t) when he leaves and enters the job, at real times t_2 and t_1, respectively.

The rate of impulses to leave a job z(t) is considered a function of time because the number of jobs in any occupational structure is finite, so that there will be an upper limit to how good a job a person can obtain given his qualifications. This means that the better job a person already has the less likely it is that an even better job will become available.

If z(t) can be specified in a way that can be empirically tested, the formulation of the Poisson process in v(t) is extremely convenient. The Markov model (4) will then be a stationary process in v(t), that is,

$$p(s^*) = e^{\lambda^*(M - I)s^*} \tag{11}$$

where s^* is an interval of time in v(t) and λ^* is independent of time v(t). Since nonstationarity in λ will imply heterogeneity in λ's observed for a group of individuals observed at a point in time,[10] the redefinition of time may remove some of this heterogeneity.

The notion that z(t) depends on the rate at which job opportunities appear suggests a specification of z(t) based on a model of the occupational achievement process.

RATES OF JOB-SHIFTS AND THE OCCUPATIONAL ACHIEVEMENT PROCESS

Although persons may consider various aspects of jobs desirable, the status and income provided by a job are usually especially important. Let y(t) represent the occupational achievement of a person at time t, as measured by his status and income. Occupational achievement changes over time as a person obtains better jobs. A simple model[11] for this process is:

$$\frac{dy(t)}{dt} = c_0 + by(t) + c_1 x_1 \tag{12}$$

where x_1 is a measure of a person's occupational resources (family background, education, etc.), and c_0 stands for the effect of unmeasured resource variables. Other things being equal, the higher a person's occupational resources the greater the rate of change in occupational achievement. However, the rate of change is also determined by

[9] A similar formulation, using age as the time scale, is developed in an earlier paper (Sørensen, 1975a).

[10] A discussion of the relation between heterogeneity and nonstationarity is presented by McFarland (1970). His argument easily generalizes to the formulation brought here.

[11] This model is the differential equation analog to the difference equation model of occupational careers developed earlier (Sørensen, 1974b). A general treatment of linear models for change may be found in Coleman (1968).

y(t)—the level of achievement already obtained. This influence is assumed to be negative ($b < 0$) so that the higher the achievement the less gain will take place. Eventually the rate of change will be zero and at that time the level of achievement will be:

$$y(e) = -\frac{c_0}{b} - \frac{c_1}{b} x_1. \tag{13}$$

The quantity y(e) shall be referred to as the equilibrium level of occupational achievement.[12] It is the highest level of achievement a person can hope to obtain and keep, for if his achievement exceeds this level the change in achievement will be negative.

The quantity b determines how much growth is possible in occupational achievement, and therefore is a measure of the opportunity structure of society. The closer b is to zero the more opportunities for better jobs are available to a person. In the extreme case in which $b = 0$, better jobs are always available regardless of how much status and income a person has already obtained, and the rate of growth will remain constant forever (or until retirement).

The career curve determined by (12) will be concave to the time axis; that is, achievement increases rapidly in the beginning and then gradually tapers off until the status and income reach the level given by (13). This is indeed the pattern of growth observed, as can be seen in Figure 1, which gives the mean occupational prestige for blacks and whites by years of labor force experience.[13] It can be noted that the curve for blacks not only overall is at a lower level than for whites, it is also flatter. This may be due to the lower levels of occupational resources of blacks, but could also reflect a difference in b due to different opportunity structures for blacks and whites.

[12] In status attainment research, where the level of status is the dependent variable, there is an implicit (but false) assumption that y(e) is achieved for all respondents, as (13) corresponds to the linear models used in this research. Some consequences of making this assumption for casual inferences in status attainment research are discussed in Sørensen (1974a).

[13] The mean status by year in labor force displayed in Figure 1 also includes the changes produced by involuntary job shifts, while in the sequel only voluntary changes are considered. Voluntary changes are however dominating.

The curves represent the solutions to the differential equation (12). Integrating (12) from time 0 to time t, where time 0 represents a person's entry into the labor force, gives:

$$y(t) = \frac{c_0}{b}(e^{bt} - 1) + e^{bt}y(0) + \frac{c_1}{b}(e^{bt} - 1)x_1 \tag{14}$$

where y(0) represents the occupational achievement at entry into the labor force.

The model of the process of occupational achievement given by (12) shall now be linked to the rate of voluntary job shifts. Gains in achievement may be assumed to entail job shifts. This is trivially true for occupational prestige, because prestige is defined as an attribute of occupational groups. It is a reasonable approximation for income, as major shifts in earnings usually entail job shift, while within-job gains represent real and inflationary increases common to all jobs. If gains in achievement then necessitate job shifts, it follows that the rate of shifts can be derived from (12). The pattern of rapid change in the beginning of the career and less change later on must entail high rates of shifts in the beginning of the career and low rates later on. It is both instructive and necessary to derive the explicit formulation for the relationship between the rate of job shifts and the achievement process.

A slight reformulation of (14) is useful. Let:

$$v(t) = \frac{1}{b}(e^{bt} - 1). \tag{15}$$

Equation (14) can now be written as:

$$y(t) = e^{bt}y(0) + v(t)(c_0 + c_1x_1). \tag{16}$$

Subtracting y(0) from both sides of (16) and rearranging gives:

$$y(t) - y(0) = y(0)(e^{bt} - 1) + v(t)(c_0 + c_1x_1). \tag{17}$$

Equation (17) may be written as:

$$y(t) = y(0) + v(t)[c_0 + c_1x_1 + by(0)]. \tag{18}$$

Consider v(t) a redefinition of time. In this time scale the career curve will be a straight

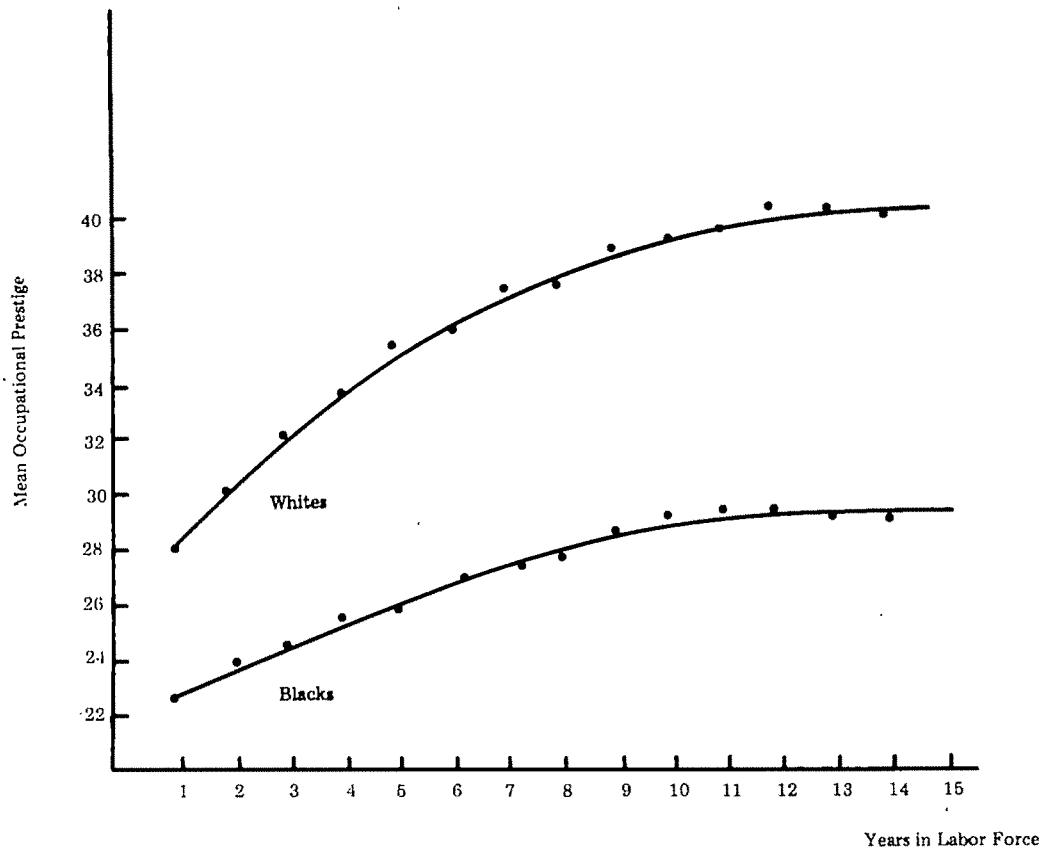

Figure 1. Mean Occupational Prestige by Year in Labor Force

line with a slope equal to $[c_0 + c_1x_1 + by(0)]$ and an intercept equal to $y(0)$. The slope will be zero when

$$y(0) = -\frac{c_0}{b} - \frac{c_1}{b}x_1; \qquad (19)$$

that is, when the first job provides a person with the equilibrium level of achievement. In general, the slope will be determined by a person's level of resources in relation to the achievement at entry into the labor force: the lower this achievement in relation to his occupational resources, the greater the magnitude of the slope.

The fact that the career is linear in $v(t)$ implies that there will be a constant gain per unit time in $v(t)$. Since gains are assumed obtained through job shifts, it follows that the rate at which shifts occur must be constant in $v(t)$. Using the notion of time suggested before, the underlying mechanism may be seen as one where job opportunities (that generate impulses to leave jobs) occur at a constant rate in $v(t)$.

The redefinition of time $v(t)$ is then the desired transformation of time so that the rate of job shifts will follow a Poisson process. The function $z(t)$ giving the rate of impulses to leave in real time can be obtained from $v(t)$ by differentiating equation (15):

$$z(t) = \frac{dv(t)}{dt} = e^{bt}. \qquad (20)$$

Further, at the individual level, the decrease in the rate of impulses to leave is proportional to $z(t)$:

$$\frac{dz(t)}{dt} = bz(t) \qquad b < 0. \qquad (21)$$

This completes the derivation of the desired redefinition of time that expresses the dependency of the rate of job shifts on time. However, the model for the occupational achievement process can also give some insights into what determines the overall rate of job shifts. Assume that a person only will undertake a job shift if a gain of a certain size in occupational achievement can be realized. The slope of the career line in v(t) will then reflect the magnitude of the rate of job shifts in v(t). This slope was shown above (equation 18) to be determined by a person's occupational resources relative to the achievement of the first job. Variation in occupational resources relative to the achievement of the first job is then a source of heterogeneity in rates of shifts (λ^*) in time scale v(t).

A theory of the occupational achievement process has been formulated that provides a specification of the dependency on time of the rate or intensity of job shifts and that also provides a framework for interpreting the size of the rate of job shift. The theory follows the traditional notion of mobility being an interplay between structural and individual characteristics, by conceiving job shifts as brought about by individuals utilizing opportunities for better jobs to maximize their occupational achievement. Because opportunities are finite, the higher the achievement already obtained, the fewer are the opportunities for better jobs and the lower the rate of shift.

The derivation of the dependency on time of $\lambda(t)$ can be used to redefine time in such a way that the Markov model is stationary in the new time scale. While the observed career curve does show the pattern to be expected from the proposed model of the achievement process, this is too impressionistic evidence. A direct verification of the expression for the dependency of rates of shifts on time is needed, and procedures needed to estimate the rate of job shifts in the redefined time scale, v(t).

TESTS AND APPLICATIONS

The model for the dependency of the rate of job shift on time formulated above relied on the redefinition of time into the new time scale v(t) in which job shifts are assumed to occur according to a Poisson process. The rate of shift λ^* in v(t) can be estimated as the inverse of the mean durations measured in v(t). Inserting the expression (15) for v(t) in the definition of d^*, the duration in v(t) as given in (10) yields:

$$\begin{aligned} d^* &= t_2^* - t_1^* \\ &= \frac{1}{b}(e^{bt_2} - 1) - \frac{1}{b}(e^{bt_1} - 1) \\ &= \frac{1}{b} e^{bt_2}(e^{bd} - 1), \end{aligned} \tag{22}$$

where t_2 and t_1 denote the time of leaving and entering the job, respectively, and $d = t_2 - t_1$ is the duration of the job in real time. The model can be tested and estimates of b obtained in several ways.

One procedure is to study whether the observed mean durations measured in v(t) can indeed be made time independent using an iterative procedure to obtain estimates of b. This is a cumbersome procedure especially since the interruption of the process at the time of interview causes estimation problems (see below). An alternative procedure is to rely directly on some property of the model both to test it and to obtain estimates of b—the parameter that determines the redefinition of time.

The latter procedure shall be attempted first. Assume that the process indeed is Poisson in v(t). The solution to the defining differential equation (1) in time scale v(t) is:

$$p(s^*) = e^{-\lambda^* s^*} \tag{23}$$

or

$$\log p(s^*) = -\lambda^* s^*, \tag{24}$$

where $p(s^*)$ is the proportion remaining in a job at time v(s) of those that have entered at time v(0).

Denote again by t_1 the time (in real time) at which a person enters a job and by $s = t_s - t_1$ a time interval of given length s. Since $s^* = t_s^* - t_1^*$, inserting for s and t_1 in (24) gives:

$$\log p(s^*) = -\lambda^* \frac{1}{b} e^{bt_1}(e^{bs} - 1)]. \tag{25}$$

Multiplying through by −1 and taking logarithms again gives:

$$\log[-\log p(s^*)] = \log \lambda^* - \log b + bt_1 + \log(e^{bs} - 1). \quad (26)$$

If a set of t_1 values is chosen and the proportion remaining after s time units is observed, a linear relation between $\log[-\log p(s^*)]$ and t_1 is indicated. For given s, all terms on the right hand side are constant except bt_1 or,

$$\log[-\log p(s^*)] = k + bt_1. \quad (27)$$

This expression may be used both to test the model and obtain an estimate of b.

The life-history data provide information on all jobs held by a sample of 30-39 year old black and white men from the time of entry into the labor force until the time of interview. A special file was created with jobs as the unit of observation; that is, the total number of observations equals all jobs held by all respondents. For blacks this gives 3678 jobs, for whites 4992. The starting and ending times of jobs are registered in months. For purposes of this analysis, all time points are measured from time of entry into the labor force and only jobs left voluntarily are used. The separation of shifts into involuntary and voluntary shifts was based on respondents' reports on whether a job was left voluntarily or not. The number of jobs left voluntarily was 2651 for blacks and 3877 for whites.

If the opportunity structure differs for blacks and whites, as is generally believed, the value of b may differ for blacks and whites. For this reason, the test and the following analysis is carried out separately for blacks and whites.

The test of the model suggested by (27) is displayed in Figure 2. Unfortunately, much of the information contained in the life-history data was not usable for the test. When a group of persons that enter a new job, say four years after entering the labor force, are followed over time, it will be the case that after a period it will no longer be possible to classify some of the respondents with respect to whether they remained in their jobs or not, simply because they are interviewed and no further information is obtained. While all respondents were between 30 and 39 when they were interviewed, age at entry into the labor force varied greatly. Consequently the proportion $p(s^*)$ is quite strongly influenced by respondents being interviewed and therefore lost for the analysis—the more so the larger the value of s. To minimize the influence of this problem and still obtain a reasonable range of values of s and t_1 only persons who entered the labor force before age 23 are used in Figure 2.

Overall, the expected linear relationship between $\log[-\log p(s^*)]$ and t_1 (given in years of labor force experience) comes through reasonably well in the graphs. Also the slope of the line relating the two quantities appears very similar in all graphs, except the graph for blacks and s = 1. Furthermore, it can be observed that for blacks the points corresponding to $t_1 = 1$ systematically deviate from the straight line relationship in all graphs. Both phenomena may be ascribed to lower reliability of the information pertaining to these early years in the labor force. Unfortunately, the deviations, although explainable, invite caution in using estimates of b obtained from equation (27) to calculate d^*.

The graphs given in Figure 2 suggest that the proposed model is a reasonable approximation to reality. Estimates of b can be obtained by pooling least squares estimates of b for the various values of s. For whites, the resulting b is -.0065, with an R^2 of .79, where b is given with months as units. For blacks, inclusion of the observations for s = 1 and $t_1 = 1$ gives a bad fit of the linear equation: $R^2 = .21$. Deleting these observations gives a much stronger fit. The obtained b is -.0086 with an R^2 of .94. This indicates that b is indeed higher in absolute value for blacks than for whites, reflecting a more unfavorable opportunity structure for blacks. However, since these estimates are based on only part of the sample and since by necessity they will use the more unreliable information from the early job history, they are not necessarily the best estimates. It is necessary to complement the test by an analysis that directly shows whether mean durations are time independent after the transformation of time.

Mean durations in v(t) can be calculated from equation (22). However most respondents held a job at the time of interview. The duration of this job in real time is not known. One might suggest deleting these jobs from the sample, but it turns out that this will produce a biased estimate of λ^* based on the remaining jobs. The problem has to do with

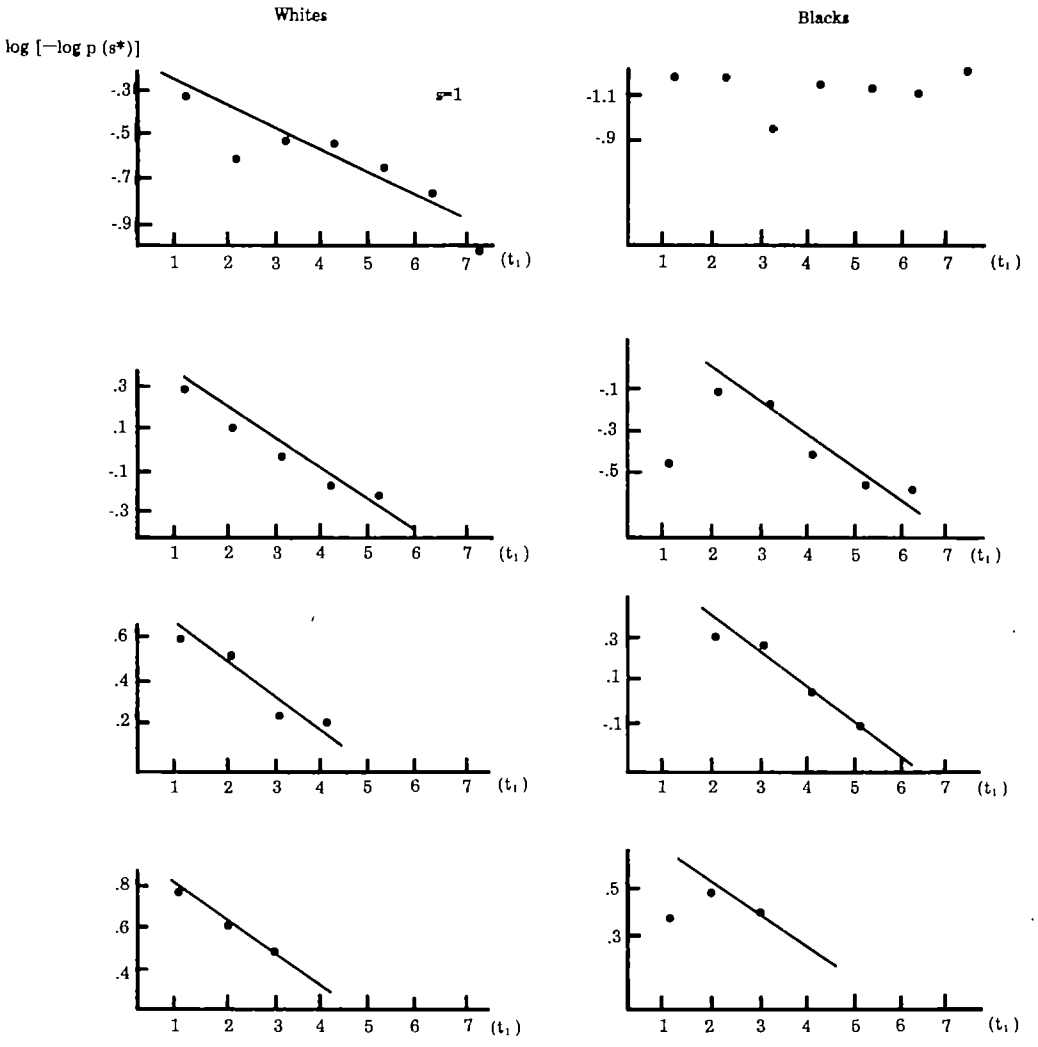

Figure 2

Plot of log [-log p (s*)] Against Time of Entering Job

the distribution of the observed portion of the duration of the job held at interview. The nature of the problem shall be briefly indicated. A more detailed treatment is given in Sørensen (1975b).

One might argue that for reasons of symmetry, the observed portion of the duration of the job held at interview should be half the expected duration. If job durations are exponentially distributed (as they are when job shifts follow a Poisson process), they will have the mean $1/\lambda^*$. As a consequence, it might be argued that the observed portion of the interrupted durations should have an expected value of $1/2\ (1/\lambda^*)$. But paradoxically, the expected value of the duration until interview for the interrupted jobs is $1/\lambda^*$, because the distribution of the duration of the jobs interrupted by the interview is not exponential, even if the assumptions for the stationary Poisson process are upheld. Rather, the expected duration of the completed interrupted jobs is $2/\lambda^*$, and the symmetry argument then gives an expectation of the observed portion of the job duration of $1/\lambda^*$. This paradox is discussed by Feller (1971) who also derives the distribution of the waiting times in a Poisson process that is interrupted.

Intuitively, the reasons for the paradox are

that the interruption of the process (i.e., the interview) tends to select longer durations among the set of randomly distributed durations; that is, a long job duration has a greater likelihood of covering the interview than a short one. Since the expected duration is $2/\lambda^*$, those jobs held at the time of interview may apparently be treated just as all other job durations. But, if the interruption selects longer jobs among those held by an individual from the time of entering the labor force until the time of interview, the noninterrupted job durations will tend to be shorter.[14] Since the time of interview in this sample is age dependent (between 30 and 39), it is necessary to adjust for this effect of the interruption, otherwise the observed mean duration of jobs held at a certain time point will depend on the proportion of these jobs that are interrupted. The proper adjustment is (see Sørensen, 1975b for detail):

$$d_i^* = \frac{d_{ni}^* \cdot T^*}{T^* - d_m^*} \tag{28}$$

where d_{ni}^* is the observed duration in v(t) of a noninterrupted job, T^* is the time from entry into the labor force until interview (also measured in v[t]), and d_m^* is the mean of the observed portion of the duration of jobs held at the time of interview.

Using the estimates of b given above and the adjustment procedure just described, durations were computed in v(t). The mean duration for jobs was then obtained and their dependency on the time of leaving the job (measured in v[t]) studied.[15] It was apparent that the estimate for b used did not completely remove the time dependency, especially not for whites. This apparent failure of the model could simply be due to poor estimates of b. Using essentially a trial and error method, a number of different values of b in the neighborhood of the first estimates were used. This procedure, quite cumbersome because of the truncation problem, gave d^*'s that behaved fairly reasonably for $|b| = .008$ for both blacks and whites. The correlation between d^* and time of leaving job is .03 for blacks and .08 for whites, as opposed to .44 and .43 before the transformation of time. The mean values of d^* by time of leaving the job, denoted t_2^*, are presented in Table 1. The first value of t_2^* shown is 30 (months of v[t]), as truly time independent durations cannot be obtained for the smallest values of t_2^*, not because of a failure of the model, but because of the necessary dependency between d^* and t_2^* for small values of t_2^*.

Although the mean d^*'s do appear to be fairly stable over time, there is a slight curvilinearity indicated for both blacks and whites, which means that b is not completely constant over time, contrary to our assumption. The absolute values of the true b apparently are underestimated by a $|b| = .008$ in the early years of labor force participation and somewhat overestimated in later years. This result can most reasonably be interpreted as reflecting changes in the overall opportunity structure of society in the period covered by the life-histories used here, which roughly covers the two decades from the late 1940s to the late 1960s. The general expansion of the economy, especially in the 1960s, probably

Table 1. Mean d^* as a Function of t_2^* Voluntary Shifts Using $b = -.008$

t_2^*	Mean d^* Whites	Mean d^* Blacks
30	14.3	18.2
40	14.3	18.4
50	13.0	18.6
60	14.2	20.1
70	14.4	19.4
80	16.7	17.3
90	15.3	18.4
100	15.0	16.5
110	12.4	17.0
Overall	14.0	17.5

[14] I am indebted to James S. Coleman for this observation.

[15] Because of the interruption, there is a necessary dependency between d^* and t_1^* measured at the start of a job, also after adjustment of d^*. A simulation established that the use of t_2^* at the end of the job was the most reasonable for demonstrating independence of time and d^*.

provided for more opportunities for better jobs toward the end of the period. This would be reflected in a decline in the absolute value of b as argued earlier: the more opportunities for better jobs, the closer b should be to zero.

A direct test of this interpretation of the slight curvilinearity in d^* can be given by analyzing the jobs held in 1968 separately. Using the iterative procedure, the best fitting values for b were b = –.005 for whites and b = –.007 for blacks. Blacks seem somewhat less sensitive to the presumed changes in opportunity structure than whites. The durations in v(t) using these values of b for jobs held at the time of interview are presented in Table 2.

Since d^* for these jobs are independent of t_2^* using b's lower in absolute value than the best fitting b's for all jobs, the interpretation given to Table 1 seems correct. This means that either the model for the dependency of rates of job shifts on time should be revised to take into account overall changes in opportunity structures during the period, or alternatively, separate estimates should be obtained for various historical epochs. These modifications are not attempted here; the development of more refined methods of estimation shall be attempted in future research.

Table 2. Duration in v(t) of Jobs Held at Time of Interview by Total Labor Force Experience (T*)

Whites (b = -.005)		Blacks (b = -.007)	
T*	d_m^*	T*	d_m^*
100	25.2	80	21.4
110	25.7	90	17.8
120	24.1	100	20.0
130	26.2	110	20.6
140	23.2	120	21.8
150	26.0	130	21.6

Note: The scales for T* differ for blacks and whites because of different values of b used.

The overall mean d^* using b = –.008 is 14.0 for whites and 17.5 for blacks, indicating that the rate of voluntary job shifts is lower for blacks than for whites. If durations in v(t) are exponentially distributed, their standard deviations should be $1/\lambda^*$. The overall estimates of the standard deviation for blacks is 18.7 and for whites 16.2. However, the interrupted jobs are known not to be exponentially distributed (Feller, 1971:12-3). The standard deviation for the noninterrupted jobs for blacks is 17.9. This is close to the value of the mean (17.3) indicating that the durations conform reasonably well to the exponential distribution for blacks. By this test, job shifts for blacks seem to be adequately described by a Poisson process. The results are somewhat less satisfactory for whites, for whom the standard deviation is 15.4, higher than the mean of 13.3 for the noninterrupted jobs. This indicates some heterogeneity in λ^* for whites.

BLACK-WHITE COMPARISONS

The preceding section would mainly be a quite elaborate statistical procedure for obtaining a stationary Markov model of mobility if it were not possible to interpret the results. To demonstrate that the framework developed does lend itself to substantive analysis, rates of job shifts for blacks and whites are compared in this section.

The sample consisted of 851 whites and 738 blacks. The black respondents held a total of 3678 jobs, while whites held 4992 jobs. This gives a mean number of jobs equal to 4.98 for blacks and 5.87 for whites. Blacks have fewer jobs and thus appear to hold their jobs longer (in real time) than whites. This difference could reflect a difference either in the opportunity structure for blacks and whites or in the likelihood of changing jobs. The latter would reflect that the occupational achievements of the early jobs blacks obtain are closer to the maximum blacks can hope to obtain.

A difference in opportunity structure between blacks and whites would show up in the duration of jobs because the higher the absolute value of b the fewer impulses to leave the job a person will receive. Hence—even if λ^* is identical for the two groups—the

durations will be longer in real time. However, the trial and error method used to establish b indicated that a b of −.008 gave the best estimates for both groups. Presumably, the durations of jobs for blacks are longer in real time because blacks early in their careers obtain an occupational achievement closer to the best they can expect than do whites. Technically the difference in average duration in real time could also be explained by a difference in age of entry into the labor force: if blacks enter earlier they have a greater chance to hold long jobs. However, the difference in mean age of entry is only a little more than half a year; and more important, our calculation of the mean duration in v(t), (using b = −.008), indicated that blacks do indeed have a lower rate of shifts ($\lambda^* = .057$) than do whites ($\lambda^* = .071$).

This conclusion should however be modified in several ways. The calculation of b used only voluntary shifts. If the average black is exposed to a more unfavorable opportunity structure than are whites, this should produce more involuntary shifts for blacks than for whites. This is indeed the case: of all jobs held, blacks left 28.7 percent involuntarily, while whites left 20.7 percent of all jobs in this way. Focusing only on voluntary shifts captures a more select group of job shifts for blacks than whites.

Including involuntary shifts in the estimation of b for blacks and whites would lead to worse, not better estimates of b, because involuntary shifts obviously occur when a person would not otherwise undertake the shift. Including the involuntary shifts with the voluntary shifts when estimating b should therefore lead to underestimation of b and thus imply a more favorable opportunity structure than actually exists.

The somewhat perverse influence of involuntary shifts on the estimation of b can be used to support an argument that our trial and error method underestimates the absolute value of b, especially for blacks. Even though only voluntary shifts are used in the estimation, the voluntary shifts of blacks may be "less voluntary" than those of whites. Whether a job shift was voluntary or not is a self-report and obviously fallible. If it is considered as an indicator of an underlying variable that expresses the degree of control over the employment situation, the higher observed percentage of involuntary shifts for blacks should indicate an overall lower level of control. Hence the level of control could also be lower for reported voluntary shifts and the absolute value of b correspondingly underestimated. For this reason, a conclusive statement regarding the difference, or lack of it, between b's estimated for blacks and whites should await more refined methods of estimation than the ones used here.

It is interesting to note in this connection that the mean duration of jobs left involuntarily, measured in v(t), is 12.8 for blacks and 8.9 for whites. On the average, blacks are fired when they have held jobs longer than whites. No one should stay in a job if a better one becomes available before he is fired, and this result therefore again reflects that blacks may have less to gain because of fewer opportunities for shifts.

Involuntary job shifts are important for the occupational achievement process because they are likely to produce a loss in status and/or income. Any gain a person may obtain should be obtained through a voluntary shift, before the firing takes place. The combined impact of less frequent voluntary shifts (and thus fewer gains in achievement) and a greater likelihood of loss, not only makes the career line of blacks flatter but also more likely to undergo a decline. This clearly illustrates that for blacks the lower observed levels of occupational achievement not only are a question of lower levels of occupational resources, but also of more tenuous employment situations.

The apparent decline in b with the expansion of the economy is a result that, even though it points to a limitation of the model, is of interest in its own right. It demonstrates that structural changes in society indeed are reflected in the mobility of men, and that the impact can be given a precise expression in the model for the rate of mobility. Furthermore, it appears that blacks have profited somewhat less from this expansion in terms of opportunities for better jobs than have whites. Another apparent failure of the model also points to an interesting substantive phenomenon. While for blacks it is the case that the standard deviation in durations in v(t) is very close to the mean, a difference was observed for whites indicating that there is heterogeneity in λ^* for whites. The rate of shifts in v(t) has been argued earlier to reflect the slope of the

career line in v(t). The result then indicates that blacks have very similar careers insofar as they can undertake voluntary job shifts, while there is some variation in slopes for whites. A greater homogeneity in starting points of the career as well as in occupational resources is indicated for blacks by this result, a result that is in accordance with the known concentration of blacks at lower levels of occupational achievement and their lower levels of occupational resources, and hence smaller dispersion in these attributes.

CONCLUSION

The occupational achievement process is a change process. Few persons' status and income remain constant over their lifetime. The change in status and income is brought about by mobility. This paper derived a stochastic model of mobility from a simple model for the occupational achievement process.

The intimate relation between occupational achievement and intragenerational mobility seems obvious. Research in the two areas has nevertheless diverged rather sharply. Status attainment research has focused on the level of status and income achieved with little concern for analyzing change in occupational achievement. Much mobility research has been directed toward the formulation of stochastic models of mobility, with little concern for the forces that generate both mobility and occupational achievement. However, research that synthesizes the two traditions seems needed if basic problems in both areas are to be solved.

From the perspective of status attainment research, a synthesis is necessary to provide a framework for interpreting and measuring the effect of individual attributes on occupational achievement and ascertaining how various structural characteristics modify these effects. Status attainment research sometimes leaves the impression that the measured effects of individual attributes on status somehow indicate the importance of these attributes for the degree of inequality in society. In particular, the effect of education has been interpreted in this way (Jencks et al., 1972). This inference runs counter to most theories of inequality which explain inequality by power, class, functional importance, etc., but not by the occupational resources of persons. If the structure of inequality is seen as fairly independent of the distribution of individual attributes such as education, then the occupational achievement process should be conceived of as an allocation process. Individual attributes are important for this process, but so is the availability of jobs at various status and income levels. This means that the achievement process should be seen as the outcome of an interplay between structural and individual characteristics, a conception that can be implemented by focusing on change, that is mobility, as does the proposed model.

From the perspective of mobility research the need for a synthesis derives from the need to obtain a mathematical model that not only fits the data, but also gives some useful information about the process. A model is needed to summarize the wealth of information contained in intragenerational mobility data, but this can hardly be a goal in itself. The objective should be to establish a theory of mobility in mathematical form. The simple Markov model is an unreasonable theory and also fits the data poorly. The needed modifications of the model should, however, be guided by considerations of why mobility takes place, not solely by statistical considerations, and a theory of the occupational achievement process is essential for such an effort.

This paper represents the beginning of such a synthesis. Only one part of the process has been dealt with: the rate of mobility. However, it was possible to derive a formulation of the dependency of this rate on time that is implied by a theory of the achievement process, and this formulation seems a reasonable approximation to reality. The model conceived of mobility as generated by an interplay between individual and structural characteristics; more specifically mobility was seen as the outcome of individuals' attempts to improve their occupational achievement in an occupational structure, where opportunities for better jobs are finite.

The proposed model is a simplification; it ignores differences in rates of job shifts not caused by the opportunity structure and also does not incorporate the influence of change in the opportunity structure on the rate of shifts. The model only describes voluntary shifts. Finally, the estimation techniques used

should be improved. These are problems for further research.

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THE OCCUPATIONAL ACHIEVEMENT PROCESS, 1940-1949: A COHORT ANALYSIS*

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Cohort analysis of the process of occupational achievement over the period 1940-1949 indicates mean occupational status was virtually constant and the net effect of father's occupational status on his son's was unchanging over the decade. The net effect of son's educational attainment on son's status changed substantially through the succession of cohorts. It is suggested that the change in the education slope was produced by a change in the educational quality of the labor supply between successive cohorts, as mean educational level has increased while the variance of education has declined.

Sociologists have long been interested in whether or not the American stratification structure has been changing. Earlier, the issue revolved around whether or not the class structure was becoming more rigid, more open or remaining unchanged (cf. Duncan, 1968b; Jackson and Crockett, 1964 for reviews of this literature). Over the past decade, the demographic approach to the study of stratification has shifted research concern from class structure to the processes by which socioeconomic status is achieved.

The research reported here is concerned with whether or not change has taken place in the processes of occupational achievement. More specifically, the objective is to determine if change has taken place in mean occupational status and in the influence of two determinants of son's status—his education and his father's status origins—using the same groups ten years later. The careers of men in 1940 may be different in 1949 because of the effects of a world war during the time they entered the labor force, became experienced job holders and possibly advanced to the peak use of their abilities. Second, we will compare age groups starting their careers ten years apart for mean change in prestige and changes in the process of status attainment. We would expect that different age cohorts have different experiences because they were born at different times, entered the educational system (with its changing content) at different times and the labor force in different economic circumstances, and were influenced by historical events at different points in their careers. While the time period is a brief one—a decade—the set of data is of interest because it pertains to a historical period about which not much is known otherwise.

Charting change in the relative effect of father's status or son's education on son's status has been carried out with longitudinal models representing the socioeconomic career (Duncan, 1966; Blau and Duncan, 1967; Featherman, 1969; 1971; Kelley, 1973) or else with approximations of historical time series (Duncan and Hodge, 1963; Duncan, 1968b; Tully et al., 1970). In contrast, the analysis presented in this paper examines

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change by means of intra- and intercohort comparisons of age groups. There are several reasons for such an approach. First, with one exception (Duncan and Hodge, 1963), previous studies have not made both intra- and intercohort comparisons. Second, instead of intercohort comparisons of different age groups from one cross-sectional sample, intercohort comparisons of groups at the same point in the life cycle but in different years will be made. Third, the Featherman-Kelley intracohort models were developed for one narrow age group; although the intracohort model employed in this study does not attempt to represent a socioeconomic career model, it is specific to age categories which more adequately represent the age range of the labor force. Finally, one can observe whether or not the summary measures for the total sample and the intra- and intercohort comparisons of age groups change in the same or different directions from each other, providing some insight into the sources of change in summary measures.

DATA AND PROCEDURE

The data for this study are taken from the Six City Survey of Labor Mobility, conducted in 1951 (Palmer, 1954), in which over thirteen thousand sample cases of men and women were collected in New Haven, Philadelphia, Chicago, St. Paul, San Francisco and Los Angeles. Complete work histories for the period 1940 to 1951 were obtained from all members in the sampled households who were employed at least one month in 1950. For the present analysis, 5,814 white males of nonfarm origins between 25 and 64 years of age in 1951 were retained in the sample. Whites of nonfarm origin were chosen since it could not be assumed that the status accorded to farm and nonfarm occupations is similarly ranked, and since the achievement experiences of Negroes (the bulk of our nonwhite sample) diverge considerably from the experiences of whites due to discrimination (Blau and Duncan, 1967; Duncan, 1968a).

Pertinent variables extracted from the work histories for the analysis are: respondent's age in 1951, educational attainment, his occupation in 1940 and 1949 and his father's longest-held occupation. Every calculation was performed within each of four age cohorts, those aged 25-34, 35-44, 45-54, and 55-64 in 1951.[1]

Status of father and son was measured with prestige scores assigned to each occupational title represented in the sample. Census titles were used to classify occupations in the Six City Survey. A study conducted in 1964 by the National Opinion Research Center on the prestige accorded to occupations has produced a score for each detailed Census occupational title.[2]

The work histories contained occupational information for January, 1940, and December, 1949. These two dates were chosen as end points for the analysis, because they are almost ten years apart, and this time span could be coordinated conveniently with the available age groupings. For example, males 35-44 in 1949 could be assumed, with only small error, to be age 25-34 in 1940.

To cope with missing data on education, father's occupational prestige, or son's occupational prestige in 1940 and 1949, several guidelines were followed. First, all cases lacking data on son's education and father's occupation were excluded. Second, all men over 35 in 1951 who did not have a prestige score for both 1940 and 1949 were excluded. On the other hand, since men 25 to 34 in 1951 had probably initiated their civilian careers after 1944, these were excluded only if they failed to report an occupation in 1949. The number of excluded men was small (about 400) and their exclusion is unlikely to have affected the analysis. (The sizes of the age groups generated by these selection criteria are shown in Table 2.)

The basic structural model underlying re-

[1] To make labeling of age cohorts less confusing, a convention will be adopted that men had the same age in 1949 as they had in 1951, and that they were ten years younger in 1940 than they were in 1951. Thus, men 35 to 44 in 1951 would be labeled 35 to 44 in 1949, and 25 to 34 in 1940. This convention has no effect on the analysis.

[2] This study was conducted under the direction of Robert W. Hodge. The major report of the study will be published in a forthcoming volume. Hodge et al. (1964) have shown that occupational prestige scores have been stable for at least fifty years. Thus, to use 1964-specific scores on occupations held in 1940 or 1949 involves little problem due to the instability of scores over time.

gression relations for both the intra- and intercohort analysis is:[3]

$$S_j = a + bE_j + cF_j + e_j \qquad (1)$$

where (leaving the index for individual implicit) j = jth age group, E = years of school completed, F = prestige of father's longest job, S = son's occupational prestige (in 1940 or 1949) and e is an unobserved random variable with mean assumed to be zero which represents the difference between the true value of S and the value predicted for S by the model. Regressions estimating son's prestige in 1949 were obtained for the total subsample and four age groups; similar estimates for 1940 prestige were made for the total sample[4] and the three oldest age groups.[5]

Differences between 1940 and 1949 in mean son's occupational prestige and in the unstandardized partial regression coefficients are examined subsequently in order to make the other cohort comparisons.[6] The unstandardized regression coefficients are used since they are more appropriate for comparisons between age groups.

[3] Some readers may question whether this is the most appropriate model. If the analysis were specific to only an intracohort model, then a "fuller" model including prior occupational status would be preferable. However, such a model would considerably reduce the size of the direct effect of education or father's status, as a large portion of the effect would be indirectly expressed through prior occupation. No such reduction in the direct effect would occur, of course, with the intercohort comparisons. Further, it is not clear that an intercohort model matching a "fuller" intracohort model is available. Hence, the simpler model of equation (1) is used to insure comparability between the inter- and intracohort estimates.

[4] The total estimate for 1940 was based on those 35-74 in 1951 since they were roughly 25-64 in 1940, and thus match those 25-64 in 1949. The occupational distribution of employed men aged 65-74 in 1951 closely resembled that of men aged 55-64 in 1951, and any error introduced by including employed men past retirement age should be minimized.

[5] Regressions for 1940 prestige were not made for the youngest age group, 25-34 as of 1951, since their 1940 job status when they were 15-24 was not likely to be meaningful to their adult careers, due to uncompleted schooling as of 1940 and to subsequent military service.

[6] Significance testing of differences between means or regression coefficients takes into account that intracohort comparisons are based on dependent samples while intercohort comparisons are based on independent samples. With intracohort comparisons, the same people are included in the estimations made for 1940 and 1949, with the result that prestige in 1940 is correlated with prestige in 1949, and the error terms in equation (1) calculated for 1940 and 1949 are correlated. With the intercohort comparisons, the samples are independent of each other—for instance, sons 25-34 in 1949 were compared with sons 35-44 in 1949 (who were 25-34 in 1940).

Tests of significance between means for dependent and independent samples are available (Minium, 1970:277-82). A test of the difference between regression coefficients for dependent samples (intracohort comparisons) was provided by the following equation:

$$S_{49j} - S_{40j} = \acute{a}_j + \acute{b}F_j + \acute{c}E_j + \acute{e}_j \qquad (2)$$

where $S_{49} - S_{40}$ is the difference between son's status in 1949 and 1940; all other terms are defined as in equation (1). The regression coefficients in equation (2) are the same as the differences between the regression coefficients estimated for son's 1940 and 1949 prestige, respectively, using equation (1). This can easily be seen by subtracting equation (1) estimated for 1940 from that estimated for 1949, or (leaving the age group index j implicit):

$$S_{49} - S_{40} = a_{49} - a_{40} + (b_{49} - b_{40})F + (c_{49} - c_{40})E + (e_{49} - e_{40}).$$

Since $S_{49} - S_{40}$ is also the dependent variable in equation (2), then $\acute{a} = a_{49} - a_{40}$, $\acute{b} = b_{49} - b_{40}$, $\acute{c} = c_{49} - c_{40}$, and $\acute{e} = e_{49} - e_{40}$. The standard errors of $\acute{b}$ and $\acute{c}$ are available for a test of the differences between regression coefficients. Indeed, equation (2) is the more direct and simpler procedure for intracohort analysis, but it does not provide all of the estimates used in this analysis; hence, the preference for equation (1).

An analysis of covariance procedure can be used to test for the difference between regression coefficients for intercohort comparisons. Let S_j represent prestige of sons in the jth age group in different years, F and E defined as in equation (1), Y is a dummy variable for the year 1949 (where Y = 1 if son's prestige score was observed in 1949, 0 if in 1940), and YF and YE are product terms which represent the effect of father's prestige and son's education on son's status in a particular year. Thus, for the jth age group,

$$S_j = a + b_1F + b_2E + b_3Y + b_4YF + b_5YE. \qquad (3)$$

For example, comparing sons 25-34 in 1940 to sons 25-34 in 1949, for sons 25-34 in 1940, Y = 0, and equation (3) reduces to $S_{25-34} = a + b_1F + b_2E$, and for sons 25-34 in 1949, Y = 1, and $S_{25-34} = (a + b_3) + (b_1 + b_4)F + (b_2 + b_5)E$. The coefficients b_4, b_5 indicate whether or not the slopes are different in 1940 and 1949, and b_3 whether or not the intercepts differ. Tests of significance for b_3, b_4 and b_5 are available for comparison to their respective standard errors (cf. Melichar, 1965:382). The analysis of covariance procedure is equivalent to the test for differences between regression coefficients for independent samples, using the standard error of the difference between b's (cf. Melichar, 1965:382 for discussion of this test).

Table 1. Means and Standard Deviations of Son's Occupational Prestige, 1940 and 1949, with Cohort Changes, by Age

	Age[a]	Son's Prestige		Change	
		1940	1949	Intracohort	Intercohort
		Means			
	25-34	37.9	39.0	-	1.1
	35-44	40.2	40.2	2.3*	0.0
	45-54	40.5	40.8	0.6	0.3
	55-64	-	40.4	-0.1	-
Total	25-64	39.2	40.1	-	0.9
		Standard Deviations			
	25-34	13.2	13.1	-	-0.1
	35-44	14.1	12.9	-0.3	-1.2
	45-54	13.7	14.0	-0.1	0.3
	55-64	-	13.6	-0.1	
Total	25-64	13.6	13.4	-	-0.2

[a]Age is as of year of son's prestige.

*Significant at .05 level.

FINDINGS

Examination of change over time in the process of occupational achievement begins at the aggregate level with the means of son's 1940 and 1949 occupational prestige for the total sample disaggregated by age (Table 1). The mean status of all sons increased only 0.9 points over the decade. Cohort comparisons reveal the source of this slight improvement by age group and intra- and intercohort change. Examples of cohort comparisons from Table 1 illustrate the approach. An intracohort comparison (the same people at different times) involves, for instance, the mean prestige level of sons 25-34 in 1940 compared with their prestige level in 1949 when they were 35-44 (37.9 vs. 40.2). To make the alternative intercohort comparison (different people at the same age at different times), sons 25-34 in 1940 are compared with sons 25-34 in 1949 (37.9 vs. 39.0). Similar cohort comparisons of the means can be made for other age groups; these differences are summarized in Table 1.

Table 1 shows that changes in mean status levels for the total sample and for older men were slight or essentially zero, indicating that the occupational structure of these six cities was virtually stable over the decade of the 1940s. The intra- and intercohort changes in mean status levels for men 25-34 do not modify this conclusion. The intercohort mean status increase of 1.1 indicates that the supply of statuses available to younger men beginning their careers improved, but to a minimal extent. The intracohort mean increase of 2.3 points shows that young individuals experienced job mobility on the average, but it says nothing about whether the supply of statuses open to individuals of that particular age group changed appreciably.

A possible explanation for the age differential might be selective inmigration to these cities of young people from areas with lower occupational distributions. However, results from this study not presented here show that those who inmigrated between 1940 and 1949 are no different in mean occupational status in 1940 or 1949 from those residing in these cities in both 1940 and 1949, regardless of age. Nor are these results due to veteran

status. Such an age differential does not seem unlikely—the most recent labor force entrants have more high quality education (Table 2) than do older workers which should enable them to compete more effectively than older men for new jobs requiring skill and preparatory training. In addition, the young have not built up seniority or other attachments to a particular job or firm which would impede changing jobs.

So far, the differences between mean prestige levels among sons in 1940 and 1949 have been examined. The means, however, only provide information on average status outcome; they do not reveal how sons achieved a given status level. This section of the analysis turns to determinants of status which are known to be important—son's education and his father's status. The unstandardized partial regression coefficients estimated from equation (1) for age groups are compiled in Table 3. Cohort comparisons analogous to those made with mean son's status were made on the net effects of son's education and father's status on son's status in 1940 and 1949. Table 3 contains the intracohort and intercohort comparisons.

The middle panel shows that change in the effect of father's occupation at the aggregate level or by cohorts is minimal and can be regarded as essentially zero. That is, the influence of father's status on son's status is stable over time.

This is not true for the effect of son's education. Comparisons for the entire sample indicate that education was somewhat more important for son's status in 1949 than it was in 1940 (the net coefficient increased 0.143). Two of the three intercohort comparisons show significant increases in size.[7] The exception for those 35-44 is a small difference, inconsistent with the other two. Further analysis of this exception suggests that it can be disregarded.[8] The significant intercohort differences indicate that the education-occupation relation was tighter in 1949 than in 1940 for those at the same life cycle stage.

In contrast, the intracohort differences between the net effects of education on son's status were small, inconsistent in sign and not statistically significant for all age groups. All such differences can be regarded as zero. Thus, a year of education resulted in the same increment to prestige when sons were ten years older, at all ages. Status returns to education did not change over time, taking an intracohort approach. Since no change occurred in the coefficients as each age group aged ten years, change in the status returns to education at the aggregate level appears to have occurred independent of job mobility. The succession of cohorts was the sole source of change in the linkage between occupation and education.

For age groups starting their careers ten years apart, the status returns for an additional year of education were greater in 1949 than in 1940. That is, in each successive cohort the same supply of statuses is distributed to individuals more and more on the basis of education. We do not suggest that employers use education more or that they raise educational entry requirements in assigning jobs, but that they are faced with an improvement

Table 2. Means and Standard Deviations of Son's Educational Attainment, by Age in 1951

	Age	Mean	Standard Deviation	N
	25-34	11.6	2.7	1646
	35-44	10.8	3.0	1810
	45-54	9.9	3.5	1475
	55-64	9.1	3.7	883
Total	25-64	10.5	3.3	5814

[7] Possibly the intercohort changes are due to sampling error. However, this does not seem likely since sizeable intercohort differences are systematically repeated within each of five of the six cities. New Haven is the exception. Of the five cities where intercohort change appeared, 12 of the possible 15 intercohort comparisons of the net slopes of son's education and his occupation are larger than .200.

[8] The age comparison of men 35-44 was further disaggregated by city. Within each of the six cities, half of the intercohort differences in the education effect were positive and half were negative. Over all cities, the negative and positive slopes essentially balanced each other, producing a small and negative result for the whole age comparison. Part of the city-specific age comparisons of men 35-44 are in line with the increase in the size of the education slope, giving support to the finding for the other two age comparisons of positive intercohort increases.

Table 3. Partial Regression Coefficients Relating Son's Occupational Prestige to Son's Educational Attainment or Father's Occupational Prestige, 1940 and 1949, with Cohort Changes, by Age

	Age[a]	Son's Prestige		Change	
		1940	1949	Intracohort	Intercohort
		Coefficients for Education ($b_{SE.F}$)			
	25-34	2.040	2.416	-	.376*
	35-44	2.139	2.018	-.022	-.121
	45-54	1.551	2.142	.003	.591*
	55-64	-	1.538	-.013	-
Total	25-64	1.673	1.816	-	.143
		Coefficients for Father's Occupational Prestige ($b_{SF.E}$)			
	25-34	.130	.144	-	.014
	35-44	.094	.111	-.019	.017
	45-54	.121	.076	-.018	-.045
	55-64	-	.140	.019	-
Total	25-64	.156	.144	-	-.012
		Intercepts			
	25-34	10.90	5.53	-	-5.37
	35-44	15.39	14.19	3.29	-1.20
	45-54	21.39	16.66	1.27	-4.73
	55-64	-	20.68	-0.71	-
Total	25-64	16.39	15.33	-	-1.06

*Significant at .05 level.

[a]Age is as of year of son's prestige.

in the quality of new entrants to the labor force.

The long-term national trend in the educational distribution of the labor force has been toward higher mean educational attainment with a reduction in the proportion of persons with no or very few years of education. The sample in this study shows patterns of educational attainment in line with the long-term trend. Table 2 indicates that mean education increased about 0.8 years between age groups while the standard deviation of education declined steadily the younger the age group. Thus, employers have been confronted with a change in the quality of supply of successive cohorts—each new cohort is better educated on the average with relatively fewer persons of low education.

Since employers use educational levels as the crucial credential for employment, the labor market, in effect, maintains the correlation between education and job status. The correlation coefficients for these data (not shown here) did not change from 1940 to 1949. With constant correlations and decreasing variance in education, the shift in the regression slopes perforce follows.

This argument can be further supported by

a graphic illustration of the process. Although a more thorough treatment would consider both supply and demand factors affecting the shape of the occupational structure, for the purposes of our discussion we will limit it to that of changes in the educational quality of the supply only. Such a decision is supported by our data—changes in the occupational structure in these cities (which would presumably reflect the demand for various types of labor) between 1940 and 1949 were negligible. The overall mean occupational status of the sample increased only 0.9 points, change in the standard deviation of son's prestige (Table 1) was negligible, and an index of dissimilarity between the occupational distributions of 1940 and 1950 computed from Census data for these cities was only 4.2. Thus, in the following discussion, we assume a virtually unchanged structure.

What effect would a change in the educational quality of the supply of workers have on the allocation of persons of different educational attainment to jobs, assuming an unchanging occupational structure? Figure 1 illustrates the proposed argument. The distribution of jobs is assumed constant; between 1940 and 1949 for any two given cohorts of the same age (for instance, men 25-34), the distribution of education changes according to the trend in the standard deviation and mean of education discussed above. The hypothetical consequences for the slopes are also presented. (The hypothetical slope for 1940 is superimposed on the graph for 1949 to show the change.)

Compared to 1940, employers in 1949 filling the top 20% of jobs would not have to go as far down in the educational distribution to get the men they need for those jobs, because relatively more men would be concentrated in the upper ranges of the educational distribution than in the past. The range of educational levels of the supply of workers which fill jobs requiring highly educated personnel will be smaller because the upper limit of the range does not change while the lower limit moves upwards. As a result, within the corresponding sector of the regression plane, the slope should be steeper because more workers in 1949 are concentrated in the left-hand side of that sector than in 1940. For the bottom 20% of jobs, the supply of less well educated workers is smaller, so employers will have to hire a better educated worker than they did ten years earlier. The slope should be steeper because more workers are concentrated in the right-hand side of that sector of the regression plane. For the middle 60% of jobs, the slope should be steeper because the range of education of the labor supply which would fill those jobs would be smaller, and workers will be located at each end of that sector of the regression plane in patterns similar to the adjacent sectors. Overall, then, the regression slope should become steeper simply as a response to the decreasing variation and increasing mean attainment of the labor supply.

Other plausible alternative explanations for the intercohort change were considered: (1) change by employers in the entry requirements for jobs; (2) expanded white-collar job opportunities for those with high educational attainments and reduced blue-collar job opportunities for less well educated men; (3) selective inmigration and (4) historical effects due to the Depression or World War II. However, examination of these hypotheses against the data produced results inconsistent with the intercohort changes in regression slopes.

Closer examination of the education-occupation relation is warranted before concluding that the intercohort differences show that education was more important for status attainment in 1949 than in 1940. The lower portion of Figure 1 illustrates this for those 25-34. Between two of the three age constant comparisons, the regression slope is steeper in 1949 than in 1940, mean prestige is constant, mean education increases and thus the intercept value is necessarily lower in 1949 than in 1940.

Figure 1 reminds us that the inference that education became more important needs to be specified carefully. While status returns for an *additional* year of education were greater in 1949 than in 1940, status returns to a fixed amount of education did not increase at all levels in 1949 compared to 1940. A man with low education in 1949 received less in status returns than an equivalent man in 1940. Meanwhile a man with high education in 1949 achieved more in the way of occupational status than his counterpart in 1940.

The intercohort change in the slope $b_{SE.F}$ is one of large magnitude. This may be understood in two ways. First, the intercohort difference in $b_{SE.F}$ in the Occupational

Changes in a Generation-I (OCG-I) data set for son's first job was large (.739), comparing sons 16 in 1933-42 and 1943-52. In the Six City data for the most comparable age group (sons 25-34) the intercohort change between 1940 and 1949 was only somewhat smaller—.376. Second, assume that a constant difference of about .4 between successive cohorts has been true for the last ten decades. Extrapolating backwards in time would mean that $b_{SE.F}$ in 1850 would be negative, or roughly −1.6 for sons 25-34. While education may have been much less important for son's status in 1850 than in 1950, it seems unlikely that high status jobs were filled primarily by the little educated while low status jobs went to the better educated. Thus, the long-run intercohort change in the relation between son's education and his occupational status must have been much more modest. In this perspective, an intercohort change of .376 is quite substantial.

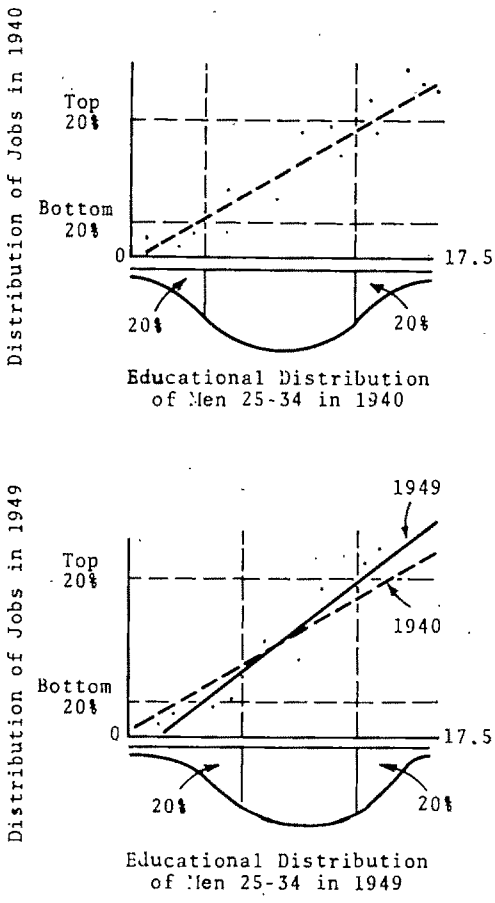

Figure 1. Educational Distributions in 1940 and 1949 and Hypothesized Effect on Relation between Education and Occupational Prestige (1940 Regression Slope Superimposed on 1949 Graph to Show Differences in Slopes)

Are the Six City cohort results in the effect of education on son's status an oscillation around some long-term trend, or are they reflective of the trend itself? The brevity of this time series, 1940-1949, does not allow us to answer this conclusively. However, some observations can be made to put the intercohort changes in perspective. First, the intercohort change found in the Six City data is probably not reflective of the long-term trend. The extrapolation backwards of an assumed constant .4 intercohort change in the influence of education on prestige over ten decades leads us to reject the implied value for 1850, and we conclude that any trend must have been either a lower constant rate of change or else possibly an oscillating rate of change over time. There is some suggestion that the latter might be true from the OCG-I data. The intercohort difference in the slope for son's first job $b_{SE.F}$ changed from .189 to .046 to .739, comparing sons 16 in 1923-1932, in 1933-1942, and in 1943-1952.[9] That is, since about World War II, intercohort change in the education-occupation linkage has become even greater, and the Six City intercohort differences between 1940 and 1949 are in line with this pattern. Comparison of data from OCG-I and OCG-II should provide more insight into the nature of any trend. Our time series is simply too short to answer whether the intercohort results are part of a trend with an increasing rate of change or else a short-term fluctuation.

Lastly, we consider the intercohort change in the relation between father's occupation and son's education, since it may reveal a change in the indirect means by which fathers pass on their status to their sons. The direct transmission of status from father to son did

[9] The values of the unstandardized regression coefficients were calculated from data in Tables 5.4 (Blau and Duncan, 1967:181) and A.5-A.8 (Duncan, Featherman and Duncan, 1972:267-70).

not change over this decade, either in intra- or intercohort comparisons of the zero-order correlations (not shown here) or in the net regression coefficients. Since $b_{SF} = b_{SF.E} + b_{EF}b_{SE.F}$ (Land, 1969:14), the intercohort change in b_{SF} and $b_{SF.E}$ is zero and the intercohort change in $b_{SE.F}$ is positive, we would expect that the intercohort change in b_{EF} would be negative. That is, for successive cohorts, the ascriptive effect of father's occupation via son's education should be declining over time while the importance of son's own achievements increases. Such a result would indicate that while fathers directly pass on their status to the same extent to their sons, the indirect mechanisms of status transmission have changed.

Table 4 presents the coefficients for the regression of son's education on father's occupational prestige and the intercohort differences between age groups ten years apart in age. Only two of the three differences are in the expected negative direction, and the pattern of these intercohort differences does not correspond to the pattern of intercohort differences in the regression slopes for the relation between son's education and occupation. Thus, the data are inconclusive about change in the ascriptive effect of father's occupation for son's educational attainment.

Table 4. Zero-Order Regression and Correlation Coefficients Relating Son's Educational Attainment to Father's Occupational Prestige, by Age in 1951

	Age	Son's Education	Intercohort Change[a]
		Regression Coefficients	
	25-34	.649	-.194
	35-44	.843	-.111
	45-54	.968	.122
	55-64	.846	-
Total	25-64	.779	-
		Intercepts	
	25-34	9.075	1.473
	35-44	7.602	1.506
	45-54	6.096	0.405
	55-64	5.691	-
Total	25-64	7.509	-
		Correlation Coefficients	
	25-34	.324	-.040
	35-44	.364	.007
	45-54	.357	.052
	55-64	.305	-
Total	25-64	.312	-

[a]Intercohort change is the difference between coefficients for a given age group and the age group ten years older.

SUMMARY AND CONCLUSIONS

This study investigated change over time in the process of occupational achievement. Previous work on change in this process has been limited for the most part to summary comparisons between time points. However, between any two time points, shifts may take place through the succession of cohorts or through intracohort changes, and such movements may be independent of each other. Through cohort analysis of data for the nineteen forties, it was demonstrated that change in the mean status of sons was negligible, except for the youngest age cohort; change in the effect of education on son's status took place only via the succession of cohorts, and the influence of father's occupation was unchanging over time.

The constant direct transmission of status from father to son contributes more evidence that the stratification structure was not growing more rigid, a conclusion in line with those of Duncan (1968b) and Jackson and Crockett (1964). The lack of change over a fifty-year period in the father-son status connection (Duncan, 1968b) raises the question of what keeps it constant, since the occupational-industrial structure, the complexity of the labor force, and the skill and quality characteristics of labor changed considerably over the past half century. We suggest that academic success and motivation are family advantages transmitted regardless of shifts in the economic structure.

It would be inappropriate not to mention some of the limitations of the analysis. First, the specification of our model may be in error, either due to the omission of relevant causal variables or to measurement error in the variables used in the analysis. The Six City data are based on retrospective work histories, and measurement errors in such reports may result in misestimates of the effects of socioeconomic background variables, although such misestimates may not be large. (See Hauser and Featherman, 1974:330 on this point.) Second, there is no necessary reason to think that changes observed for the sample drawn from these six cities reflect changes occuring in the society as a whole. And finally, the period of the 1940s may not be like other decades, and we are unable to determine whether these results are part of a long-term trend or a fluctuation around the trend.

While we do not regard our estimates of trends in attainment processes as definitive, we do think that cohort analysis provides important insights into the process of occupational achievement over this decade. The negligible change in mean occupational attainment indicates that the occupational structure was constant. Fathers handed on their status to their sons to the same extent, but part of the mechanisms changed in the indirect transmission of status via father's status to son's education to son's status. Both the OCG-I and Six City data show a large intercohort increase in the importance of education for job status over this period. We might speculate it is part of a long-term trend which accelerated sharply during the 1940s. Examination of this latter change deserves replication in other suitable sets of data for different time periods as such sets become available. It is clearly of importance to determine whether this is a trend or an isolated curiosity of this particular decade.

Finally, while it cannot be assumed that these six cities are wholly representative of the distribution of cities in the United States, that other periods are similar to the 1940s, or that sources of change in the past or future will be those identified in this analysis, the findings presented here are clearly consistent with Ryder's (1968) observation that "the continuing evolution of technological structure relies less on the retraining of older cohorts than on the recruitment of new ones."

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A COMPARISON OF MEN'S AND WOMEN'S INTERGENERATIONAL MOBILITY IN THE UNITED STATES*

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A nationally representative sample provides the basis for a comparison of married men's and married women's intergenerational mobility in the United States; men's occupational mobility is compared with women's marriage mobility. Women are found to have greater mobility through marriage, both upward and downward, than men do through occupations, and women more readily cross the boundaries among the major status groupings of white-collar, blue-collar and farm. Men, on the other hand, are more likely to "inherit" their fathers' statuses and there is greater association between the statuses of fathers and sons than between the statuses of fathers and daughters. An analysis for historical trends in the data indicates little evidence for either increasing fluidity or rigidity in the mobility system of either sex.

The social mobility of women and their part in the social stratification process in general has not been systematically investigated by sociologists. For example, many of the recent surveys and readers in the stratification literature make only the most cursory mention, if any mention at all, of the mobility of women or their place in the stratification system (e.g., see Barber, 1957; Bendix and Lipset, 1966; Blau and Duncan, 1967; Lauman, 1970; Svalastoga, 1964). Glenn et al. (1974) report that in a recent bibliography on social mobility only about one percent of the listings dealt primarily with women. Acker (1973:936) sums up this lack of attention to women in the stratification literature:

> In the last 10 years, empirical studies and speculative discussions on the disadvantaged status of women have increased rapidly. Although social inequality is the subject matter of social stratification studies, little of this work on the position of women has been done by sociologists in the field of social stratification. Indeed, sex has rarely been analyzed as a factor in stratification processes and structures, although it is probably one of the most obvious criteria of social differentiation and one of the most obvious bases of economic, political, and social inequalities. Very few sociologists have even recognized that we have, with the exception of the study of the family, constructed a sociology that tends to deal with only the male half of humanity.

In the last few years, sociologists have been rushing to fill this void in the stratification literature and a number of articles have appeared comparing various forms of men's and women's mobility. The logic behind these comparisons is that in order to discover the distinctive features in one stratification system, it is helpful to compare it with other stratification systems. A number of these comparisons have been based upon nationally representative samples for the United States. Rubin (1968) has compared men's and women's marriage mobility by relating wives' origin statuses, determined by fathers' occupations, to husbands' origin statuses, likewise

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determined by their fathers' occupations.[1] DeJong et al. (1971) have contrasted men's and women's occupational mobility, and methodological and substantive reviews of their work have been given by Havens and Tully (1972) and Tyree and Treas (1974). Comparisons of men's mobility through occupations and women's through marriages have been provided by Glenn et al. (1974) and Tyree and Treas (1974). The comparisons of men's occupational and women's marriage mobility are of particular interest since in the United States and indeed much of the industrialized world the main determinant of a man's social status is his occupation, and the main determinant for a woman is the social status of her husband. As Rossi (1971:110) states, "What a man 'does' defines his status, but whom she marries defines a woman's. In meeting strangers one can 'place' a man socially by asking what he does, a woman by asking what her husband does."

This article is also a comparison of men's occupational and women's marriage mobility for a nationally representative sample of the United States; the mobility of husbands from their fathers' occupations to their own first jobs will be compared with the mobility of wives from their fathers' occupations to their husbands' first jobs. The research reported here makes significant improvements in both data and methods over the two previous comparisons of men's occupational and women's marriage mobility and will, in addition, help to supplement and clarify the results of those studies. In the discussion that follows, I shall first briefly describe the sample and methods used by each of the studies and then point out how certain problems in these earlier studies will be alleviated here. A comparison between the findings here and those of the earlier studies will be given at the end of this article in the Discussion section.

Tyree and Treas' (1974) data were taken from the Occupational Changes in a Generation (OCG) sample prepared by the United States Bureau of the Census in 1962 and utilized in Blau and Duncan (1967). Data from this same sample is used here, and the sample will be described in greater detail below. Tyree and Treas' data for men's occupational mobility are based upon men's movements from their fathers' occupations to the statuses determined by their own jobs at the time of the OCG survey. Only married women not working in the civilian labor force, a little less than half of all married women, were included in their sample, and in similar fashion to men, their mobility is given by movements from their fathers' occupations to their husbands' jobs at the time of the survey. Most of their analysis was based upon a technique of marginal adjustment of men's and women's mobility tables; this technique will be discussed in greater detail below. In one case, men's and women's mobility tables were each adjusted to the same uniform marginals; each column and row total was adjusted to be equal to 9.0, the number of occupational categories used. In the other case, men's and women's mobility tables were each adjusted to the origin distribution of statuses for men in OCG (the origin distribution for males in their data to begin with) and to the destination distribution given by the total labor force (men and women combined) in the 1960 Census.

The data used by Glenn et al. (1974) came from four surveys: the 1966, 1968 and 1970 Michigan Survey Research Center Election Studies and the 1972 General Social Survey of the National Data Program in Sociology. Men's mobility was indicated by movements from their fathers' occupations to their own occupations at the times of the surveys and wives' mobility by changes from their fathers' occupations to the occupations of their husbands at the times of the surveys. A variety of methodological techniques were used in the comparison of women's and men's mobility tables: outflow percentages, correlation between origin and destination statuses and a number of summary measures including percent of men and women having upward, downward and no mobility from their origins and per-person upward and downward mobility (see Glenn et al., 1974 for details).

In both of these previous studies, a subject's status is given by his own occupation or the occupation of a husband, in the case of women, at the time of a survey. Thus the

[1] The work of Rubin (1968) and the earlier analysis of similar data by Centers (1949) is weakened by problems with the methodologies that each employed and by restrictions in their data sets. The problems of their studies and a comparison of men's and women's origins in the OCG data are discussed in Chase (1973).

statuses of subjects in these studies were determined by a mixture of men who had just started their careers, who were in mid-career and who were nearing retirement. The statuses of subjects are an inhomogeneous blend since older cohorts of men would have been exposed to many more mobility opportunities, on the average, than younger men. This problem resulting from a mixture of career stages can be alleviated by what Glenn et al. (1974:687, footnote 6) refer to as "the often-stated but rarely achieved ideal of comparing occupations in different generations at the same stages of the careers." In this article, therefore, men's occupational mobility is defined as the movement of men from their fathers' occupations to their own first jobs, and in comparable fashion, movement from fathers' occupations to husbands' first jobs defines marriage mobility for women. In addition, since all men and women are at the same stage in their "careers," they can be divided into cohorts to check for the presence of historical trends and for the stability of findings across independent subsamples.

As many researchers in mobility have stated, a method which is not influenced by marginal differences is needed for comparing the mobility tables of two groups (cf. Glass, 1954; Rogoff, 1953; Rubin, 1968; Svalastoga, 1959). That is, they were willing to take the marginal distributions as given by economic and demographic conditions, but they wanted to contrast entries in comparable cells of two mobility tables in a way which did not take into account the differences in marginal frequencies between the two mobility tables. The mobility ratio and the transition matrix, or outflow percentages when the transition probabilities are expressed as percents, were developed by mobility researchers as techniques to remove marginal effects in mobility comparisons (e.g., see Glass, 1954; Kemeny and Snell, 1960; Rogoff, 1953; Svalastoga 1959). However, both Blau and Duncan (1967) and Tyree (1973) have shown that the mobility ratio and the transition matrix (outflow percentage) do not achieve their intended purpose and are influenced by marginal differences across tables. Many of the techniques used by Glenn et al. in the comparison of men's occupational and women's marriage mobility—outflow percentages; percent of men and women with stable, upward and downward mobility; and per-person measures of mobility—are influenced by the marginal differences between men's and women's mobility tables. Consequently, their results must be viewed with this understanding in mind.

Fortunately for the mobility researcher, there are techniques which remove the effects of marginal differences in the comparison of mobility tables. These techniques all have as their aim the adjustment of the cell entries of a mobility table, or in general a cross-classification table, so that the adjusted entries give some new, specified marginal totals. Various criteria may be used in adjusting the cell entries; for example, a least squares requirement where the sum of the squares of the differences between the original and adjusted entries is minimized or a requirement that the marginals of a table be adjusted while preserving the original interactions in the table (see Deming, 1943, for a discussion of various methods of marginal adjustment). Duncan (1966) used the least squares requirement in adjusting Rogoff's 1910 mobility table to the marginals of her 1940 data in order to determine if the mobility patterns of men in the study had changed over time, and Levine (1972) used the preservation of interaction requirement in the comparison of men's intergenerational mobility patterns in England and Denmark. It is difficult to determine which adjustment criterion is used by reading the discussion in Tyree and Treas (1974) since they do not clearly state their method, the mathematical formulation of their adjustment procedure is not internally consistent, and they do not provide their raw data.

The discussion in Tyree (1973), where the same mathematical formulation of the adjustment procedure as in Tyree and Treas (1974) is used, indicates, almost certainly, that they used the adjustment procedure which preserves interactions. The adjustment procedure which preserves interactions is used in this article, and I shall discuss its virtues in greater detail below in the Methods section. However, there are two shortcomings in Tyree and Treas' use of this procedure. First, the case where they used uniform marginals (all equal to 9.0) makes it difficult to interpret what differences in cell entries actually mean.[2] On

[2] Tyree and Treas (1974:295) give their rationale for choosing to adjust marginal totals to 9.0 as follows: "In the case of statistical independence

the other hand, if real marginal frequencies are used, for example, a men's mobility table is adjusted to have the same marginals as a women's table, then comparisons between the tables are based upon differences in actual numbers of people with a particular combination of origin and destination statuses. That is, with the use of real marginals, it would be possible to make the following sort of observations: "After the removal of marginal effects, 206 more women than men from farm origins had mobility to farm destinations, and 1700 more women than men, or 30 versus 28 percent, had upward mobility from their origins." Second, where Tyree and Treas did use actual marginal frequencies for the adjustment, their choice of destination distribution, the marginals from the total (men and women combined) labor force in 1960 is questionable. If we are examining the mobility of women through marriage, then we are interested in the occupational distributions of husbands, not those of both men and women in the total labor force.

DATA

The data analyzed in this article were collected in the Occupational Changes in a Generation (OCG) survey by the United States Bureau of the Census. The sample design, questionnaire administration and data tabulations for OCG were performed by the Census as an adjunct to the monthly "Current Population Survey" of March, 1962. The survey was conceived by and carried out under the supervision of Peter Blau and Otis Dudley Duncan; their book (1967), *The American Occupational Structure,* presents a detailed description of the OCG sample.

The approximately 20,100 men responding to the Census questionnaire represented the 45 million men between the ages of 20 and 64 in the civilian, non-institutional population of the United States in 1962. Women were not sampled directly, but men in the sample provided various data concerning their wives.[3] The sample frequencies were inflated so that all data tabulations became estimates of population frequencies. All data from OCG presented in this article show frequencies in thousands; counts should be multiplied by 1000 to give an estimate of the number of equivalent people in the United States in 1962.

The subsample from OCG that will be analyzed here is composed of white, married couples with the wife's age between 22 and 61.[4] The total size (in thousands) of the inflated subsample is about 27,000 each of men and women. The couples have been divided into eight cohorts based upon successive five-year intervals of wife's age.

The data from OCG are arranged in standard form mobility matrices; origin statuses are given by fathers' occupations and destination statuses by husbands' first jobs.[5] The status of a father was determined by his job when the son or daughter was about 16 years old.

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within the adjusted table, the adjusted cell frequencies take on a value of 1.0, a condition not necessary to the comparison, but convenient to those of us who have spent years looking at mobility ratios, where independence was also reflected with an entry of 1.0." This statement is somewhat misleading since it is only true if *all* interactions are statistically independent in a set of 2 x 2 subtables corresponding to the degrees of freedom for interaction in the adjusted table. Since it is extremely unlikely that all interactions in an adjusted table would show independence, an adjusted frequency of 1.0 would not necessarily indicate independence, as with the mobility ratio, but would have to be interpreted with that cell frequency as part of some possible 2 x 2 subtable. The suppositions of table adjustment are explained in greater detail in the Methods section below.

[3] Since husbands reported both their fathers' and fathers-in-laws' occupations, it is possible that some differences in men's and women's mobility could result from differential accuracy between a man's report of his own and his wife's origins. However, Blau and Duncan (1967) checked a number of retrospective variables and found fairly good levels of accuracy—levels comparable to other studies in which similar types of data were reported (see Blau and Duncan, 1967: Appendices D and E). The use here of broad occupational categories should be of further help in mitigating the results of inaccuracies in the reporting of origin statuses.

[4] The Census experiences a general problem of underrepresentation of non-whites in many of its samples; and, more specifically, the mobility tables for non-whites in OCG show the absence of many combinations of origin and destination statuses—over half the cells in their mobility tables have entries of zero. The mobility of non-whites was judged to be significantly different, therefore, from the mobility of whites, and non-white data were excluded from this analysis.

[5] Duncan et al. (1968:288) report that for native, non-Negro men, 25 to 64 years old in the OCG sample, the average age at the first full-time job after

The twelve occupational categories used here are derived from the original seventeen used by Blau and Duncan (1967) by combining retail and other salesmen into one category, all craftsmen into one category, all operatives into one category and all laborers except farm laborers into one category. The twelve categories form a reasonable consistent rank order using Blau and Duncan's (1967) ranking method on the criteria of median income and median years of schooling for occupational groups. The occupational categories from top to bottom are as follows: self-employed professionals, salaried professionals, managers, proprietors, clerical workers, salesmen, craftsmen, operatives, service workers, laborers, farmers and farm laborers. In five cases, there are negative percentage increases in median income or median years of schooling as one moves up the ranks from one occupation to another. In these cases, following Blau and Duncan (1967:26), the higher occupation is designated as the one which gives the largest positive percentage increase in either score. The one exception is that salesmen were placed above craftsmen to retain the nonmanual-manual distinction.

In undertaking this comparison of men's occupational and women's marriage mobility, no argument is made that the data used here describe men's and women's mobility in entirety. Both men and women have other sources of mobility, for example, through their own continuing careers, their own educational achievements, and men, as well as women, have marriage mobility. Thus the comparison of men's occupational and women's marriage mobility is a particularly important comparison but only one of several types of mobility comparisons.

METHOD

Since the data analyzed in this article come from married couples, the marginal frequencies for destination statuses in the men's and women's tables should be identical except for tabulation errors and differing rates of no response concerning origin statuses. An examination of the column marginals for the unadjusted men's data and the unadjusted women's data in Table 2 shows the totals to be close. However, the marginal frequencies for origin statuses can differ for a number of reasons. First, differing rates of marriage, divorce and mortality for women and men from the same origins result in differing marginals. Second, since the distribution of occupations has changed with time and since men are usually a little older than their wives, husbands and wives have somewhat different origin distributions. Third, there is the possibility that sampling inadequacies could lead to differences in men's and women's origin marginals.

For these reasons, and related ones in other mobility studies, we want a technique for comparing mobility tables that is not influenced by marginal differences. As mentioned above, the technique that is used here to achieve this aim is a marginal adjustment procedure which leaves intact the interactions in the original table; that procedure will be referred to as proportional adjustment hereafter. In this analysis, then, the following question about the differences between men's and women's mobility is asked: controlling for marginal differences, but keeping unchanged the original interactions in the mobility matrices, what is the pattern of differences between cell frequencies in comparable cells of men's and women's tables? Or, in other words, to be more specific: what is the pattern of differences between comparable cell frequencies in men's and women's mobility tables if the women's table is adjusted so that it has the same marginals as the men's table but the original interactions in the women's table remain unchanged?

The measure of interaction which is used in proportional adjustment and which remains

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leaving school was reported as 18.5 and the average age at first marriage was 24.3. They note that there is an inconsistency between the age at first job and the years of school completed by a considerable number of men in the sample. For example, the authors (1968:277) state that over a sixth of the men who had completed at least one year of graduate school report having taken their first full-time job after leaving school before the age of 19. The authors suggest that some men misreported either their education levels or age at first job or some left school, took a full-time job, and then later returned to school. Thus, it is highly likely that the average age at first job is higher than 18.5 and nearer the average age at first marriage. In any event, for the purpose of the present comparison of men's and women's mobility, a man should have his first job before he marries, and since the average age of men at their first jobs and first marriages are quite close, the status of a man's first job should be a good indicator of his status at the time of his first marriage.

Table 1. Illustration of the Table Adjustment Procedure

A. Target Table

Father's Occup.	Own Occup. High	Own Occup. Low	Total
High	10	5	15
Low	5	10	15
Total	15	15	30

$\alpha = 4.00$

B. Table to be Adjusted

Father's Occup.	Own Occup. High	Own Occup. Low	Total
High	10	100	110
Low	10	400	410
Total	20	500	520

$\alpha = 4.00$

c. Table B After One Row Adjustment Operation

Father's Occup.	Own Occupation: High	Own Occupation: Low	Total
High	$\frac{10(15)}{110} = 1.36$	$\frac{100(15)}{110} = 13.64$	15.00
Low	$\frac{10(15)}{410} = .37$	$\frac{400(15)}{410} = 14.63$	15.00
Total	1.73	28.27	30.00

D. Adjusted Frequencies of Table B Compared to Target Frequencies from Table A (Upper Value in Each Cell from Table A and Lower from Table B).

Father's Occup.	Own Occupation: High	Own Occupation: Low	Total
High	10.00 9.97	5.00 5.03	15.00 15.00
Low	5.00 5.03	10.00 9.97	15.00 15.00
Total	15.00 15.00	15.00 15.00	30.00 30.00

unchanged when a cross-classification table is given new marginals is the cross-product ratio or odds-ratio. The cross-product ratio is defined for 2 X 2 tables, or in larger tables for each possible 2 X 2 subtable, and it is computed by dividing the product of the diagonal entries by the product of the off-diagonal entries. For example, in the hypothetical mobility table in Panel A of Table 1, $\alpha = (10)(10)/(5)(5) = 4$. The cross-product ratio can be interpreted as a ratio of odds, hence its alternative name. For example, in Panel A of Table 1, the odds of starting with a high father's occupation and achieving a high rather than low own occupation are 10/5, and the odds of starting with a low father's occupation and achieving a high rather than low own occupation are 5/10. The ratio of these odds is (10)(10)/(5)(5); three other ratios of odds from this table give the same value. The cross-product ratio can take on values from 0 to positive infinity. A cross-product ratio equal to unity indicates independence in a table, and a particular value for a ratio and its reciprocal correspond to the same degree of association in a table but in opposite directions.

The cross-product ratio is invariant under positive, multiplicative marginal effects; that is, the original cell frequencies of a 2 X 2 table can be multiplied by a unique factor for each row and each column and the value of the cross-product ratio will remain unchanged.

For example, the table in Panel B, Table 1, was obtained from the table in Panel A by multiplying the number of people with a high father's occupation (row 1) by 2, with a low father's occupation (row 2) by 4, with a high own occupation (column 1) by .5 and with a low own occupation (column 2) by 10. The value of the cross-product is 4.0 for each table.

The invariance properties of the cross-product ratio make proportional adjustment possible. Since an adequate description of the procedure is not available in the sociological literature, I shall go through an example using Panels A and B of Table 1 (see Mosteller, 1968, for a discussion of adjustment procedures in a variety of problem areas). Since both tables have the same cross-product ratio, the cell frequencies of the two tables will be equal (within .03 of a unit) when B is adjusted to have the marginals of A. To begin adjustment, the frequencies of Table B are converted to either column or row percentages, say row percentages, and then these percentages are multiplied by the row totals of Table A to give a new table with the proper row totals. Panel C shows Table B after the row adjustments; the cross-product ratio is unchanged since the frequencies have simply been multiplied by a factor for each row. The column marginals of the adjusted table are not the same as the target table (A), and so a column operation analogous to the row operation is performed on the adjusted table. The result of four column and four row adjustments is given in Panel D; in most cases, only a few cycles of row and column operations are required to bring the marginals of an adjusted table quite close to those of a target table. The adjustment procedure utilizes the same row and column operations for tables larger than 2 X 2.

If a set of 2 X 2 subtables exhausting the degrees of freedom for interaction is taken from each of two larger tables being compared and the cross-product ratios in comparable subtables are equal, then the two tables will have identical cell frequencies if one table is adjusted to the other's marginals. If comparable cross-product ratios are not equal, the adjusted and target tables will not have identical cell frequencies after adjustment. Unequal cell frequencies between adjusted and target tables can be compared by simply subtracting a cell frequency in one table from the comparable entry in the other table. In this way, excesses and deficiencies in the numbers of men and women with particular combinations of origin and destination statuses can be discovered.

In addition to simply subtracting one cell frequency from another, a method of standardization is available to indicate which differences between frequencies are large compared to other differences. Freeman and Tukey (1950) have developed a statistic which is similar to chi-square in that deviations between the frequencies of equivalent cells in observed and expected tables are calculated, and the squared Freeman-Tukey deviate is distributed similarly to chi-square for many degrees of freedom. In using the Freeman-Tukey deviate here, the table which is adjusted is considered to have the observed values, and the target (unadjusted) table to have the expected frequencies.[6] But unlike chi-square statistics before squaring, positive and negative Freeman-Tukey deviates calculated from tables tend to have symmetric distributions. In the case of chi-square, since expected frequencies in a table can never be negative, a lower limit is placed upon the size of negative chi-square statistics before they are squared. The distribution of unsquared, negative chi-square deviates in a table tends, therefore, to have a smaller variance than the unsquared, positive deviates, and the combined deviates tend not to form a symmetric distribution.

The symmetric properties of negative and positive Freeman-Tukey deviates will be utilized here to standardize the differences between comparable frequencies in men's and women's tables after adjustment and to determine which differences in cell frequencies are large compared to other differences. The symmetry of the Freeman-Tukey deviates allows them to be normally plotted, i.e., points defined by standard normal and Freeman-Tukey deviates which have the same probability of occurring in their respective cumulative distributions are plotted.[7] Those

[6] A Freeman-Tukey deviate is calculated from the following formula where "E" stands for expected value and "O" for observed: $\sqrt{O} + \sqrt{O+1} - \sqrt{4E+1}$. See Bishop (1967) for discussion and illustration of the uses of the Freeman-Tukey statistic in estimating cell values in contingency tables.

[7] Kimball (1960) and Chernoff and Lieberman (1954) provide more details or graphic methods for

Freeman-Tukey deviates which are outlyers to the roughly normal distribution of other deviates will be considered as large. This graphic method, rather than a more usual method using a statistic like chi-square to locate statistically significant differences between cell frequencies, will be used here because of the large sample size in the OCG data. As Blau and Duncan (1967:17) state, "The sample size here . . . is so much larger than in most sociological surveys that it is largely a waste of time to compute significance tests . . . . Indeed, the data show all kinds of 'significant differences' (not due to sampling error) that can be given no clear interpretation and that may be so slight as to be of no practical importance."

Goodman (1969) describes methods for comparing two mobility tables by measurement of the differences in cross-product ratios themselves. If a cross-product ratio from one mobility table is larger than the equivalent cross-product ratio from another table, then there is a stronger association between the status of father and son or father and daughter in the first table than in the second. The direct comparison of cross-product ratios through Goodman's techniques provides then, an alternative methodology that will be used to corroborate the results of the primary methodology of proportional adjustment.

RESULTS

The description of the results begins with the findings for the whole sample of wives and husbands and then continues with those for the eight cohorts. The analysis of the whole sample indicates the broad patterns found in the comparison of men's occupational and women's marriage mobility systems. The women and men of a cohort, on the other hand, have more consistent experiences, although still quite varied, in terms of the availability of marriage partners and jobs than the entire sample of men and women. The analysis of the cohort data will be used, therefore, as a test for the stability of findings in independent subsamples and to check for the presence of historical patterns. For all comparisons of men's and women's data using proportional adjustment, the marginals of the women's tables have been adjusted so that they depart less than a tenth of a unit from the comparable men's marginals.

1. *Whole sample.* Table 2, which gives the outflow percentages and actual marginal frequencies for women and men, indicates the mobility patterns of each sex before their mobility matrices are adjusted. Examination of the main diagonal of the table indicates that for all status categories men are more likely to have the same status as their fathers than are women. The outflow percentages of women, on the other hand, are less clustered around their statuses of origin, and they tend to have larger outflows in statuses both above and below their statuses of origin. Thus, in the unadjusted data men are more likely than women to have the same status as their fathers, 20.1% versus 16.8%, respectively. Women, on the other hand, have more upward and downward mobility—39.9% and 43.3%, respectively—than men, 37.8% and 42.2%. A partial exception to this finding is that upper white-collar men (self-employed professionals through proprietors) have more upward mobility than upper white-collar women.

In spite of these differences in husbands' and wives' outflow matrices, there are strong similarities. For each sex, the percentages having upward and downward mobility are relatively close, and both men and women have slightly more downward than upward mobility (cf. Blau and Duncan, 1967:30ff. for an analysis of mobility to first jobs for all men in OCG). The percentages for both men and women tend to be relatively high in occupational categories at considerable distances from the main diagonal; occupational inheritance is not the predominant form of mobility and medium distance moves are relatively common. This tendency to move considerable distances from the status of origin is in contrast to the mobility of all men in OCG from their fathers' occupations to their own jobs at the time of the survey (cf. Blau and Duncan, 1967:29). The outflow percentages for this latter group of men tend to be highest on the main diagonal and decrease with movement away from it.

In comparing men's intergenerational mobility tables from different historical

determining the forms of distributions. Fienberg (1969) describes a technique in which the cross-product ratios themselves, rather than the Freeman-Tukey deviates between adjusted and target tables, are graphically analyzed.

Table 2. Outflow Percentages and Actual Marginal Frequencies for Occupational Mobility of Husbands with Wives 22-61 Years Old and for Marriage Mobility of Wives 22-61 Years Old.

Father's Occupation	Husband's First Job 1	2	3	4	5	6	7	8	9	10	11	12	Marginal Frequencies
1 Prof., self-emp.	12.4	29.8	2.0	.5	17.4	11.4	6.1	11.9	1.5	5.3	.8	1.0	396
	2.8[a]	19.2	2.8	1.2	16.0	18.2	8.5	16.2	2.2	4.7	1.0	7.3	506
2 Prof., salaried	1.7	31.5	3.8	0.0	13.1	6.7	10.9	17.6	3.8	8.2	1.0	1.7	943
	2.2	21.1	3.2	.4	16.9	8.0	10.9	20.4	1.6	9.2	1.5	4.1	1097
3 Managers	2.2	17.1	2.9	1.1	20.2	10.5	10.5	20.5	2.0	11.0	.7	1.4	1176
	1.7	16.2	2.7	1.3	18.4	9.6	9.4	22.1	1.7	10.4	1.8	4.8	1026
4 Proprietors	2.2	14.7	4.4	4.7	12.2	17.6	9.4	21.1	2.9	8.4	.2	2.2	2241
	2.0	14.3	2.1	1.9	14.6	10.1	11.5	22.6	4.7	9.0	1.7	5.4	1970
5 Clerical	.5	15.3	2.0	.2	22.6	7.2	11.5	26.4	2.4	9.8	.9	1.3	921
	.8	11.6	.9	1.0	18.9	9.5	10.6	21.1	3.7	15.2	1.6	5.1	871
6 Salesmen	1.6	13.2	2.5	1.8	18.5	18.7	6.7	22.6	2.2	8.2	.5	3.7	1201
	1.8	14.6	1.8	2.2	16.5	13.2	9.0	20.9	3.6	9.3	2.1	5.1	1166
7 Craftsmen	.2	6.5	.6	.2	13.5	6.9	18.5	31.6	3.9	13.6	.6	3.9	5443
	.5	6.0	1.5	.3	12.9	7.4	13.7	32.6	3.2	12.6	1.4	7.7	5490
8 Operatives	.3	4.8	.6	.1	11.7	5.2	9.4	46.0	3.4	15.0	.3	3.3	4327
	.2	4.9	1.0	.6	10.3	6.4	10.8	38.6	3.3	14.5	1.3	8.1	4895
9 Service	.3	4.2	1.1	.6	14.1	7.2	12.7	33.7	6.6	15.1	.7	3.7	1249
	1.3	6.0	.3	.8	14.2	6.7	14.6	30.2	4.6	12.8	.9	7.5	1059
10 Laborers	.9	4.0	.4	.1	9.3	5.7	8.4	32.0	4.1	28.1	.9	6.1	1517
	0.0	3.9	.4	.2	9.2	5.4	9.3	29.5	3.2	24.5	1.5	12.9	1694
11 Farmers	.2	3.9	.6	.4	4.5	3.5	6.5	20.5	1.6	11.1	10.6	36.6	7256
	.3	5.2	.5	.5	7.0	5.1	7.7	23.9	2.0	11.8	8.7	27.2	6472
12 Farm Laborers	.3	1.0	.3	0.0	3.8	1.8	7.0	20.9	3.3	12.4	2.0	47.1	603
	.3	1.9	1.7	.7	3.9	2.7	5.3	26.1	2.6	13.6	4.3	36.9	583
Marginal Frequencies	221	2207	346	195	3042	1955	2863	7673	817	3489	888	3577	27273
	198	2096	332	192	3093	1939	2804	7610	804	3449	874	3438	26829

[a]Upper number in a cell is the frequency for husbands, lower number the frequency for wives.

samples, Blau and Duncan (1967:104), following Jackson and Crockett (1964), used Cramér's V as an over-all measure of association between origin and destination statuses. Cramér's V and the product moment correlation coefficient, for those whose intuition is better served by this measure, have been calculated for the men's and women's unadjusted data. Cramér's V is .21 for the men and .13 for the women; r is .46 for the men and .32 for the women. For each measure, the association between origin and destination status is greater for men than for women. These data, like data for upward, downward and equal (to origins) mobility, suggest somewhat more rigidity in the men's than the women's mobility system and that men are more channeled in their mobility by their origins than are women.

Table 3 compares the unadjusted men's and adjusted women's data for the whole sample; the upper number in each cell shows frequencies for men and the lower, the adjusted frequencies for women. These data have been used to calculate the percentages of husbands and wives with upward, downward and equal mobility, and the percentages appear in the first line of Table 4.[8] After the differences in marginal frequencies between the men's and women's tables have been controlled, the same relationships as in the unadjusted data still hold; husbands have more equal mobility and wives have more upward and downward mobility. Out of the approximately 27 million couples represented in the sample (converting to thousands), about 1.2 million more men than women have equal mobility, nearly one million more women have upward mobility and about a quarter of a million more women have downward mobility.

Similar results are obtained when the data from Table 3 are combined to form the major class groupings of white-collar (self-employed professionals through salesmen), blue-collar (craftsmen through laborers) and farm (farmers and farm laborers). The first line of each panel of Table 5 indicates the percent of total men and total women of each origin status having mobility to white-collar, blue-collar and farm destinations. Husbands are more likely to remain in their class of origin while wives are more likely to cross major class boundaries in their mobility.

Through the normal plotting of Freeman-Tukey deviates, eleven cells of the men's mobility table were judged to have significantly larger frequencies than the comparable women's cells, and seventeen cells as having significantly larger frequencies of women.[9] The locations of these cells are given in Table 6. Most of the large positive deviates (indicating larger frequencies for husbands) fall on the main diagonal or near it corroborating the finding that men have more mobility to their fathers' statuses or nearby statuses than women. Almost half of the negative deviates (indicating larger frequencies for wives) fall in the destination status of farm laborers, indicating that women from many origins marry into this status more frequently than men from the same origins take first jobs in this status. The one strong exception to this latter finding is that women whose fathers are farmers tend not to marry farm laborers as frequently as their "brothers" become farm laborers.[10] Other large negative deviations are found in the origin categories of farmers (four deviations), craftsmen (one) and operatives (one); women from these origins have

[8] The methodological difficulties associated with using outflow percentages have been avoided since the outflows here are based upon identical marginal frequencies for men's and women's origin and destination statuses.

[9] The normal plots of Freeman-Tukey deviates for the whole sample and the eight cohorts are available upon request from the author.

[10] In another related method of analysis, the men's and women's unadjusted tables were combined to make a three-way table of father's occupation by husband's first job by sex. Then a table with no three-way interaction, i.e., cross-product ratios in the layers for husbands and wives were equivalent to each other and equal to 1.0, was adjusted to the two-way marginals of the original data tables. The cell values of the assembled three-way data table were then compared to the adjusted three-way table (with no interactions) using the Freeman-Tukey technique. Substantially the same results were obtained as in the analysis reported in the text. A difference measure (a chi-square based upon a sum of squares of Freeman-Tukey deviates) between the three-way data and adjusted tables was considerably reduced by the removal of category 12, farm laborers, but the removal of the diagonal entries as per Goodman's (1965; 1969) use of quasi-independence only decreased the difference measure a moderate amount.

Table 3. Occupational Mobility for Husbands with Wives 22-61 Years Old Compared to Adjusted Marriage Mobility of Wives 22-61 Years Old.

	Husband's First Job												
Father's Occupation	1	2	3	4	5	6	7	8	9	10	11	12	Total
1 Prof., self-emp.	49.0	118.0	8.0	2.0	69.0	45.0	24.0	47.0	6.0	21.0	3.0	4.0	396.0
	11.8[a]	78.3	11.3	4.6	61.3	71.4	33.8	64.1	8.6	18.8	3.7	28.4	396.0
2 Prof., salaried	16.0	297.0	36.0	0.0	124.0	63.0	103.0	166.0	36.0	77.0	9.0	16.0	943.0
	22.2	205.4	31.1	3.3	153.5	74.9	103.4	192.1	15.4	90.1	13.7	37.9	943.0
3 Managers	26.0	201.0	34.0	13.0	238.0	123.0	124.0	241.0	23.0	129.0	8.0	16.0	1176.0
	21.0	196.6	33.3	14.5	209.7	112.7	110.7	260.5	19.4	122.9	19.4	55.3	1176.0
4 Proprietors	49.0	329.0	99.0	105.0	273.0	394.0	211.0	473.0	65.0	188.0	5.0	50.0	2241.0
	49.1	331.4	49.5	40.9	317.1	223.7	259.7	506.5	105.5	202.8	36.3	118.6	2241.0
5 Clerical	5.0	141.0	18.0	2.0	208.0	66.0	106.0	243.0	22.0	90.0	8.0	12.0	921.0
	8.0	110.7	8.8	9.3	169.4	87.4	98.1	195.3	33.8	140.3	13.9	45.9	921.0
6 Salesmen	19.0	158.0	30.0	22.0	222.0	224.0	80.0	271.0	27.0	98.0	6.0	44.0	1201.0
	23.4	181.1	22.5	26.1	191.7	157.8	108.9	251.8	43.2	111.6	23.2	59.9	1201.0
7 Craftsmen	10.0	352.0	34.0	9.0	737.0	376.0	1008.0	1719.0	213.0	739.0	32.0	214.0	5443.0
	29.0	340.0	85.6	18.4	683.3	400.9	752.9	1783.0	175.4	686.2	72.8	415.6	5443.0
8 Operatives	11.0	208.0	25.0	4.0	505.0	223.0	408.0	1990.0	146.0	651.0	13.0	143.0	4327.0
	9.6	221.6	44.1	25.0	432.4	273.8	469.8	1677.6	144.1	630.4	51.6	347.0	4327.0
9 Service	4.0	53.0	14.0	7.0	176.0	90.0	158.0	421.0	82.0	189.0	9.0	46.0	1249.0
	17.9	78.2	3.7	9.2	171.7	83.4	184.3	378.7	57.8	161.1	11.1	91.9	1249.0
10 Laborers	14.0	60.0	6.0	2.0	141.0	86.0	128.0	486.0	62.0	427.0	13.0	92.0	1517.0
	0.0	61.3	6.5	3.5	135.7	81.3	141.9	449.8	49.3	373.7	21.1	192.8	1517.0
11 Farmers	16.0	284.0	40.0	29.0	326.0	254.0	471.0	1490.0	115.0	805.0	770.0	2656.0	7256.0
	26.9	390.6	38.7	36.3	493.0	371.0	566.9	1754.8	149.0	868.6	596.9	1963.1	7255.9
12 Farm Laborers	2.0	6.0	2.0	0.0	23.0	11.0	42.0	126.0	20.0	75.0	12.0	284.0	603.0
	2.3	11.8	10.8	4.1	23.2	16.6	32.5	158.6	15.6	82.5	24.5	220.5	603.0
Total	221.0	2207.0	346.0	195.0	3042.0	1955.0	2863.0	7673.0	817.0	3489.0	888.0	3577.0	27273.0
	221.0	2207.0	346.0	195.0	3042.0	1955.0	2863.0	7673.0	817.0	3489.0	888.0	3577.0	27273.0

NOTE: Frequencies are in thousands.

[a]Upper number in a cell is the frequency for husbands, lower number the frequency for wives.

Table 4. Percent of Men and Women Having Downward, Equal and Upward Mobility

Cohort	Downward		Equal		Upward	
	Men	Women	Men	Women	Men	Women
22-61	42.2	43.1	20.1	15.8	37.8	41.1
22-26	43.2	43.3	20.4	18.7	36.4	38.1
27-31	41.7	42.1	19.4	17.3	38.9	40.6
32-36	40.3	42.2	21.9	14.7	37.8	43.1
37-41	43.4	44.7	18.6	13.6	37.9	41.7
42-46	43.5	44.8	20.3	17.0	36.2	38.2
47-51	40.9	42.2	20.2	15.0	38.9	42.8
52-56	42.5	41.5	18.0	16.4	39.4	42.0
57-61	41.3	43.0	22.6	14.0	36.1	43.0

NOTE: Data are calculated as the percent of total men or women having mobility to downward, equal and upward mobility destinations.

considerably more mobility to various white-collar and blue-collar occupations than their same status brothers. Three negative deviates are found in the destination category of farmers; women from the origins of proprietors, craftsmen and operatives have more mobility than men from the same origins to the status of farmers.

2. *Cohort analysis.* The percentages of women and men with downward, equal and upward mobility in the cohorts are given in Table 4. In the cohorts, as in the total sample, wives have more upward and downward mobility and less equal mobility than do husbands. The only exception to this general pattern is that men in the 22–26 and52–56 cohorts have slightly more upward mobility than women. The percentage spreads between men and women tend to be somewhat larger for equal and upward mobility than for downward mobility.

Table 5 shows the percentages of men and women of white-collar, blue-collar and farm origins having mobility to destinations among these three major status groupings. The findings for the cohorts are again the same as those for the total sample—men have more mobility to positions equal in status to their origins while women pass more easily through the major class boundaries. For the eight cohorts and three origin status groupings, there are only two cases (27–31 and 42–46 cohorts, blue-collar origins to white-collar destinations) where husbands have greater mobility out of their origin class than wives. The greater percentage flows of women from white-collar and blue-collar origins to farm destinations is especially marked.

The locations of significant differences in men's and women's cell frequencies for the eight cohorts are given in Table 7. Women in the cohorts have markedly higher frequencies of mobility than men into the status of farm laborers from almost all occupations except from the origin status of farmers—men have significantly higher frequencies of mobility from farmer origins to farm laborer destinations. Wives have several cases of significantly larger frequencies than men to several mobility destinations from the origins of proprietors and craftsmen. Husbands have a number of instances of markedly larger mobility frequencies from a variety of origins into the destination status of salaried professions and into destinations equal in status to their origins.

3. *Comparison of cross-product ratios.* In Goodman's (1969) method of comparing cross-product ratios between two mobility tables, standardized differences between the ratios are found by transforming them to logarithms, subtracting one from another and then dividing by the square root of a quantity estimating the combined variance of the two cross-product ratios. The standardized difference can be considered as a standard normal

Table 5. Percent of Men and Women, by Origin Status, Having Mobility to White-Collar, Blue-Collar, and Farm Destinations.

Cohort	White-Collar		Blue-Collar		Farm	
	Men	Women	Men	Women	Men	Women
A. White-Collar Origins						
22-61	55.6	48.3	41.7	45.0	2.6	6.6
22-26	53.9	47.2	43.5	49.1	2.6	3.8
27-31	54.0	50.5	43.1	45.1	2.9	4.5
32-36	59.3	50.1	38.7	43.8	2.0	6.1
37-41	52.7	43.1	45.0	50.1	2.3	6.9
42-46	50.4	42.9	45.7	50.5	3.9	6.6
47-51	61.8	49.8	36.7	42.9	1.6	7.2
52-56	54.4	54.1	43.3	36.6	2.3	9.2
57-61	63.1	51.8	32.3	32.3	4.6	15.9
B. Blue-Collar Origins						
22-61	25.1	25.7	70.4	64.7	4.5	9.6
22-26	21.5	22.0	73.0	68.8	5.4	9.1
27-31	25.2	24.6	70.8	67.9	4.0	7.5
32-36	24.7	27.8	71.9	64.5	3.3	7.7
37-41	23.4	24.6	71.6	63.5	5.0	12.0
42-46	25.5	25.0	70.0	63.8	4.6	11.2
47-51	27.7	29.7	67.8	60.3	4.5	10.0
52-56	30.2	27.3	64.5	64.4	5.3	8.3
57-61	26.3	27.6	70.1	61.0	3.6	11.4
C. Farm Origins						
22-61	12.6	18.1	40.0	46.2	47.4	35.7
22-26	16.5	24.3	42.1	46.2	41.4	29.4
27-31	13.3	18.7	45.8	49.2	40.9	32.1
32-36	14.5	18.8	42.1	50.6	43.4	30.6
37-41	12.3	18.5	39.6	48.6	48.2	32.9
42-46	11.0	17.4	38.7	43.7	50.3	39.0
47-51	12.5	18.3	39.8	44.7	47.7	37.0
52-56	11.7	15.1	37.7	41.6	50.6	43.4
57-61	10.3	15.6	33.3	43.2	56.4	41.2

NOTE: Data are calculated as the percent of total men and women in each origin state having mobility to white-collar, blue-collar, and farm destinations.

difference score, but following the warning given above concerning the large sample size of OCG, no emphasis will be placed on statistically significant differences.

Some of Goodman's measures involve laborious computations in large tables, so the original 12 X 12 mobility tables have been collapsed to form 3 X 3 tables based upon the major class divisions of white-collar, blue-collar and farm. Results from the comparison of cross-product ratios between men's and women's mobility tables from the whole sample and the cohorts appear in Table 8. All the differences in the table were formed by subtracting women's scores from men's scores, thus positive differences indicate greater associations between the statuses of fathers and sons and negative scores, greater associations between the statuses of fathers and daughters.

The first two rows of Table 8 indicate the signs of the standardized differences found by comparing a set (not unique) of four (one for each degree of freedom allocated to interaction effects) 2 X 2 subtables drawn from the whole sample tables for husbands and wives and from each of the cohort tables. In the whole sample, all four differences are positive demonstrating greater association between the statuses of fathers and sons than between the statuses of fathers and daughters. Some negative differences are found in the cohorts, but on the whole there is stronger association between the statuses of men and their fathers than between the statuses of women and their fathers.

Rows 3 through 9 give the values of differences in various inheritance effects (see Goodman, 1969, for a detailed description of the meaning and derivation of these measures). For nearly all the inheritance measures, men have a greater tendency to inherit their fathers' statuses—have mobility to statuses equal to their fathers' statuses—than women.

Table 6. Husbands' Occupational and Wives' Marriage Mobility. Locations of Large Freeman-Tukey Deviates for the Whole Sample.

	Husband's First Job												
Father's Occupation	1	2	3	4	5	6	7	8	9	10	11	12	Total
1 Prof., self-emp.	P											N	1N 1P
2 Prof., salaried		P											0N 1P
3 Managers												N	1N 0P
4 Proprietors			P	P		P					N	N	2N 3P
5 Clerical												N	1N 0P
6 Salesmen						P							0N 1P
7 Craftsmen			N				P				N	N	3N 1P
8 Operatives				N				P			N	N	3N 1P
9 Service												N	1N 0P
10 Laborers	P											N	1N 1P
11 Farmers		N			N	N		N			P	P	4N 2P
12 Farm Laborers													0N 0P
Total	0N 2P	1N 1P	1N 1P	1N 1P	1N 0P	1N 2P	0N 1P	1N 1P	0N 0P	0N 0P	3N 1P	8N 1P	17N 11P

NOTE: A negative deviation is signified by an "N" and a positive deviation by a "P." Negative deviations indicate much larger frequencies of women, and positive deviations much larger frequencies of men.

Table 7. Husbands' Occupational and Wives' Marriage Mobility. Locations of Large Freeman-Tukey Deviates for the Eight Cohorts.

Father's Occupation	Husband's First Job 1	2	3	4	5	6	7	8	9	10	11	12	Total
1 Prof., self-emp.			1N			1N							2N
	1P	1P				1P							3P
2 Prof., salaried													0N
		1P							1P				2P
3 Managers													0N
		1P	1P										2P
4 Proprietors	1N				1N		1N		2N		1N	3N	9N
	1P		1P	1P	1P	1P							5P
5 Clerical						1N	1N			1N		1N	4N
			1P			1P			1P				3P
6 Salesmen				1N					1N	1N		2N	5N
		1P			1P	2P							5P
7 Craftsmen	2N		1N								1N	4N	8N
		1P					3P		1P		1P		6P
8 Operatives						1N						4N	5N
					1P			2P	1P				4P
9 Service					1N							1N	2N
		1P	1P		1P			1P	1P	1P			6P
10 Laborers												2N	2N
	1P	1P						1P					3P
11 Farmers					2N	1N		1N					4N
												6P	6P
12 Farm Laborers			1N										1N
							1P					1P	2P
Total	3N	0N	3N	1N	4N	4N	2N	1N	3N	2N	2N	17N	42N
	3P	7P	4P	2P	4P	5P	4P	4P	5P	1P	1P	7P	47P

NOTE: A negative deviation is signified by an "N" and a positive deviation by a "P." Negative deviations indicate much larger frequencies of women, and positive deviations much larger frequencies of men.

Table 8. Standardized Values of Difference Between Odds-Ratios in Men's and Women's Mobility Tables.

Standardized Values	Age Group								
	22-61	22-26	27-31	32-36	37-41	42-46	47-51	52-56	57-61
1. Positive Differences	4	3	3	3	3	3	4	3	2
2. Negative Differences	0	1	1	1	1	1	0	1	2
3. Status Inheritance of White-Collar Status	11.5	2.6	2.1	5.3	5.5	3.5	6.1	2.9	3.3
4. Status Inheritance of Blue-Collar Status	8.0	2.9	1.9	4.2	4.6	4.0	2.2	-1.4	3.6
5. Status Inheritance of Farm Status	22.4	4.7	6.1	9.1	10.5	8.5	8.6	6.4	8.1
6. Intrinsic Status Inheritance of White-Collar Status	5.7	1.2	0.5	2.7	2.8	1.2	4.1	1.8	1.0
7. Intrinsic Status Inheritance of Blue-Collar Status	-1.1	0.6	-0.3	0.2	0.0	0.7	-1.5	-3.2	0.8
8. Intrinsic Status Inheritance of Farm Status	20.7	4.2	5.9	8.2	9.6	7.8	7.8	6.5	7.3
9. Intrinsic Net Status Inheritance of All Statuses	23.2	5.5	5.7	10.2	11.3	8.6	9.7	4.8	8.1
10. Fathers and Subjects with Different Statuses	-0.9	-1.7	-1.4	0.7	-0.2	-2.9	0.8	2.0	-0.3

NOTE: Positive values indicate larger effects for men, and negative values, larger effects for women.

In a few cases, women do have larger inheritance measures for the blue-collar status. In each set of three inheritance measures (rows 3–5 and 6–8), the scores for the inheritance of farm status are largest, thus corroborating the finding from above that in the cohorts and the whole sample, men of farm origins take farm jobs much more frequently than farmers' daughters marry farmers. Row 9 is an over-all measure of inheritance, and the positive values indicate that men have more mobility to statuses equal to their origins than women. Row 10 gives a measure of interaction that is not directly affected by the number of husbands and wives in the same status categories as their fathers; the measure is based upon all cells which indicate both upward and downward mobility. In the whole sample and over half of the cohorts, this measure is larger for wives, thus indicating larger effects for fathers and daughters with different statuses than for fathers and sons with different statuses. This finding is in agreement with the finding from the analysis above (using table adjustment) that women have more upward and downward mobility than men.

4. *Historical trends.* Jackson and Crockett (1964) and Blau and Duncan (1967) computed a variety of summary measures to check for the presence of historical trends in men's mobility tables. Those mobility measures have been calculated for the women's and men's whole samples and the cohorts; the results appear in Table 9. The measures were computed from the unadjusted mobility tables for each sex, and so, to be comparable to the calculations in Blau and Duncan (1967:103 ff.), occupations were combined to form the three major groups of white-collar, blue-collar and farm. The same calculations were done using mobility tables with twelve occupational categories, and essentially the same results as with the three major occupational groups were found.

The first line of Table 9 gives the percents of men and women having mobility out of their status of origin—the sum of those having upward and downward mobility. In the whole sample and in each of the cohorts, wives have more gross mobility than husbands. Therefore, whether marginal differences are controlled or not, women have more mobility through marriages than men do through occupations. There are no monotonic trends in the percent of observed mobility across the cohorts for either sex; for each sex the percents rise and fall several times through the span of the cohorts.

The minimum proportion of mobility necessary to produce the intergenerational change in marginal distributions is given in row 2 of Table 9. In every cohort, the percent of minimum movement is lower for women. Again there are no monotonic trends. If we subtract the minimum percent of mobility from the percent actually observed, we obtain a measure of the excess of observed mobility over that necessitated by changes in marginal distribution. That measure was termed "circulation" by Jackson and Crockett and it appears in row 3 of Table 9. In all cohorts, the percent circulation is higher for wives than husbands; this is in spite of the fact that the percent of structural movement is lower in all cohorts for wives. In other words, the actual mobility of women exceeds that predicted by structural changes to a greater extent than does men's mobility. Once again monotonic trends are absent.

The amount of mobility expected, if a husband's or wife's status were independent of father's status, is given in row 4. Women, by the hypothesis of independence, are expected to have smaller proportions of mobility in all cohorts but one—the percents are equal in the 57–61 cohort. In all cohorts, the percent of observed mobility for women is closer to the percent expected under independence than is the case for men. For both men and women, the percent of expected mobility under independence rises from the younger to the older cohorts.

The level of association between father's and respondent's status is given by Cramér's V and appears in the last line of the table. In all cohorts, there is more association between the status of fathers and married sons than between the status of fathers and married daughters. There are no clear trends, but older cohorts show somewhat more association between father's and respondent's status than younger cohorts—perhaps an indication of a slightly decreasing rigidity in the class structure.

The summary measures on the unadjusted mobility tables reflect the findings using adjusted tables: women have more mobility from their status of origin, and there is less association between the status of father and

Table 9. Summary Measures of Mobility for Men's Occupational and Women's Marriage Mobility Tables, Whole Samples and Eight Cohorts, Three Major Occupational Groups

Measure of Mobility	Cohort								
	21-61	22-26	27-31	32-36	37-41	42-46	47-51	52-56	57-61
Percent Mobile									
Observed	40.0[a]	38.1	41.2	38.7	39.7	41.0	40.5	42.8	36.6
	46.8	43.6	44.2	46.9	48.9	48.1	49.0	44.7	48.2
Minimum (structural movement)	12.4	7.7	11.4	12.1	12.0	12.8	15.5	15.9	13.8
	10.2	5.5	10.4	10.0	9.7	9.9	13.1	11.7	13.5
Observed minus minimum (circulation)	27.5	30.4	29.8	26.6	27.8	28.3	25.0	26.9	22.8
	36.5	38.1	33.8	36.8	39.1	38.2	35.9	33.0	34.7
Expected, hypothesis of independence (full equality model)	62.9	57.8	61.4	61.8	62.3	63.6	65.0	65.2	65.5
	61.8	52.2	59.5	60.7	60.3	62.7	64.0	64.7	65.5
Contingency coefficient (Cramer's V)	.429	.386	.396	.436	.425	.424	.443	.419	.490
	.279	.262	.286	.265	.225	.275	.279	.346	.300

[a]Upper data in each row are for men, lower for women

daughter than between the status of father and son. The analysis using the summary measures gives little evidence of either increasing flexibility or rigidity in the marriage mobility of women or the occupational mobility of men. The one minor exception to this finding was the slight decrease in Cramér's V for younger cohorts. The discovery of relatively similar mobility patterns across cohorts for each sex (taken separately) is in agreement with Blau and Duncan's (1967:107ff.) finding of stable mobility patterns across cohorts for all men (married and unmarried) in OCG from their father's occupation to their own first jobs.

DISCUSSION

The clearest and most present finding in the analysis here is that men more frequently "inherit" their origin statuses through their first jobs while women more frequently move both upward and downward through their marriages. This finding appears in both the cohorts and the whole samples, in the raw as well as the adjusted data, and regardless of which technique of analysis is used. Further, women more frequently cross class boundaries when the data are arranged in the twelve broad status categories as well as when the three major class groupings of white-collar, blue-collar and farm are used. The combined upward and downward mobility of wives is greater than that of husbands despite the fact that smaller percentages of mobility are necessary to produce the intergenerational change in marginal distributions for wives. The percent of mobility for women more nearly approximates that predicted by the hypothesis of independence.

It is ironic that several of the hypotheses in Glenn et al. (1974) are better supported here than in their article. Glenn et al. hypothesized that women would have more upward and downward mobility and men would have greater status inheritance. Their expectation concerning downward mobility was supported in their study—greater percentages of women were downwardly mobile in all four surveys—but their results concerning upward and equal mobility were mixed. In three of four surveys *men*, rather than women, had larger percentages of upward mobility, and in two out of four surveys men had larger percentages of equal mobility. Glenn et al. (1974:693ff.) discuss several possible sources of bias in their study which could erroneously lead to the conclusion that men, rather than women, have more upward mobility. In addition to the factors that they suggest, the finding of consistently greater upward mobility for women here could arise from sampling inaccuracies between the data in OCG and the surveys used by Glenn et al. or from differences in the sample universes of the two studies. Here, only currently married (in 1962) couples are included and destination status is given by the husband's first job. In Glenn et al., while currently married women made up the female sample, men with jobs, married and unmarried, made up the male sample, and destination status was measured by the occupation of a man or a wife's husband at the time of a survey. If this difference in findings is not artifactual, then it would be an interesting topic for further research to see how it arises. One possibility would be that upwardly mobile wives marry men who skid in status from their first jobs, and another would be that men who first marry upwardly mobile wives later divorce them to marry women of their (the men's) current status.

Tyree and Treas (1974) were not primarily interested in the differences between men's occupational and women's marriage mobility, but rather whether these differences were larger or smaller than those between men's and women's occupational mobility. However, a comparison between their adjusted frequencies for men and women (their Tables 1 and 3, respectively, pages 296 and 298) indicates that men tend to have higher frequencies of equal mobility while women tend to have somewhat greater amounts of upward and downward mobility. This corroboration is not too surprising since Tyree and Treas used methods similar to those used here on related samples—all men and married women not in the labor force in OCG.

The consistent though moderate amount of greater fluidity in women's mobility is indicated by the larger proportions of women that are able to pass through the major class boundaries among white-collar, blue-collar and farm statuses. The somewhat greater rigidity in the men's mobility system is supported by both the analysis showing percentages of each sex moving among the three major status groupings and that using

the Goodman inheritance effects. While men tend to inherit all three statuses to a greater extent than women, this result is most clearly marked in the white-collar and farm cases. As in the case of upward mobility, the results of Glenn et al. (1974) are mixed concerning which sex has greater mobility across major class boundaries. In all four surveys, greater proportions of wives from nonmanual origins were mobile to manual and farm destinations, but in two out of four surveys, manual and farm men had greater mobility to nonmanual destinations.

In a result related to greater inheritance effects for men, the analysis here indicates that the association between the statuses of fathers and married sons is greater than that between fathers and married daughters. This finding also shows consistency; it appears in both the whole samples and the cohorts through the use of Cramér's V and Goodman's method for comparing cross-product ratios. Glenn et al. (1974) provide corroboration for this finding—in all surveys the correlation between the status scores of men and their fathers was higher than that between the scores of women and their fathers.

The use of Freeman-Tukey deviates indicates that especially large standardized differences between husbands' and wives' frequencies are found for a variety of statuses. First, in the whole samples and to a lesser extent in the cohorts, men have significantly larger frequencies to occupations equal in status to their origins. The second and most consistent finding using the Freeman-Tukey deviates is that women from nearly all origin statuses have significantly greater mobility into the status of farm laborers than their same status brothers. The one major exception is the origin status of farmers—farmers' sons become farm laborers much more frequently than farmers' daughters marry farm laborers.

This finding may not be as aberrant as it seems at first glance. The Census included farm managers in the same occupational category with farm laborers, and so managers of large farms are placed in the same class as seasonal farm workers. Also, many of the men classed as farm laborers may be the sons of farmers, presumably some on large, profitable farms, who will in time take over the family farm. Therefore, it is quite possible that many of the women from higher origins who marry "farm laborers" are actually marrying farm managers or men who will take over large farms. Since men of this sort probably enjoy considerable material success, the marriage of women of high origins to these "farm laborers," if such marriages do indeed occur, would not be so surprising. Farmers' sons classified as farm laborers while waiting to take over the family farm could account for the significantly greater frequencies of farmers' sons becoming farm laborers. Farmers' daughters, on the other hand, are not limited by this kind of inheritance and are freer to distribute themselves in marriage to a variety of statuses. Large standardized differences between men's and women's frequencies are found in other statuses, but the findings concerning men's status inheritance and women's marriage to farm laborers are the strongest and most consistent patterns.

The analysis for historical trends indicates the absence of monotonic patterns across the cohorts of each sex. A partial exception to this finding is the slight decrease in Cramér's V for younger cohorts. Although 40 years of wife's age are covered by the cohorts, there is little evidence of either increasing fluidity or rigidity in mobility as one moves through the cohorts of either sex. In their analysis of mobility from origins to first jobs for all men in OCG, Blau and Duncan (1967:107ff.) similarly found an absence of historical trends. However, as they warn, it is likely that any real changes in mobility patterns are of a smaller order of magnitude than the errors in the data analyzed.

CONCLUSION

The aim of this study has been to indicate the differences between the main forms of intergenerational mobility for each sex in the United States—occupational mobility for men and mobility through marriage for women. The major results here are that women are more mobile, both upward and downward, through marriage than men are through occupations and that the association between fathers' and subjects' statuses is greater for men—they are more likely to inherit their fathers' statuses—than for women. Although these differences are consistently found through a variety of methodological techniques in both the whole samples and the cohorts, men's and women's mobility systems

are in some ways relatively similar. For example, some statuses have similar patterns of recruitment in both men's and women's tables, and the spreads between the percents of women and men with upward, downward and equal mobility are not very large. The differences between women's mobility through marriage and men's through occupations are small when compared to those between women's and men's occupational mobility. Tyree and Treas (1974:298) report that the indexes of dissimilarity for origins and destinations are about twice as great for men's and women's intergenerational occupational mobility (all men and women in OCG in the labor force) as for men's intergenerational occupational mobility and women's intergenerational marriage mobility (wives in OCG not in the labor force).

It is both difficult and speculative to suggest possible explanations of the differences in men's and women's mobility discovered here. On one hand, there are enough contradictory findings and unsupported beliefs in the sociological literature concerning occupations and marriage that many, including some opposing findings, could be supported *post hoc*. On the other hand, there is little work which attempts to explain how the concrete decision and social structural processes behind the selection of mates and jobs lead to broad patterns of mobility—such as those described here. With these words of warning, I shall suggest several factors which I think might be important for explaining the differences in men's and women's mobility discovered here. I shall concentrate on the main finding of greater assortment in origin and destination statuses for women's marriage mobility than for men's occupational mobility.

Research has indicated that married couples are significantly alike in terms of a whole constellation of physical features—features as surprising as space between the eyes and length of ear lobe as well as more expected features like color of eyes and hair and general body build (e.g., see Spuhler, 1968 and the literature reviewed there). It seems reasonable to expect that these physical features have, on the average, either no or relatively low correlations with origin status—features such as height may be an exception. To the extent that physical features are important in mate selection and they have little correlation with origin status, we would expect a relatively large amount of status heterogeneity in marriage partners. In other words, a woman's origin status would be a relatively poor predictor of her husband's status. In contrast, things like educational level, vocational or professional training and "contacts" are probably much more important than physical features in determining the status of a man's first job. To the extent that factors of the latter type are important in the selection of men for jobs and the factors are highly correlated with origin status, we would expect a relatively small amount of heterogeneity in the statuses of men's origins and their first jobs.

In a related point, the skills and training which are required for many high status first jobs are most easily acquired by men from high status origins. For example, occupations like those of doctor, lawyer, research scientist and business executive usually require long years of expensive training, and the expense is more easily afforded by families of relatively high status. Some of the endowments and skills such as beauty, good grooming and "charm" which make women desirable to potential husbands of high status are found in women of low origin status. Such skills and endowments are probably more easily gained (considering that it takes at least some minimal skill to enhance natural endowments) by women of low origins than the skills and training that would allow the brothers of these women access to high status jobs. In partial support of this hypothesis, good grooming and physical attractiveness have been found to be predictive of upward mobility through marriage in women (Elder, 1969).

To the extent that the skills required for upward mobility can be more easily gained independently of family origins by women than by men, we would expect greater amounts of upward mobility by low status women than low status men. Competition for high status husbands with women from low status origins would force some high status women to marry down if they wanted to marry at all. While some women of high origins do marry down, as has been shown here, others may elect not to marry rather than marry down (see Glenn et al. for a discussion of rates of nonmarriage by sex for different classes).

Finally, some occupations are virtually inherited by sons from their fathers—for example, a son can take over the family farm, his father's business or he can gain a place in certain construction unions more easily if his father is a member of the same union. In fact, the three occupations showing the highest measures of self-recruitment for first jobs for men in OCG are farmers, proprietors and craftsmen in construction (Blau and Duncan, 1967:53). The lack of a mechanism for the direct inheritance of an origin occupation would leave women freer to marry into statuses both above and below their origins.

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PARTY CLASS IMAGES AND THE CLASS VOTE IN CANADA*

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Canada stands out sharply as a country in which the ralationship of social class to electoral politics appears to be almost non-existent. The class vote in Canada is re-examined using a new measure which takes into account voter perceptions of the class positions of the political parties. The results indicate that voter interest in class issues is greater than previously thought. This, in turn, suggests that the main source of the anomaly associated with the Canadian case resides, not with the Canadians themselves, but with the nature of the electoral options presented to them. This finding suggests that one cannot assume that the politics of a democracy faithfully reflect the salient concerns of its citizens. The results also suggest that conventional measures of voting behaviour, which normally fail to take into account the variable nature of electoral options, provide a poor indicator of the nature of mass sentiment.

INTRODUCTION

In the spectrum formed by industrialized Western democracies, Canada stands out sharply as a country in which the relationship of social class to electoral politics appears to be almost non-existent (Alford, 1963a). Lenski and Lenski (1974:356) found, in a survey of nine countries, that the association of social class with the vote, as measured by Alford's Index of Class Voting (See Alford, 1963a), varied from a high of +58 in Norway to a low of +7 in Canada. In a survey of seventeen countries, Rose and Urwin (1971:220) found only three without a nation-wide class party—the United States, Ireland and Canada. Furthermore, experts who compare the United States with Canada argue that political parties in the United States are much more clearly associated with class than those in Canada (e.g., Scarrow, 1965).

Canadian politics thus provide an outstanding exception to the usual generalizations about the role of social class in the politics of industrialized democracies. The dominant explanation of the Canadian case has reflected the widespread assumption that the politics of a democracy are strongly influenced by the wishes of its citizens and has emphasized the nature of class sentiment within the general population. Generally speaking, it has been assumed that, for whatever reasons, Canadian voters do not care about class issues. Alford (1963a:257), noting the classless image of Canadian political parties, assumes that this fact faithfully reflects the opinions of the electorate. "Neither class nor national identities are well developed, and the major diffuse loyalties or attachments of people are to regional and religious loyalties." Englemann and Schwartz (1967:58), in another authoritative work, agree with this interpretation:

> His interpretation, compatible with our own view of Canadian society, is that regional-ethnic and regional-economic interests and loyalties are so strong, that even in the case of economic interests, they work against the emergence of national class-oriented behaviour.

Similar points of view are expressed by a number of other authorities (Beck, 1968:420; McLeod, 1966:335; Smith, 1967:192; Fox, 1966:337; Meisel, 1972:60).

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More recently however, Alford (1967) has revised his earlier view and has argued that explanation of the low class vote in Canada has more to do with the Canadian party system than with the motivations of the Canadians themselves. Similarly, Schwartz (1974:589) has argued that:

> . . . class-based voting exists; it is consistent class-based parties that are missing.

This paper outlines a measure of the class vote which can take the nature of voter perceptions of electoral options into account. This measure permits inferences as to the nature of voter motivation which are more plausible than those drawn from conventional measures. The results of this measure will, in turn, allow us to make comments on these contrary interpretations of the Canadian case.

DATA

The data used in this paper came from John Meisel's national survey of the 1965 Canadian federal election.[1] It contains a battery of semantic differential questions on party images which run on a scale from 1 to 7. One of these items provides a measure of the class images of the Canadian political parties. It asks respondents to rate parties on a scale from 1 "for the working class" to 7 "for the middle class." This provides a measure of voter perception of the class positions of the political parties.[2]

Measurement of Class Voting with the Actual Party Image Variable

The standard method of estimating the rate of voting on the basis of social class has been to cross-tabulate the social class position of the respondents with the social class position of the political parties. Class of respondent is usually measured by occupation or respondent class self-image. The class position of the parties is assigned by academic opinion. Partisanship is measured by the vote or by partisan identification. The rate itself is calculated either with tau beta (as is done in the paper), or with Alford's Index of Class Voting which is based on the index of dissimilarity (see Alford, 1963a). The two methods yield essentially the same figure when the marginals of the fourfold table are not extremely skewed—the usual case for estimates of the class vote.

Little attention is usually paid to the placement of the political parties. To begin with, it tends to be assumed that the political elites of a polity will present a meaningful choice on issues of importance to the population. This is not necessarily the case. For example, Butler and Stokes (1969) found that the two major political parties did not present a choice on the issue most salient to the British population at the time—coloured immigration. Similarly, Hamilton (1972) found that majority desires on class issues in the United States have been largely ignored by the major parties. As Campbell et al. (1960:364) point out, it is difficult to vote on the basis of an issue if the political parties do not differ on that issue. This may not prevent people from casting votes on the basis of a perceived difference which does not exist. However, it would seem likely to reduce the proportion of the people who attempt to do so. Additionally, the less pronounced the choices provided on the issue, the greater is the likelihood of dissensus among the general population as to the positions which the parties take. This can easily lead to a situation in which two voters with identical motivations vote for two different parties because their perception of the positions of the parties differ. Conventional measurements do not take this into account. Consequently, they may easily underestimate the degree of voter interest in a given issue in political units where a clear choice on an issue is not provided by the parties.

A second, and related assumption, is that

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[1] Data were gathered from a modified cluster sample of 2113 Canadian voters selected randomly from voting lists. The data were subsequently weighted to the point where the weighted N is 2718. The response rate was only 62.8%. For further information, see Meisel and Van Loon (1966).

[2] If a consensus methodology for the class positioning of political parties is developing, it would appear to be in the nature of voter perceptions. Barnes and Pierce (1971:646) argue that:

> This method of ordering the parties transfers the subjectivity involved in the measurement process from the researchers to the population involved. And that measurement technique permits us to report statistics at the ordinal level. . . .

This consensus has been encouraged by the fact that voter perceptions have tended to correspond reasonably well with the opinions of expert observers. (For the United Kingdom, see Butler and Stokes, 1969; Norway, Converse and Valen, 1971; France, Converse and Pierce, 1970; Italy, Barnes and Pierce, 1971.)

Table 1. The Class Images of Canadian Political Parties, 1965

	NDP	Creditistes	Social Credit	Conservatives	Liberals
Canada	2.7	3.2	3.4	4.4	4.4

The numbers presented are mean scores. The higher the score, the more "for the middle class" the image on a scale from 1 to 7. The midpoint on the scale is, of course, 4.

the expert assessment of the scholar as to the position of parties will be shared by the general population. Again, this is not necessarily the case. Even in a highly class polarized society, such as the United Kingdom, there are some people who do not share expert evaluations. Butler and Stokes (1969:89) report that: "Fully 90 percent of our respondents placed the Conservatives toward the middle class end of the scale and 83 percent put Labour toward the working class end." In societies such as the United States, there is considerable dissensus between academic authorities and portions of the general population on the class positioning of the political parties. For example, it would appear that from twenty-five to thirty-three percent of the American population see little difference between their major political parties (Alford, 1967:79), and hence would not agree with the designation—conventional in calculations of the class vote—of the Democrats as "working class" and the Republicans as "middle class."

This means that many people attempting to cast a class consistent vote may not vote for the party which the social scientist thinks they should vote for. For example, a working class Canadian might remember that former Prime Minister John Diefenbaker raised the old age pension and decide to vote Conservative. This class-motivated vote lowers the level of the class vote as it is conventionally measured. Consequently, it will usually be interpreted as a sign that the voters are not motivated to vote on the class issue. In sum, the greater the dissensus on party positions in any given society, either between academics and voters or within the voting population itself, the greater the degree to which conventional measures will underestimate the degree of mass sentiment on that issue.

Such dissensus does exist to an unusual degree in Canada. The standard classification of Canadian political parties for purposes of measurement of the class vote has been that of Alford (1963a:13-4) who bases his classification on the opinion of a leading Canadian authority, Dawson (1954:501). For purposes of the calculation of class voting, he usually lumped the Liberals and New Democratic Party together as "Left" and the Progressive Conservative and Social Credit Parties together as "Right."

However, the conventional wisdom of academics as to the class positions of the parties is very definitely not shared by the general population. (See Table 1.) In distinct contrast to the Alford/Dawson classification, the aggregate view of the citizens places the two major parties, the Liberals and the Progressive Conservatives, together as "middle class" and the minor third parties together as "working class."[3]

Similar dissensus exists within the population itself. For example, a full forty-one percent of the citizenry with an opinion views the two major parties as taking the same position on the class issue. Another twenty-nine percent sees the Conservatives as being more middle class than the Liberals, while yet another twenty-nine percent sees the Conservatives as being more working class than the Liberals.

In sum, dissensus on party class positions exists between the academics and the voters, and within the voting population itself. Furthermore, it appears that the two major parties do not, in fact, present a meaningful

[3] This perception of similarity of major party class positions by the Canadian citizenry is not typical of mass populations. Citizens in other countries perceive clear differences in the class positions of the major political parties. (For example, on the United Kingdom, see Butler and Stokes, 1969; on Norway, see Converse and Valen, 1971.)

choice on class issues to the population (Ogmundson, 1975a). If the arguments previously outlined have been correct, conventional measures have likely underestimated the degree of intended class voting by the Canadian citizenry. Consequently, they have likely also led to an underestimation of the degree of interest in the class issue.

One gets a better idea what the voter thinks he is voting for if one allows the voters to assign the class position of the parties. This allows a more plausible inference as to their motivation. To begin with, the effects of dissensus between academics and the general population on the positioning of the parties can be removed by assigning the party position in accordance with the aggregate means provided by voter perceptions. The effects of dissensus within the population can be removed by allowing the individual voter to assign the class position of the party he votes for. This is fairly simple in those cases where individuals rated a party as "for the working class" at 1 to 3 on the scale of 7, or as "for the middle class" at 5 to 7 on the scale of 7. A problem arises with the classification of a response of "4" on the scale. One might argue that a "4" response is one which favours no change, which is consequently for the *status quo* and continuation of the relatively privileged position of the middle classes, and call the "4" a middle class response. Indeed, the aggregate preference of the middle classes is for an Ideal Party with a position of 4. On the other hand, one might throw out the "4's" leaving only those who voted for a party perceived as 1 to 3 as voting for a working class party, and leaving those who voted for a party perceived as 5 to 7 as voting for a middle class party. This method loses some data while making no assumptions about the 4's. Both classifications are used. The results are similar in either case.[4]

FINDINGS AND DISCUSSION

The results are presented in Table 2. Four measures of social class are used so as to increase confidence in the findings.[5] The four classifications of the political parties are those discussed in the previous section. The first classification is the conventional Alford/Dawson one which considers the Social Credit, Creditiste and Progressive Conservative parties as "middle class" and the Liberal and New Democratic parties as "working class." When this classification is used, an unusual pattern of negative rates of class voting appears. The second classification assigns political party class position according to the aggregate perceptions of the population—the Liberals and Progressive Conservatives are both viewed as "middle class" while the other parties are viewed as "working class." This classification removes the effects of disagreement between the perceptions of academics and the general citizenry. In this case, very moderate, but positive rates of class voting appear. Finally, the third and fourth classifications allow the individual voters to classify the party they voted for according to their own perceptions. This removes the effects of the substantial within-population dissensus concerning the positioning of the parties and allows us to classify similarly motivated votes for different parties as being class consistent. When this is done, the class vote increases again.[6]

[4] Very similar findings also emerge when different measures of partisanship (1963 vote, partisan identification) are used. (See Ogmundson, 1972:Ch. 6.)

[5] Since the main purpose of this research was to explore the political party variable, the measures of social class were dichotomized in the manner customary to studies of the class vote. Professional, executive, sales, clerical and other white collar occupations were classified as middle class. Those with twelve or more years of education were considered middle class. Those with an income of more than $6,000 were classified as middle class. (The median income in 1965 was approximately $5,200.) Those who identified themselves as upper class, upper middle class or middle class were also considered middle class.

[6] It has been pointed out that these increases could be partially attributed to cognitive consistency factors. I believe the role of cognitive consistency to be minimal and the criticism unimportant. To begin with, one can point out that cognitive consistency on this topic would be an indication of the class consciousness we are attempting to measure. However, the construction of the questionnaire is such that only the most alert and class conscious respondent could be expected to match the variables of party class image, the vote and class background. The party class image questions are part of a complicated battery of semantic differential questions in the first half of a long questionnaire. They elicit opinion of six different parties—Liberal, Conservative, NDP, Social Credit, Creditiste and Ideal—on no less than twelve different dimensions—out of

Table 2. The Canadian National Class Vote in 1965 As Measured by Tau Beta with Different Measures of Respondent's Social Class and Different Classifications of the Political Parties

Measure of Social Class	Classification of Political Parties			
	ALFORD (a)	NATIONAL (b)	IND'L-I (c)	IND'L-II (d)
Income	-.09 (2106)	.03 (2106)	.11 (1996)	.125 (1212)
Occupation	-.03 (1863)	.08 (1863)	.11 (1786)	.13 (1093)
Education	-.01 (2171)	.07 (2171)	.12 (2055)	.18 (1246)
Subjective Class	-.06 (2088)	.10 (2088)	.18 (1992)	.21 (1213)

(a) ALFORD Classification - The Liberals and New Democratic Party are classified as "working class" or Left and the Progressive Conservative Party, Social Credit Party and Creditiste Party are classified as "middle class" or Right.

(b) NATIONAL Perceptions Classification - Parties are classified on the basis of national means of perception of the Canadian population. The Liberal and Progressive Conservative Parties are seen as "middle class" while the others are seen as "working class."

(c) INDIVIDUAL Perceptions Classification I - A vote for a party viewed by respondent as being from 1 to 3 on scale from "for the working class" at 1 to "for the middle class" at 7 is classified as being a working class vote and vice versa.

(d) INDIVIDUAL Perceptions Classification II - Same as the previous category except that votes for parties viewed as "4" are removed.

date to modern, competent and incompetent, powerful to weak, foolish to wise, for the middle class or working class, united to split, good to bad, left wing to right wing, strong to weak, honest to dishonest, dull to exciting, young to old, and slow to fast. Forty more questions pass before the respondent is asked for whom he voted in 1965. About another forty questions pass before the respondent is asked his subjective social class. It would take a very alert and class conscious voter indeed to be sufficiently aware of his party image response 40 or 80 questions ago in such a way that he would vary his report of his vote or class position to maintain his own cognitive consistency. In order to give deliberately consistent responses to the party class image questions initially, he would have to be very aware of the class issue.

Furthermore, the increase of the class vote using this new measure varies dramatically from region to region. This variation would be unlikely if the increases were a simple artifact of a new measure which allowed people to express their bent toward cognitive consistency. If cognitive consistency factors were significant, one would expect them to be most important in class-conscious, urbanized, industrialized, highly-educated Ontario and British Columbia. The increase is almost non-existent in Ontario and there is actually a drop in British Columbia. (See Ogmundson, 1972:Ch. 6.) This is exactly the opposite of what one would expect if cognitive consistency factors were playing an important role.

Before proceeding to a discussion of the findings, it may be wise to outline the uses and limitations of this new approach. The measure allows us to ascertain what the voters thought they were voting for. Consequently, it provides an improved measure of voter motivation. However, it does not provide a complete indication of voter sentiment on the issue. On the one hand, it cannot tap that portion of the population which, while motivated to cast a class consistent vote, fails to try to do so because of the lack of a perceived or realistic choice. It also cannot ascertain the degree of class interest which would emerge if

Canadians were exposed to elite mobilization on this issue comparable to that found in countries like Norway.[7] Furthermore the measure does not provide a measure of what we might term an objectively defined class vote. If a labourer votes Conservative and believes that this is a class consistent vote, this is important information if one is concerned about voter motivation. However, this does not mean that the social scientist must accept the voter's view that he is acting in such a way as to further his class interests. Indeed, from this point of view of the realities of power and the policy outputs of government, the intended class voting revealed by this measure is insignificant.

From the point of view of an interest in voter motivation however, this new measure can tell us something. In the case of Canada, the findings clearly show that voter interest in the class issue is substantially greater than conventional measures indicate. This supplements research reported elsewhere (Ogmundson, forthcoming) which indicates that class-related economic issues are the most important ones at the mass level in Canada, and that, consequently, the classless nature of Canadian politics cannot be attributed to a lack of voter interest in class issues. All this tends to focus attention on the important roles played by political elites[8] and suggests that explanation of the anomalous Canadian pattern may lie with the nature of élite activities. In particular, it would appear that minimization of the issue by the two major political parties is crucial to a full explanation of the classless nature of Canadian politics and of the considerable dissensus on the positioning of the political parties. In short, the findings of this paper support the revised view of Alford (1967) and Schwartz (1974) that explanation of the classless nature of Canadian electoral politics has more to do with the Canadian political parties than with the Canadian voters.

These findings also indicate, perhaps not surprisingly, that voter behaviour is influenced by what the voters think they are doing. More particularly, these findings indicate that conventional measures, which do not take into account what the voters think they are voting for, would seem likely to be inadequate as a measure of mass sentiment. This would be especially true concerning issues which are minimized by the major parties. More particularly still, these findings indicate that voter perception of their electoral options, and indeed the actual nature of those options, is likely to influence voter behaviour and hence should be taken into systematic consideration in studies of the vote. Consequently, they also suggest that regional and international variations in voting patterns may often best be attributed in the main to the nature of electoral options, and not, as is so often assumed, to the nature of mass social structure and public opinion (e.g., Ogmundson, 1975b). The measures used in this paper provide a means by which differences in electoral options, on a variety of dimensions, may be partially taken into account.

The finding that the classless nature of Canadian politics apparently has more to do with the Canadian political parties than with the Canadian citizenry is very similar to that of Hamilton (1972) in the United States. Findings such as these lead to the more general suggestion that one cannot reasonably assume, as is so often done, that the politics of a democracy faithfully reflect the salient concerns of its citizens. This, in turn, points to the importance of elite variables and the need for integrated conceptual frameworks which simultaneously consider both mass and elite variables. In distinct contrast to conventional approaches which assume that political explanation somehow resides in the nature of mass social structure and the consequent public will, a simultaneous emphasis on the

[7] There is reason to believe that the effects of such mobilization may be considerable. For example, the level of the class vote in the United States jumped from its average .16 level to .41 in 1948 (Alford, 1963b:103) when the Truman-Dewey confrontation made class issues salient to the population. (See Berelson et al., 1954.)

[8] The term "elite" is used in the sense that Porter (1965) uses the term. It refers to the relatively small groups of people in the important positions at the head of our major institutions, i.e., generals, bishops, deputy ministers, cabinet ministers, corporation directors and so forth. More specifically, the phrase "political elites" refers, of course, mainly to political party leadership. However the broader rubric is used in order to keep the general picture in mind. Political elites may be strongly influenced by other elites. For example, it would seem unwise to assume that the fact that the business elite finances the two major political parties in Canada has no influence on their minimization of the class issue.

explanatory power of elite variables suggests that present realities may be an outcome of elite interests rather than mass desires, and that the *status quo* may be illegitimate in terms of democratic values. (For further discussion of these points, see Ogmundson, forthcoming.)

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"THE INTERNAL STRATIFICATION OF THE WORKING CLASS": A REANALYSIS*

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This paper reanalyzes data from a recent study of the internal stratification of automobile workers in four countries. Where the previous research relied on the inspection of percentage differences in cross-tabulations, this paper utilizes loglinear models for the decomposition of association. The earlier study reported cleavages in participation patterns within the working class. It also noted an "increasing internal stratification of the working class," citing as evidence divergencies in participation by levels of skill and industrialization. Using an appropriate statistical model, the reanalysis reported here shows: (1) only limited support for the hypothesis that participation varies by skill level; (2) little, if any, support that skill differences in participation increase with industrialization; and rather, (3) industrialization is the strongest correlate of participation.

In some recent research, William Form (1973) poses several questions about the internal stratification of the working class: (1) What is the relationship between one's involvement in the social system inside and outside the factory? (2) Does system involvement vary between more skilled and less skilled workers? And (3) finally, how are the above mentioned relationships affected by national levels of industrialization?

Form hypothesizes that the work place dominates other institutional arenas and participation patterns for skilled workers will spill over from the factory to the outside (Form, 1973:698). For the less skilled workers, Form predicts no relationship between participation in- and outside of the factory. He also hypothesizes that skilled workers have more participational advantages than do less skilled workers, and thus more system involvements.[1]

That such system involvements are indicative of the effects of structural differentiation on social stratification is summarized by Form:

> My position is that industrial workers are not a homogeneous mass, but a stratified body with strong cleavages between the skilled and less skilled workers. The more industrialized the society, the more distinctive skill strata become in their plant and community involvement. The structural differentiation attending industrialization . . . is accompanied by a pattern wherein skilled workers become increasingly active in various social systems of the society, more differentiated from the unskilled in their behavior, and more independent in their politics . . . This pattern solidifies with industrialization. As industrial societies become more integrated and organizationally dense, skilled workers become involved in both formal and informal systems inside and outside the factory. (1973: 697-9)

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[1] The work reported here addresses primarily one facet of Form's 1973 article (i.e., a reanalysis of the data). It is believed that the article by Form might equally well be scrutinized from two other vantage points. First, the social psychology implicit in the hypotheses might be reexamined. Second, a number of important issues vis-à-vis measurement and conceptualization in social stratification are inherent in Form's argument (see, for example, Duncan, 1966; 1968; Treiman, forthcoming).

Statistically, Form's hypotheses may be summarized as follows: (1) among more-skilled workers, but not among less-skilled workers, there will be a positive relationship between participation inside and outside the workplace; (2) there will be a positive relationship between skill and participation on or off the job; and (3) that differences in participation by skill level will increase with industrialization. While Form (1973:709) concludes, "the basic hypotheses of this paper tend to be supported," he presents no data relating participation on and off the job, which would be needed to test the first hypothesis. Form's analysis does deal directly with the second and third hypotheses, and our reanalysis is limited to them.

To test the latter two hypotheses, Form looks at cross-classifications of some 40 system involvement variables by three levels of skill among automobile workers in four nations, which are taken to vary in level of industrialization (hereafter referred to as "industrialization"). Form tests the relationships between skill and participation with the usual Pearson X^2 statistic, but apparently he relied on inspection of percentage differences to test the interaction hypothesis (i.e., that skill differences in participation increased with industrialization). Using an appropriate statistical model, we shall demonstrate: (1) there is only limited support for the hypothesis that participation varies by skill-level; (2) there is little, if any, evidence that skill differences in participation increase with industrialization; and rather, (3) industrialization (country) is by far the strongest correlate of participation.

Form's Tables 2 through 8 give the percentages of persons participating by skill and industrialization levels. Unfortunately, while these descriptive statistics were not misleading, it was not possible to reconstruct the original cell frequencies from any of these tables. Professor Form kindly provided the raw frequencies for eight of the three-way classifications, which are the object of our reanalysis.[2] Table 1 summarizes our reanalysis; each panel refers to a participation measure used by Form in the cited table.

Our reanalysis is based on Leo Goodman's system for the hierarchical analysis of cross-classifications (1970; 1972; Davis, 1973). Let P = participation, I = industrialization and S = skill. In Table 1 the first five lines of each panel pertain to a statistical model which might account for all of the association in the classification of P by I by S. The model in line 1, denoted by [IS] [P], estimates the complete table from the marginal (two-way) association of I and S, and the P marginal. It says that I and S may be associated, but their bivariate distribution is statistically independent of P. The columns denoted by "df," "X^2_{LR}," and "P," refer respectively to the degrees of freedom in estimating a given model, the value of the chi-square likelihood-ratio, and the probability of obtaining the X^2_{LR} by chance. The column denoted by "% of association" refers to the percent of association (relative to the base model) attributable to fitting the given marginal (s). In fitting the base model [IS] [P], we have removed the association between I and S, which is irrelevant to the hypotheses offered by Form. Thus, we take the X^2_{LR} under this model to represent all (100%) of the association involving P, to be explained by S, I or their interaction.

The second row in each panel fits the univariate marginals ([S] [I] [P]) for each variable to the given cross-classification, testing the hypothesis of independence among the marginals. The third row for each panel fits all pairwise and lower-order marginals ([SI] [SP] [IP]), with the X^2_{LR} referring to the association attributable to the three-way interaction. Rows four and five fit several pairwise marginals used in obtaining the amounts of association accounted for by the participation-industrialization marginal (row six) and the participation-skill marginal (row seven).

Several observations are immediate from Table 1. First, the model specifying independence among the marginals (row two) does not provide an adequate fit in any of the tables. Second, in six of eight cases (A,B,C,E,F,G), fitting all pairwise marginals adequately accounts for the observed frequencies at conventional probability levels. Obversely, the three-way interaction accounts for a significant component of the association in only two of eight tables (D,H). In comparison D, the three-way interaction "explains" eleven percent of the association among industrialization, skill and activities which took place at

[2] Available upon request.

Table 1. Decomposition of Relevant Association for Eight Comparisons

	Dependent Variable/Models	df	X^2_{LR}	p	% of Association	
A.	MEETS WITH FELLOW WORKERS (Table 4b)					
(1)	Due to relevant association [IS] [P]	11	27.66	.004	100	(1)
(2)	[P] [S] [I]	17	65.35	.000		
(3)	Due to three-way interaction [PI] [PS] [IS]	6	2.78	>.5	10	(3)/(1)
(4)	[PI] [IS]	8	5.66	>.5		
(5)	[PS] [IS]	9	21.96	.009		
(6)	(A3 vs. A5) Association attributable to participation-industrialization marginal	3	19.18	.000	69	(5)-(3)/(1)
(7)	(A3 vs. A4) Association attributable to participation-skill marginal	2	2.88	.243	10	(4)-(3)/(1)
B.	INDEX OF OUTPLANT INTERACTIONS (Table 4d)					
(1)	Due to relevant association [IS] [P]	11	391.39	.000	100	
(2)	[P] [S] [I]	17	444.59	.000		
(3)	Due to three-way interaction [PI] [PS] [IS]	6	7.79	.239	2	
(4)	[PI] [IS]	8	11.33	.184		
(5)	[PS] [IS]	9	389.52	.000		
(6)	(B3 vs. B5) Association attributable to participation-industrialization marginal	3	381.53	.000	97	
(7)	(B3 vs. B4) Association attributable to participation-skill marginal	2	3.54	.191	1	
C.	VACATION OUT OF TOWN (Table 5)					
(1)	Due to relevant association [IS] [P]	8	101.70	.000	100	
(2)	[P] [S] [I]	12	133.89	.000		
(3)	Due to three-way interaction [PI] [PS] [IS]	4	3.70	.447	4	
(4)	[PI] [IS]	6	19.19	.004		
(5)	[PS] [IS]	6	78.83	.000		
(6)	(C3 vs. C5) Association attributable to participation-industrialization marginal	2	75.13	.000	74	
(7)	(C3 vs. C4) Association attributable to participation-skill marginal	2	15.49	.000	15	

home and away from home during vacations. In comparison H, the significant interaction accounts for 13% of the association among level of industrialization, level of skill, and the presence of high normless anomie. Even in the two cases where the predicted interaction is statistically significant, the size of the effect is relatively small.

Let us compare these results in more detail with those obtained by Form. One of the system involvement areas surveyed was the respondents' contacts with workmates outside the factory. Five indicators of interaction with workmates were presented in Form's Table 4 (1973:702). Of the data presented, Form summarizes:

> Evidence in Table 4 shows that relatively more of the skilled workers had extra-work contacts with friends both from their workgroup and other departments. . . .a

Table 1. (Continued)

	Dependent variable/Models	df	χ^2_{LR}	p	% of Association
D.	ACTIVITIES AT HOME AND AWAY DURING VACATION (Table 6)				
(1)	Due to relevant association [IS] [P]	11	153.72	.000	100
(2)	[P] [S] [I]	17	227.42	.000	
(3)	Due to three-way interaction [PI] [PS] [IS]	6	16.37	.012	11
(4)	[PI] [IS]	8	35.60	.000	
(5)	[PS] [IS]	9	122.58	.000	
(6)	(D3 vs. D5) Association attributable to participation-industrialization marginal	3	106.21	.000	69
(7)	(D3 vs. D4) Association attributable to participation-skill marginal	2	19.23	.000	13
E.	INTEREST IN NATIONAL NEWS (Table 9a)				
(1)	Due to relevant association [IS] [P]	11	57.64	.000	100
(2)	[P] [S] [I]	17	95.26	.000	
(3)	Due to three-way interaction [PI] [PS] [IS]	6	2.87	>.5	5
(4)	[PI] [IS]	8	8.05	.429	
(5)	[PS] [IS]	9	53.08	.000	
(6)	(E3 vs. E5) Association attributable to participation-industrialization marginal	3	50.21	.000	87
(7)	(E3 vs. E4) Association attributable to participation-skill marginal	2	5.18	.080	9
F.	PARTICIPATION IN NATIONAL PROBLEM DISCUSSIONS (Table 9b)				
(1)	Due to relevant association [IS] [P]	11	77.70	.000	100
(2)	[P] [S] [I]	17	113.34	.000	
(3)	Due to three-way interaction [PI] [PS] [IS]	6	4.32	>.5	6
(4)	[PI] [IS]	8	27.29	.001	
(5)	[PS] [IS]	9	52.56	.000	
(6)	(F3 vs. F5) Association attributable to participation-industrialization marginal	3	48.24	.000	62
(7)	(F3 vs. F4) Association attributable to participation-skill marginal	2	22.97	.000	30

five question index which summarized different types of contacts outside the plant showed that more of the skilled workers had outside contacts with their workmates. . . .the trend for skilled workers to have more outplant contact with coworkers increased with level of industrialization of the country. . . .(1973: 701-3)

Two of the five comparisons were reanalyzed (4B, meets fellow workers not in work group outside the factory, and 4D, index of outplant interactions) and are presented in panels A and B of Table 1. These results show any divergence of the skill-participation relationship by level of industrialization to be statistically nonsignificant in both cases. Rather, the industrialization-participation relationship accounts for 69% of the association in the case of meeting with fellow workers and 97% of the association for the index of outplant interactions. The variation

Table 1. (Continued)

	Dependent Variable/Models	df	χ^2_{LR}	p	% of Association
G.	IDENTIFICATION OF NATIONAL PROBLEMS (Table 10a)				
(1)	Due to relevant association [IS] [P]	11	127.69	.000	100
(2)	[P] [S] [I]	17	163.96	.000	
(3)	Due to three-way interaction [PI] [PS] [IS]	6	2.93	>.5	2
(4)	[PI] [IS]	8	37.59	.000	
(5)	[PS] [IS]	9	93.17	.000	
(6)	(G3 vs. G5) Association attributable to participation-industrialization marginal	3	90.24	.000	71
(7)	(G3 vs. G4) Association attributable to participation-skill marginal	2	34.66	.000	27
H.	HIGH NORMLESS ANOMIE (Table 11)				
(1)	Due to relevant association [IS] [P]	11	193.04	.000	100
(2)	[P] [S] [I]	17	233.25	.000	
(3)	Due to three-way interaction [PI] [PS] [IS]	6	24.18	.001	13
(4)	[PI] [IS]	8	25.13	.002	
(5)	[PS] [IS]	9	192.29	.000	
(6)	(H3 vs. H5) Association attributable to participation-industrialization marginal	3	168.11	.000	87
(7)	(H3 vs. H4) Association attributable to participation-skill marginal	2	.95	>.5	1

KEY: [P] = participation; [I] = industrialization; [S] = skill.

of system involvements by skill level, which is emphasized by Form (1973:701-3), accounts for only ten and two percent of the association in the respective tables.

Another area of system involvements examined was free time and family activities. Based on Tables 5 and 6 (1973:703-4; percentage of workers who spent at least part of their vacation out of town, and activities which took place at home and outside the home during vacations), Form concludes:

> In sum, an analysis of the pattern of free time activities points to emerging differences in the life styles of workers at different levels; the higher their skill, the less passive their activities and the less they took place in local groups . . . . thus again, the skill levels were more differentiated in the most industrialized countries. . . . (1973:703)

The reanalyses can be found in panels C and D of Table 1. The data fail to support either of Form's conclusions. First, the "emerging life styles of workers at different levels" absorbs only 15% and 13% of the association in the two tables. Rather, level of industrialization accounts for the largest portion of variation in participation patterns (74% and 69% of the association, respectively). Second, the three-way interaction is small in both cases (4% and 11%), but is statistically significant for the latter.

A third participational arena for which data were reanalyzed was national involvement. Three of four measures were chosen: Table 9 in Form (1973:706), interest in national news and participation in economic and political discussions (panels E and F in Table 1), and Table 10 (1973:707), specification of national problems which require organizational solutions (panel G). Again, the presence of the three-way interaction was cited as a very salient differentiating phenomenon:

> However, within each nation, more of the skilled than the less skilled identified national problems; and this trend was stronger in the more industrialized societies (see Table 9) [sic—this should read (see Table 10)].... Two remaining indicators of national involvement were reading newspapers and participating in discussion of political and economic events. Data in Table 11 [sic—this should read Table 9] show that in all countries, the higher their skill the more workers were interested in both community and national news. This trend was much stronger in OLDS and FIAT than it was in IKA and PAL. Finally, more of the skilled, with the possible exception of FIAT, discussed political and economic events. As in the case of other social systems, so it was for national involvement; differences among skill levels were larger for the more rather than the less industrialized nations. (Form, 1973:706-7) [3]

In sharp contrast, the three-way interactions for these comparisons are effectively nonexistent (5%, 6% and 2% of the association, respectively). Again, the industrialization-participation marginal is by far the most informative component in evaluating the observed cell frequencies, accounting for 87%, 62% and 71% of the relevant association. This too, is in sharp contrast to Form's emphasis on system involvements varying by skill level.

A final area considered was anomie by levels of skill and industrialization (Table 11 in Form, 1973:707). Here the three-way interaction is statistically significant ($P = .001$), but accounts for only 13% of the association in the cross-classification. Consistent with Form's discussion, the vast proportion (87%) of effects in this table are attributable to fitting the anomie-industrialization marginal. But his comments on variations in anomie by skill level are in vain, as the fitting of this marginal absorbs only one percent of the relevant association in the table.

These decompositions appear to portray "level of industrialization" as a very important variable in explaining system involvement, while level of skill and the predicted three-way interaction have very little, if any, explanatory power.

As was indicated earlier, the presence of a significant three-way interaction provides only minimal support for the specific interaction predicted. Goodman (1970:228-30; 1972:1039-44) defines a measure of effect, lambda (λ), which here refers to additive effects on the logarithm of the odds of participating (hereafter referred to as "effects on participation"), which can be used to further examine the significant interactions. The odds of participation (activities at home for comparison D and being high on normless anomie for comparison H) are an increasing function of the lambda values. Table 2 shows these values for the different levels of the interaction effect for the two comparisons in question.

For comparison D, if the pattern appropriate to the predicted interaction is present, then as industrialization and skill *jointly* increase, the effects on activities at home should decrease (i.e., as skill and industrialization jointly increase, activities at home should decrease). Looking at the actual lambda values we see positive or small negative effects on the odds of spending vacation time at home for the higher levels of industrialization and skill. For the lower levels of industrialization and skill, in conformity with the prediction, for unskilled workers from IKA ($\hat{\lambda} = .228$, $P < .05$) and semiskilled workers from PAL ($\hat{\lambda} = .350$, $P < .05$) there are significantly higher effects on participation. In contrast to the prediction, for both the unskilled for PAL ($\hat{\lambda} = -.078$, n.s.) and the semiskilled for IKA ($\hat{\lambda} = -.259$, $P < .05$) there are negative effects on the odds of vacationing at home. While all four of the significant lambdas might be interpreted as consistent with Form's hypothesis, such an argument would also have to contend with the sharp clustering of these effects at the lower levels of industrialization. The general pattern is more supportive of Form's prediction than not, but there are several large deviations. Remember also that the total size of this effect does not exceed eleven percent of the relevant association in the table.

For the second comparison (high normless anomie), we would expect a similar pattern of lambda values in support of the three-way interaction. As industrialization and skill jointly increase, the effects on the odds of

[3] OLDS, FIAT, IKA and PAL represent the four automobile plants, respectively, from the United States, Italy, Argentina and India.

Table 2. Lambda Values for the Three-Way Interaction for Comparisons D and H

	OLDS	FIAT	IKA	PAL
Activities Which Took Place at Home and Away During Vacation (D)				
Unskilled	-.112	-.037	.228*	-.078
Semiskilled	-.088	-.002	-.259*	.350*
Skilled	.200	.039	.032	-.272*
High Normless Anomie (H)				
Unskilled	.179	-.088	-.098	.007
Semiskilled	.271*	.050	-.196	-.124
Skilled	-.450*	.038	.296*	.117

*p < .05

high normless anomie should decrease. Conversely, as industrialization and skill jointly decrease, the effects on the odds of high normless anomie should increase (Form, 1973:707-8). For the lower levels of skill and industrialization (IKA and PAL, unskilled and semiskilled) this pattern of findings is absent except for the most extreme cell (which should show the largest positive lambda.) For the higher levels of skill and industrialization, an analogous pattern is found. Inconsistent with the prediction, in three of four cells in question the effects on the odds of being high on normless anomie are positive and significant in one case (OLDS, semiskilled; $\hat{\lambda} = .271$, $P < .05$). But for skilled workers from OLDS the effect of the interaction is in the predicted direction ($\hat{\lambda} = -.450$, $P < .05$). Two of three significant lambdas, for the table as a whole, are in the expected direction. Again, the general pattern can be found but sharp deviations exist, such that arguments based solely on the interaction effect are on tenuous grounds. In comparison H, the interaction represents 13% of the relevant association in the table.

Thus, in these two cases where the three-way interaction is statistically significant, a pattern of findings tending to corroborate Form's hypotheses was found, but was less than decisive due to several sharp deviations from the expected pattern and the small size of the overall effect.

Discussion

A reanalysis of eight tables from Form (1973) has shown the basic hypotheses and conclusions of that paper not to be supported. Where Form (1973:709) concludes system involvement, "differences among skill levels increase with industrialization of the country," this analysis has found the implied interaction effect to be nonexistent in six of eight tests. In the two tests where this effect is statistically significant, it is of negligible size. Form concludes (1973:709) that, "data from this study suggest the internal stratification of the working class may be increasing." If this phenomenon is: (1) manifested in social system involvement differences among skill levels, diverging by level of industrialization; (2) if the trichotomization by skill level is a valid and reliable representation of a stratification hierarchy, then very little, if any, evidence in support of the increasing internal stratification of the working class was found in this reanalysis. In fact, for all of the tables analyzed, level of industrialization has much more to do with social system involvement of factory workers than does skill level. This finding can be much more parsimoniously explained by the increased number of opportunities for social system involvement in more industrialized societies (the United States and Italy versus Argentina and India), rather than invoking explanations of work group solidarity or participational cleavages by skill level.

A partial caveat need be mentioned regarding this reanalysis. Form discusses over forty dependent, system involvement measures. Eight of these measures, which seemed

a priori, central to the reasoning presented by Form, were chosen for reanalysis. It is possible that the hypothesized patterns might be found in other tables. But this must be shown by an appropriate statistical model; the evidence adduced by Form is clearly inadequate.

Form utilizes what is analogous to a synthetic-cohort approach, sampling individuals at one point in time from the same industry, in four countries. To the extent the four countries are not indicative of all levels of industrialization and to the extent the implied synthetic-cohort misrepresents actual changes in the patterns of social stratification through time, any inferences concerning trends in social stratification will be in error (Form, 1973:709). In the interest of an accurate test, his hypotheses might well describe what is occurring, but go unevidenced in the data due to not allowing for sufficient variation in industrialization.

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DETERMINANTS OF CAREER PLANS: INSTITUTIONAL VERSUS INTERPERSONAL EFFECTS*

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This article examines the effects of institutional and interpersonal influences upon career plans among Israeli youth. The Israeli system of education activates a series of institutionalized controls to promote centralized public policies with regard to educational and occupational opportunities. Hence, individual goals and plans for future careers are severely constrained by formal mechanisms such as an early separation of more promising students from less able ones, differential training of various classes of students through distinctive types of high schools and the granting of official credentials in the form of various diplomas and licenses. Such institutional arrangements are one form of a "sponsored" mobility system within which the role of informal, interpersonal influences on levels of career aspiration is rather limited, particularly in later stages of the educational process. Empirical findings obtained in Israel support this thesis. A comparative analysis which draws upon American studies points to some cross-cultural implications for further research.

Recent research on the early stages of status attainment has yielded a series of related models depicting the major factors underlying this process and the causal order among them (Sewell, Haller and Portes, 1969; Sewell, Haller and Ohlendorf, 1970; Woelfel and Haller, 1971; Hauser, 1972). Drawing upon extensive previous efforts in this domain (cf. Wilson, 1959; Simpson, 1962; Ellis and Lane, 1963; McDill and Coleman, 1963; Alexander and Campbell, 1964; Campbell and Alexander, 1965; Cohen, 1965; Rehberg and Westby, 1967; Sewell and Shah, 1968; Duncan et al., 1968; Kandel and Lesser, 1969), these models have shown how certain social and psychological properties of a youth (e.g., socioeconomic status and mental ability) affect both academic performance in high school and the nature of interpersonal influence exerted on him. The latter then shape the youth's level of educational and occupational aspirations.[1] In addition to the general description of this causal process, those models have assessed the relative importance of the variables involved, as well as the amount of direct versus indirect influence exerted by them.

Notwithstanding the theoretical contribution and the empirical evidence yielded by this line of endeavor, some of the major works tend to concentrate on individual and interpersonal processes within a given system of education while disregarding the properties of the system itself. More specifically, it is implied that, given certain predetermined individual constraints (e.g., family socioeconomic status, mental ability), one's progress throughout the educational process is essentially a resultant of motivation, performance and interpersonal encouragement. This concept takes the educational machinery for granted, leaving out the generic effect of institutional features upon individuals' aspirations and attainments. Assuming that modern societies share a common system of formal education which serves as a main vehicle for

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[1] In most of these models (cf. Duncan et al., 1968; Sewell et al., 1970), levels of aspiration determine, in turn, actual occupational attainment.

social mobility, these societies are by no means uniform with respect to the normative and structural features of that system. Differences in institutional characteristics may, in turn, have important consequences for the status attainment process, so that alternative models are required for the depiction of that process.

A useful distinction among types of educational systems is suggested by Turner's conceptualization of "contest" versus "sponsored" mobility (1960). Briefly, contest mobility attempts to create similar conditions and programs of education throughout the schooling process, in which practically everyone is permitted and encouraged to take his chances. Ideally, such a system maintains an open race among its youth, allowing them to compete for the prize of higher education with little regard to early differences in ability and achievement. Under such circumstances, the large majority of youth typically complete high school, unless they voluntarily drop out. Furthermore, practically all high-school graduates are considered qualified for higher education.

In contrast, a sponsored-mobility system attempts to regulate the participation in the schooling process at earlier stages. Ideally, such a system strives to maximize congruency between talents and occupations by means of selection and placement of youngsters in different types and levels of schooling. Thus, differences in ability are detected early and students are classified accordingly and differentially trained. In short, "contest mobility tends to delay the final award as long as practicable to permit a fair race; sponsored-mobility tends to place the time of recruitment as early in life as practicable to insure control over selection and training" (Turner, 1960:410).

Sponsorship is reflected in the structure of education mainly in the institutionalization of selection procedures, in the establishment of various school "charters" (cf. Meyer, 1970; Kamens, 1971) and in the application of formal credentials on the basis of standard tests and examinations. The operation of these and similar mechanisms produces a specific structure of education within which the process of status attainment takes place. Thus an educational structure which restricts the interference of its agencies with one's course of educational progress is likely to develop an essentially uniform school system which promotes similar schooling over all; such a structure, then, deemphasizes different functions of school charters and leaves the mobility structure of opportunities open to self-selection. Accordingly, credentials in the form of "earned" graduation are based primarily on effort and determination rather than on aptitude and performance (cf. Jencks, 1972:135-75). Where the educational system takes, instead, an active part in the regulation of the course of education, clear cut criteria such as scholastic aptitude or academic performance are likely to be established. These in turn become the standard for the selection and channeling of youth into different classes of schools. Consequently, such an educational process provides a variety of school "products" formally labeled by various sorts of certificates and diplomas, of which only a few permit further education toward elite positions.

THEORETICAL PERSPECTIVE

The preceding conception suggests that the structural properties of an educational system are highly consequential for the process of early status attainment. Thus, under conditions of sponsorship, careers are publicly defined by formal means and authorities so that informal cues become less critical for the formation of educational and occupational plans. On the other hand, under institutional arrangements that correspond more closely to the contest type, career plans are mainly entrusted to individuals, thus leaving greater room for informal messages.

While both types of formal and informal feedback are continuous throughout the process of schooling, their impact probably is most salient at points of transition from one level or program to another. At such phases of decision-making, the relative predominance of either source—institutional or interpersonal—depends on the clarity and determinism of its message. Whenever the educational system operates to communicate its assessment of ability and performance by means of official credentials, the role of reference groups or individuals becomes secondary. Conversely, where such a system performs in an equivocal and less rigid fashion, the need for personal guidance and persuasion becomes crucial. Hence, within a sponsored-mobility context, the more advanced one is in the schooling

process, the greater the cumulative effect of institutional directives and the less the relevance of interpersonal guidance for future career plans. Empirically, it is expected that the relative effect of "educational training" upon educational and occupational aspirations would be stronger than the corresponding effect of "significant others' influence."[2] Moreover, the expected effect of the latter factor is likely to diminish upon successive stages of official accreditation.

In order to examine these assumptions and hypotheses, the remaining part of this essay deals mainly with a study of Israeli youth with some comparisons of studies of American youth. In this manner, intra-societal as well as inter-societal examination of institutional *vis à vis* interpersonal effects is attempted.

INSTITUTIONAL FEATURES OF ISRAELI EDUCATION

At the time of the present study, the Israeli structure of education consisted of four main levels: primary education (eight years of schooling, usually between the ages 6-14); secondary education (2-5 years of schooling, varying from one type of training to another); higher education (2-4 years of undergraduate studies, varying according to the type of institution); graduate education (two or more years of academic and professional training). At approximately 18 years of age, practically every boy and girl is recruited for military service under compulsory draft law; for a duration, at that time, of two and three years for females and males respectively. Hence, higher education, if chosen, usually begins at the age of 20-21.

Toward the end of the primary education level, the Ministry of Education and Culture used to administer to all students in the country a standard composite aptitude test known as the "Seker" (literally "survey"). On the basis of their performance on the test, students were classified into various categories; accordingly, the better students were encouraged to continue in an "academic" type of high school and the remaining students were advised to proceed in either vocational, commercial, industrial or agricultural schools. To insure that students would, in fact, follow their "prescribed" educational careers, the governmental authorities allocated stipends, exemptions, loans and other forms of financial aid to those who selected the "appropriate" type of secondary school. Since secondary education imposes a heavy financial burden on the average Israeli family, such an aid serves as an important incentive to conformity. Furthermore, for the less promising students the chances to attend academic schools, especially the public ones which comprise the large majority of schools, were further reduced by the prevalent policy of admitting only applicants who successfully passed the "Seker."

The classification of students on the basis of the "Seker" constituted the first critical event in the life chances of these youth, due to the sharp differences among the various types of high schools. The academic school mainly prepares the student for higher education. The content and quality of its educational program, as well as the composition of its student body, constitute a clear "charter" for providing a broad, general education. These students are destined for higher education and professional careers. Schools belonging to other categories are more heterogeneous with regard to quality, curriculum, years of schooling, ability of students and other relevant factors; in general, they are chartered to produce technical personnel, craftsmen and skilled labor of various levels of proficiency.

The process of channeling youth into different tracks continues through the secondary education level. The major mechanisms employed for this purpose are standard exams (e.g., "preliminary matriculation exams"), placement of students in specialized fields of study and accreditation of various certificates and vocational licenses—all of which are applied within one to four years of schooling, depending upon completion of the specific training.

Like the "Seker," a second major barrier on the road to higher education is encountered upon completion of secondary education—the matriculation exams. These consist

[2] "Educational training" refers to the indicators of educational skills and performance as determined and acknowledged by a given system of sponsorship (e.g., type of schooling, level of education, grades and diplomas). The term "significant others' influence" summarizes the overall encouragement of parents, friends and teachers with respect to levels of aspiration (cf. Sewell et al., 1969).

of a battery of specific tests in several disciplines (e.g., mathematics, sciences, foreign languages, history, etc.)—administered by the Ministry of Education and Culture on a national scale. A successful performance in all exams qualifies a student for the "Matriculation Diploma." This certificate is an official prerequisite for candidacy to colleges and universities. Thus, admittance to college is guaranteed neither by graduation from any type of high school nor by the number of years of schooling; rather the matriculation diploma is a necessary, though insufficient in itself, credential for university enrollment.

While the matriculation exams are universally open regardless of type and level of secondary education, their content and requirements strongly favor graduates of academic high schools. Hence, while seemingly offering a "second chance," the actual odds for other students are rather slim. At the time of the present investigation, no more than about 20-25 per cent of Israeli youth acquired the matriculation diploma annually. Although students in academic schools comprise no more than 35-40 per cent, they annually constitute an overwhelming majority (over 85%) among recipients of the matriculation diploma. These figures may be explained by the fact that graduates of academic high schools enjoy a better preparation and are provided with so called "protective grades." The latter are granted by local schools and legally recognized by the Ministry, so that the final grades represent averages of the matriculation grades and the protective ones; such a recognition is awarded solely to academic high schools and it reflects the public "charter" of these schools as college preparatories.

Other characteristics of the Israeli sponsorship system could be described, but the features outlined above should suffice to indicate its regulative functioning at the various stages of schooling. Once the relevance of institutional constraints as such is recognized, it is necessary to explore their impact upon processes of career formation and status attainment.

SAMPLING AND DATA COLLECTION

For the purposes of this study, a national sample of youth was randomly drawn from military records of service men and women. This sample consists of over 500 males and females, aged 20-21 years, who have at least some secondary education. At the time of the survey, those youth were about to terminate their military duty in the Israeli Defense Forces at the beginning of the year 1971. Thus, respondents were approached at the time when plans and objectives were of immediate concern rather than related to a remote future. From this viewpoint, though two to three years older, those respondents are comparable to high school seniors who constitute the typical class of subjects in the American studies, since military service precedes college.

Data for this study were collected by written questionnaires administered to selected respondents by field workers at their various locations in military bases all over the country. With a very few exceptions, all subjects returned completed or nearly completed questionnaires. As noted earlier, Israel maintains a universal compulsory draft system so that this sample represents practically its entire cohort, with the omission of minor categories of adolescents (e.g., illiterates, juvenile delinquents, orthodox females, etc.).

VARIABLES AND THEIR MEASUREMENTS

Independent variables

Two dimensions were selected to represent the presumed effects of social background on the early status attainment process.[3]

Socioeconomic status. This dimension is expressed operationally by an index, comprising the following four measures: (1) family income—categorized into seven income levels; (2) father's occupation—classified into nine prestige levels by employing Duncan's method (1961), adjusted to the Israeli society, as suggested by Yuchtman and Fishelson (1972);[4] (3) father's education—years of schooling; (4) mother's education—years of schooling.

The index was constructed as an average of

[3] Due to technical difficulties, we were unable to secure information about the respondents' mental abilities. The omission of this variable, however, does not impede the specific purpose of our analysis since its effects are largely contained in the indicators of educational training.

[4] This adjustment involves the assignment of a greater weight to education than to income in assessing the prestige level of occupations.

the four measures, weighted by their item-to-total correlations.[5]

Sociocultural origin.[6] The cultural diversity of Israel, due to the continuous waves of immigration from a variety of countries, implies important differences in the values and attitudes held among the social groups and strata constituting this society. Such differences have affected the relative success of these groups in adjusting to the mainstream in general and to the society's modern institutional structures in particular (Eisenstadt, 1967).

Probably the most meaningful way of delineating the sociocultural dimension in Israel is along the lines of ethnic origin, particularly according to the distinction between "Orientals" (those who come from Near-Eastern and North African countries) and "Westerners" (immigrants from European and American countries). Oriental Jews come typically from traditional societies, with little exposure to modern, bureaucratic institutions. Their adaptation to the Israeli society has, therefore, been a difficult process (Eisenstadt, 1967; Lissak, 1969; Peres, 1971). Hence, it was expected that the differences between Oriental and Western youth would be expressed in the aspiration and attainment levels at the earlier stages of social mobility.

Intervening variables

Educational training. The specific features of the educational system in Israel, as outlined above, may be represented by three major indicators which together determine the quality and level of training of pre-college youth. These indicators were defined and operationalized as follows:

a. Type of High School–This variable was dichotomized into "academic" versus "vocational" and similar schools.

b. Years of Schooling–Number of completed years of secondary education (ranging from one to four years).

c. Matriculation–Since this examination consists of a battery of tests which can be taken over a period of time, respondents were classified into the following three levels: (1) those who did not pass any of the tests; (2) those who passed at least one, but not all of the tests; (3) those who passed all the tests and were accredited with the matriculation diploma (i.e., "certified").

Significant others' influence. For the purpose of assessing the impact of various interpersonal influences upon educational and occupational plans, three classes of "significant others" were selected: parents, friends and teachers. This particular selection of sources and the specific operationalization of their influence has been adapted in order to be in line with similar usages of this concept, particularly by Sewell and his colleagues (1969; 1970). Accordingly, the following set of questions was presented to respondents: (1) "What do your parents want you to do after release from the military service?" Responses were then classified according to (a) parents do encourage their child to go to college; (b) parents do not encourage their child to attend college or do not care. (2) "What do your friends plan to do after their military service?" Answers were classified according to whether friends plan or do not plan to attend college. (3) "What did your teachers advise you with respect to your future career?" Answers were classified according to whether the respondent was advised to proceed into higher education, or otherwise. For every individual, a composite index of SOI was constructed by a simple sum of the three items, where scores of 1 and 2 were assigned to negative and positive answers, respectively; thus index scores range between 3 and 6.

Dependent variables

Occupational aspirations. Respondents were asked to specify their actual intended occupations. The list of occupations was classified into 9 prestige levels in the same way as father's occupation.

Educational aspirations. This variable consists of a four-point scale, measuring the degree to which the respondent was certain he would attend college or (alternatively) enter the labor market.

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[5] Item-to-total correlations were as follows: (1) Family Income, 0.43; (2) Father's Occupation, 0.52; (3) Father's Education, 0.59; (4) Mother's Education, 0.49. For rationale and procedure, see Lord and Novick (1968: 98-9) and Bohrnstedt (1969:546).

[6] The sociocultural aspect was not included as a distinctive independent variable in the works of Sewell and his colleagues, presumably because of the relatively homogeneous populations they dealt with.

Table 1. Inter-Correlations among Indicators of Individuals' Social Background, Educational Training, SOI, and Career Plans (N = 534)

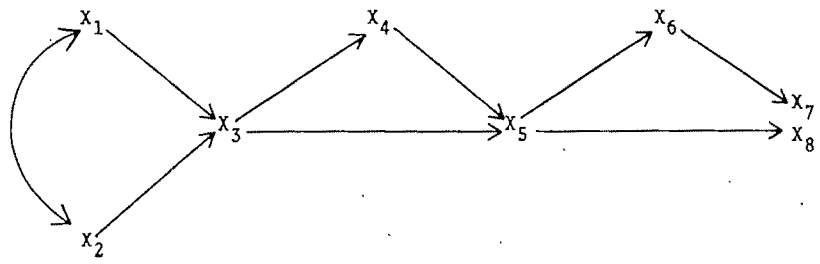
	X_1	X_2	X_3	X_4	X_5	X_6	X_7	X_8
X_1 - Socioeconomic Status	...	0.39	0.20	0.21	0.25	0.18	0.32	0.19
X_2 - Sociocultural Origin	...	...	0.14	0.25	0.22	0.25	0.29	0.23
X_3 - Type of High School	...	...	...	0.43	0.55	0.18	0.35	0.37
X_4 - Years of Schooling	...	...	...	...	0.60	0.29	0.49	0.44
X_5 - Matriculation Diploma	...	...	...	...	...	0.38	0.59	0.58
X_6 - SOI	...	...	...	...	...	...	0.47	0.39
X_7 - Occupational Aspirations	...	...	...	...	...	...	...	0.62
X_8 - Educational Aspirations	...	...	...	...	...	...	...	...

RESULTS

As a first step in the present analysis, intercorrelations among the variables contained in our model are presented in Table 1. The correlations matrix serves as an initial exploration of the relationships among determinants of career aspirations. A more rigorous way of testing our hypotheses requires an analysis of net effects rather than covariations alone. This may be performed by the method of path analysis which enables a quantitative estimation of direct and indirect effects underlying a system of causally ordered variables (e.g., Duncan, 1966). The proposed model assumes the following structure: an individual's social background (i.e., socioeconomic status and sociocultural origin) represents the first link of the causal chain. Educational training (i.e., type of high school, years of schooling and credentials) stands for the second link of this chain; whereas the third link is indicated by significant others' influence. All of these presumably determine the level of educational and occupational aspirations. Schematically, the hypothesized process may be drawn as in Figure 1. The results obtained by the application of the path analytic procedure to this model are presented in Table 2.

The use of this particular set of variables accounts for nearly half of the variance in levels of occupational aspirations (R = 0.69), and about 40 per cent in the variation of educational aspirations (R = 0.62).[7] The overall predictive power of this model may be further decomposed for the purpose of testing hypotheses. To begin with, consider the separate effects of indicators of educational training. The type of high school exerts practically no direct effect on either occupational or educational aspirations, since its gross impact (see Table 1) is entirely transmitted through the subsequent phases of training—years of schooling and matriculation. The number of years of schooling affects the two dependent variables directly, as well as via the matriculation credential. The acquisition of the matriculation diploma shows, in turn, relatively strong effects upon occupational and educational aspirations—directly as well as indirectly, through its impact on SOI. It may be ascertained that an individual's educational attainment, as primarily indicated by the

[7]The use of composite indices, instead of separate items, reduces the overall proportion of explained variance in the present case. For instance, the adoption of combined measure of SOI brings about a loss of 2.5% of explained variance in either occupational or educational aspirations.



X_1: Socioeconomic Status
X_2: Sociocultural Origin
X_3: Type of High School
X_4: Years of Schooling
X_5: Matriculation Diploma
X_6: Significant Others' Influence
X_7: Occupational Aspirations
X_8: Educational Aspirations

Figure 1. Determinants of Career Plans: Flow of Effects

possession of the matriculation diploma, has a significantly greater impact on both aspects of career aspiration than the corresponding effect of significant others' influence:[8] P_{74} = 0.35 vs. P_{75} = 0.25 and P_{84} = 0.39 vs. P_{85} = 0.18.

These findings demonstrate a typical aspect of status attainment process in a system such as the Israeli one. Namely, progress from primary school into a certain type of secondary school largely determines expected years of schooling. Both type of school and number of years completed influence the chances of obtaining the matriculation diploma. Possession of this particular certificate is quite critical for career plans. In fact, it appears that this credentialing effect is stronger than the effect of significant others.

Next, to assess the overall net impact of educational training *vis à vis* significant others' influence, the unique contribution of either to the proportion of explained variance may be sorted out. In practice, the variables under consideration are removed and the multiple correlation between the remaining and a dependent variable is recomputed. The reduction in explained variance in the latter correlation reflects the net contribution of the removed variables, over and above common variance. In the present case, educational training was found to add 17.8 per cent to the explained variance in levels of occupational aspiration (i.e., $R^2_{7.123456} - R^2_{7.126}$ = 0.468 – 0.289). Similarly computed, the net contribution of SOI amounts to 5.2 per cent. The corresponding results with regard to educational aspirations were 20.6 per cent and 2.8 per cent for educational training and significant others' influence, respectively. These findings further demonstrate that the quality of education one is processed through, in a sponsored-mobility system, plays a much more important role in the formation of career plans than that of significant others.

COMPARATIVE ANALYSIS

In order to broaden the meaning of the above results, a comparative perspective may prove helpful. For this purpose, the findings reported by Sewell and his associates (Sewell et al.,1969; 1970) may serve as a useful frame of reference. These scholars conclude that "perhaps the most important single finding is the critical role of significant others' influence

[8]The separate path coefficients from parents', peers' and teachers' influences to levels of occupational aspirations are: 0.15, 0.24, 0.02, respectively; the corresponding paths to educational aspirations are 0.13, 0.21, 0.06.

Table 2. Path Coefficients (and Standard Errors) for Six Determinants of Occupational (X_7) and Educational (X_8) Aspirations, Total Sample (N=534)

Independent Variables	Dependent Variables X_3	X_4	X_5	X_6	X_7	X_8
X_1 - Socioeconomic Status	.17 (.06)	.07 (.05)	.07 (.04)	.04 (.05)	.13 (.04)	.01 (.05)
X_2 - Sociocultural Origin	.07 (.06)	.17 (.05)	.04 (.04)	.16 (.05)	.05 (.04)	.07 (.05)
X_3 - Type of High School		.39 (.05)	.34 (.04)	.06 (.06)	.01 (.05)	.06 (.05)
X_4 - Years of Schooling			.43 (.04)	.08 (.06)	.17 (.05)	.11 (.05)
X_5 - Matriculation Diploma				.33 (.07)	.35 (.06)	.39 (.06)
X_6 - Significant Others' Influence					.25 (.04)	.18 (.05)

in the status attainment process" (Sewell et al., 1970:1095). The present findings suggest, in contrast, that SOI plays a relatively secondary role in the Israeli system. Due to immanent differences between the systems under consideration, a straight comparison between specific figures in the two studies cannot readily be drawn. Nevertheless, it appears both legitimate and illuminating to resolve the seemingly contrasting results, provided that systematic dissimilarities are kept in mind and a more refined analysis is performed.

It must be admitted that Sewell and his associates have selected a study of high school seniors, whereas the present sample consists of youth with a variety of educational training among whom high school seniors are only a minority. Moreover, the samples also differ in sex and age composition, as well as with respect to measurement of educational performance. It is possible to argue, therefore, that the obtained results in both studies are due mainly to differences in research design. Such an argument, however, does not fully recognize the fact that neither sampling nor the selection of measures has been made accidentally. Instead, differences in procedure reflect the underlying differences in the systems under study. Thus Sewell's sample apparently represents a large majority of white American youth, primarily all those brought up in social settings similar to the Wisconsin scene. An attempt to confine an Israeli study to a comparable sample of "high school seniors" alone would misrepresent the Israeli scene altogether. Similarly, the application of a standard yardstick of educational performance, such as the GPA, to an entirely different system of credentials is meaningless. Taking these considerations into account, cross-cultural comparisons of this type should weigh empirical results derived from unlike measures of analytically similar concepts. Such a strategy permits only an indirect inference, but one which seems more meaningful than a straight contrast.

With these qualifications borne in mind, and for the sake of the argument, it is still possible to attempt a closer comparison between the Wisconsin studies and ours by sorting out a subsample of Israeli students roughly matching the American high school seniors. This group of students consists, however, of two main classes, neither of which is strictly identical to their American counterparts. The first one comprises youth who completed four years of secondary education, mainly in academic high schools, but are not legally eligible for college since they do not possess the matriculation certificate. The second class comprises youth who have already acquired this diploma and are, hence, qualified for enrollment in institutes of higher education, regardless of specific type of training or completed years of

Table 3. Path Coefficients (and Standard Errors) for Three Determinants of Career Plans in Two Subsamples of "High School Graduates" (Males)

	Seniors*		Matriculates**	
	Occupational Aspirations	Educational Aspirations	Occupational Aspirations	Educational Aspirations
Socioeconomic Status	.33 (.11)	.11 (.12)	.14 (.13)	.07 (.06)
Sociocultural Origin	-.03 (.12)	.15 (.12)	-.12 (.12)	-.13 (.12)
Significant Others' Influence	.35 (.11)	.25 (.12)	.01 (.13)	.12 (.13)

*Students completed four years of high school but have not acquired the matriculation diploma.

**Students accredited with the matriculation diploma, regardless of schooling.

schooling. Thus, the former represent a minority of their age-group which still has a realistic option to attend college, provided they attain the matriculation diploma. The decision to try to acquire the matriculation diploma is usually contingent upon career plans and therefore becomes a matter of personal choice. Those youngsters are likely to be exposed, then, to interpersonal pressures and advice. The other class of youth has, in fact, already passed this stage. The very possession of the matriculation diploma indicates that most of them aspire to higher education as well as having been "chosen" by the system for future elite positions. Official statistics demonstrate that an overwhelming majority of youth who have the matriculation diploma proceed to colleges and universities. It appears clear that for these youth informal definitions of aspirations and goals become relatively unnecessary.

In order to examine this line of analysis empirically, the path analytic procedure was separately applied to the two subsamples of "high school graduates." To come even closer in this comparison to Sewell's sample, only males were included. Table 3 summarizes the results.[9]

As may be observed, there is a sharp difference in the relative strength of significant others' influence upon career plans between certified and uncertified aspirants. These results agree strongly with the main theme of the present discussion and demonstrate the specific effect of credentialing in an educational system like the Israeli one: among college eligibles, the effect of SOI diminishes to practically nil. Finally, compared with the results obtained by Sewell and his associates—the path coefficients from SOI to occupational and educational aspirations were 0.54 and 0.59 in the 1969 study, and 0.44 and 0.51 in the 1970—the figures reported in Table 3 reaffirm the assertion that interpersonal effects are not as critical in a sponsored mobility system as they seem to be in a contest-oriented one.

CONCLUSION

The main purpose of the preceding discussion has been to examine the effects of institutional features of educational systems on the causal determination of individual career aspirations and attainments. Distinguishing sponsored from contest mobility, it has been our central concern to show how differences between these two systems affect the place of interpersonal influence on career aspirations. In a sponsored-mobility system, the effect of interpersonal influence was found to be minor because a youth's career aspirations are so strongly determined by the credentialing process of the formal educational system. While some controversy may exist regarding the distinction between contest and sponsorship, particularly in their manifestation in the real world, the ultimate test of this conceptual model lies in its capacity to account coherently for differences

[9] Note that these figures are presumably somewhat inflated estimates of SOI due to the deletion of measures of academic performance from this analysis.

in the process of status attainment in different educational systems within as well as between societies.

On this issue, our approach seems to be consistent with Porter's recent account of the differential impact of interpersonal effects on white and black students in the U.S.: "Where the contest model applies, the booster effect would occur; where the sponsored applies, the effect of significant others would be indirect. Thus both the clear direct effect of significant others' on white educational attainment and the lack of such an effect among blacks are rendered more understandable when seen from Turner's perspective." (Porter, 1974: 315)

Further examination is required in order to apply the present perspective to a larger range of situations. Such an examination should preferably be carried out in societies or communities with different constellations of institutional properties. In conducting analyses of this kind, at least two considerations should serve as useful guidelines: (a) the specific manifestations of sponsorship may vary from one social context to another. Hence, empirical indicators are not necessarily identical across different educational systems; (b) since both elements of contest and sponsorship co-exist in most real life situations, it is necessary to identify expressions of either principle and detect their actual operation within a given system of education.

In sum, the present article suggests that the process of early status attainment in modern society is shaped by a large variety of factors operating at various levels. However, the relevance and importance of those factors is not necessarily uniform across social settings. Hence, a proper analysis of this phenomenon requires a delineation of institutional characteristics of a particular system. Obviously such properties may prove common as well as unique to various systems of education.

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COMMENTS

"THE INTERNAL STRATIFICATION OF THE WORKING CLASS": A REANALYSIS*

(COMMENT ON SPENNER, ASR AUGUST, 1975)

I appreciate Mr. Spenner's comments on my work because they are based on statistical techniques which were unavailable when I launched the research. Goodman's method for studying interaction effects clearly enables us to test complex hypotheses much better than earlier techniques. I was therefore happy to provide all the data Spenner requested but I am disappointed with his report because he: (1) misinterpreted what he found, owing to his unfamiliarity with the larger study; (2) underemphasized his results which supported my hypothesis; (3) selected data which supported his criticism but ignored available data which, when I re-tested with Goodman's method, robustly supported my hypothesis; (4) ignored contextual problems of interpretation necessary in comparative research.

1. In a series of articles (see References in Form, 1973), I discussed three hypotheses of the study: (1) that the extent of national industrialization is the most important factor explaining the behavior of industrial workers; (2) that these workers are not a homogeneous mass since skill differences account for patterned variation in behavior; (3) that skill differences which increase with industrialization have an increasingly important effect on worker behavior. Spenner's reanalysis of my data (see line 6 of each panel of his Table 1) strongly supports my first hypothesis; the percent of industrialization is large and significant in all 8 panels. When I ran six Goodman tests on additional data relevant to the first hypothesis, I also found significant results, although at lower levels than Spenner reports. However, because he found three-way interaction effects to be smaller than industrial effects, he concluded that my third hypothesis concerning the internal stratification of the working class should be rejected. This view is unjustified. The fact that my first hypothesis is supported more strongly than the third, as I repeatedly predicted, does not imply the rejection of the third. Spenner's failure to consider my hypothesis in context is possibly explained by his apparent unfamiliarity with the larger study. For example, he objects that I presented no data on the hypothesis dealing with the spillover effect of interaction on the job for social participation off the job, but he ignored repeated references to my article (Form, 1972) on that topic.

2. Spenner's tendency to underemphasize his results which support my position is most clearly seen in his conclusion that skill effects on participation are unimportant. Yet in five of his eight tests (line 7 of his Table 1), the percentage of association explained by skill is statistically significant and moderately large – from 9 to 30 percent. In the remaining three cases the evidence is weaker, but it still supports the hypothesis that skill affects participation in factory and nonfactory social systems. Moreover, to anticipate the next point, Spenner ignored three important data sets reported in my article. When I subjected these to Goodman's technique (line 7, Table 1.A, B and E), the percentage of association explained by skill is substantial: 38, 23 and 35 percent, respectively. Even with his selected data, Spenner's statement that my conclusions about skill effects are wrong is doubtful; when we consider that his analysis is based on a limited set of data, his assessment seems to be an ex-cathedra judgment.

3. Spenner tends to select data from my article which lend less support to my hypothesis and to ignore data which might support it. This is best seen in his treatment of the third hypothesis dealing with three-way interaction effects: that skill increasingly affects participation on and off the job with increasing industrialization (line 3 of his Table 1 and

*I am grateful to Fred C. Pampel for the statistical work on this paper.

Table 1. Decomposition of Relevant Association for Five Comparisons

	Dependent Variable/Models	df	X^2_{LR}	P	Percent of Association
A.	MEETS WITH FELLOW WORKERS IN COMMUNITY ORGANIZATIONS (Table 4a)				
(1)	Due to relevant association (IS) (P)	11	127.64	.000	100%
(2)	(P) (S) (I)	17	165.40	.000	
(3)	Due to three-way interaction (PI) (PS) (IS)	6	41.83	.000	33%
(4)	(PI) (IS)	8	89.77	.000	
(5)	(PS) (IS)	9	82.28	.000	
(6)	A3 vs. A5 Association attributable to participation-industrialization marginal	3	40.45	.000	32%
(7)	A3 vs. A4 Association attributable to participation-skill marginal	2	47.94	.000	38%
B.	QUALITY OF INTERACTION WITH FELLOW WORKERS (Table 4e)[a]				
(1)	Due to relevant association (IS) (P)	11	48.27	.000	100%
(2)	(P) (S) (I)	17	86.03	.000	
(3)	Due to three-way interaction (PI) (PS) (IS)	6	9.53	.146	20%
(4)	(PI) (IS)	8	20.64	.008	
(5)	(PS) (IS)	9	35.02	.000	
(6)	B3 vs. B5 Association attributable to participation-industrialization marginal	3	25.49	.000	53%
(7)	B3 vs. B4 Association attributable to participation-skill marginal	2	11.11	.010	23%
C.	UNION OPINION FAVORABLE (Table 3c)[a]				
(1)	Due to relevant association (IS) (P)	11	42.39	.000	100%
(2)	(P) (S) (I)	17	80.15	.000	
(3)	Due to three-way interaction (PI) (PS) (IS)	6	16.94	.010	40%
(4)	(PI) (IS)	8	18.42	.018	
(5)	(PS) (IS)	9	40.34	.000	
(6)	C3 vs. C5 Association attributable to participation-industrialization marginal	3	23.40	.000	55%
(7)	C3 vs. C4 Association attributable to participation-skill marginal	2	1.48	.40	3%

my Table 1). Although I sent Mr. Spenner most of the raw data I used to prepare the article, equivalent to about 40 tables, he analyzed only eight. Before commenting on his sampling procedures, let us examine the consequences of selecting these eight tables.

Spenner (Tables 1.D and 1.H) describes statistically significant associations as "relatively small" (I call them moderately high) for two variables: "activities at home and during vacation" and "normless anomie." In the remaining six panels, associations are small and nonsignificant. However, Panel 1.C, "vacations out of town," is missing for the Indian case because vacations were not formalized in the Indian factory. But skill differences for this item were clearly greater for the more industrial countries (U.S. and Italy) than for Argentina. In India, absenteeism was strongly associated with out-of-town visiting (the phenomenal equivalent of vacations), but no skill differences were found for absenteeism (Shar-

Table 1. (Continued)

	Dependent Variable/Models	df	χ^2_{LR}	P	Percent of Association
D.	COMMUNITY PROBLEMS IDENTIFIED (Table 8g)				
(1)	Due to relevant association (IS) (P)	11	7.69	.5	100%
(2)	(P) (S) (I)	17	45.45	.000	
(3)	Due to three-way interaction (PI) (PS) (IS)	6	2.03	.5	26%
(4)	(PI) (IS)	8	2.10	.5	
(5)	(PS) (IS)	9	7.55	.5	
(6)	D3 vs. D5 Association attributable to participation-industrialization marginal	3	5.52	.18	72%
(7)	D3 vs. D4 Association attributable to participation-skill marginal	2	.07	>.5	1%
E.	DISCUSSES POLITICAL AND ECONOMIC ISSUES WITH FELLOW WORKERS (Table 3d)				
(1)	Due to relevant association (IS) (P)	11	114.55	.000	100%
(2)	(P) (S) (I)	17	152.31	.000	
(3)	Due to three-way interaction (PI) (PS) (IS)	6	30.15	.000	26%
(4)	(PI) (IS)	8	69.81	.000	
(5)	(PS) (IS)	9	73.41	.000	
(6)	E3 vs. E5 Association attributable to participation-industrialization marginal	3	43.26	.000	38%
(7)	E3 vs. E4 Association attributable to participation-skill marginal	2	39.66	.000	35%

KEY: (P) = participation; (I) = industrialization; (S) = skill.

[a]These data are for systematic samples for OLDS and IKA. All other data in Spenner's tables and mine are for analytic samples in which skilled workers were slightly overrepresented.

ma, 1975:95), as the hypothesis predicts. In Panels E and G, Spenner failed to follow the text. For "interest in national news" and "specification of national problems," the industrialization effect (line 6) runs *opposite* the hypothesized pattern: the less industrial the country, the greater the interest in national news and the ability to specify national problems because, as I explained, the problems of less developed societies are so overwhelming that everyone knows of and talks about them. For these two instances, the skill effects are decisive for the argument; and they not only run in the predicted direction, they are also strong and significant according to Spenner's analysis (line 7, panels E and G). For panel F, "extent of discussion of national news," the three-way interaction effect is small due partly to the Argentine deviation. If we combine Italy and Argentina as a single case of intermediary development, the three-way interaction would approach statistical significance. In summary, of the eight three-way interaction effects Spenner examined, only two failed to meet both his criteria and mine, two met his criteria, two met my criteria for the contextual problem and one approached significance when the middle cases were collapsed.

That these findings constitute a mixed bag is important because Spenner selected two panels from Table 4 (Form, 1973) but omitted two others: "meets with fellow workers in community organizations" and "quality of interaction with fellow workers." When I subjected these two to Goodman's technique,

decomposition of the relevant associations, especially the three-way interaction effect in line 3 of Tables 1.A and 1.B, shows significant and strong effects accounting for 33 and 20 percent of the associations, respectively.

In addition, Spenner ignored other data in the article which support the hypothesis concerning the internal stratification of the working class. Three panels of my Table 1 illustrate this point. For "community problems identified" and "discusses political and economic issues with fellow workers" (panels D and E of Table 1), use of Goodman's technique shows that over a quarter of the association is due to the three-way interaction effect. Finally, Spenner fails to analyze data on union involvement, important for the hypothesis that skilled workers belong to cohesive work groups which influence union policies. Panel C of Table 1 shows strong three-way interaction effects on union involvement, accounting for 40 percent of the association by Goodman's tests.

Adequate evaluation of such a large research project requires more extensive data analysis or at least an explanation of why some data are analyzed and not others. Combining Spenner's analysis and mine tends to support the hypothesis concerning the internal stratification of the working class. A good illustration is our use of lambda (λ), a measure of effect of three-way interaction proposed by Goodman. Spenner's interpretation of the lambdas for significant three-way interaction is guarded. My Table 2 presents lambda values of items in my Table 1; they are generally larger, reach higher levels of significance and more of the signs are in the predicted direction that those which Spenner analyzed to support his critique. Combining his lambda analysis and mine warrants the conclusion of my article: "Data from this study suggest that the internal stratification of the working class may be increasing" (Form, 1973:709).

4. Spenner failed to consider problems of data interpretation. First, the tables in Form (1973) included only one of the columns drawn from the original tables. A single column was used for the sake of consistency although it represented some nations better than others. But I used the full tables to interpret the data. Working only with column residuals raises questions of data reliability, a risk which can be reduced by using all of the original data – which, like the other data, were available to Spenner on request.

Second, Spenner seemed less interested in the question under study than in the application of a statistical device. Thus, since Italy and Argentina differ slightly in industrial development, differences in workers' attributes such as level of education might affect participation rates. National differences could easily

Table 2. Lambda Values for the Three-Way Interaction

	OLDS	FIAT	IKA	PAL
Meets with Fellow Workers in Community Organizations				
Unskilled	-.451*	.147	.093	.211**
Semiskilled	-.296*	.142	.098	.056
Skilled	.748*	-.289*	-.191**	-.267*
Quality of Interaction				
Unskilled	-.166**	-.030	.117	.079
Semiskilled	.143**	.052	-.154*	-.041
Skilled	.023	-.021	-.037	-.039
Union Opinion Favorable				
Unskilled	-.217**	-.065	.157**	.125
Semiskilled	-.233*	.138**	-.092	.187*
Skilled	.449*	-.072	-.064	-.313*
Discusses Political and Economic Issues with Fellow Workers				
Unskilled	-.270*	.015	-.095	.350*
Semiskilled	.078	.096	-.212*	.038
Skilled	.192**	-.110	.307*	-.388*

* $p < .05$.
** $p < .10$.

be reduced by treating both countries as a single case of intermediary development, thereby strengthening evidence of three-way interaction effect.

Third, since Spenner did not participate in the study, he can not be expected to account for deviant findings. Thus, the suspension of elections in Argentina lowered political participation rates; multiunionism in Italy reduced union participation rates; threat of war with China increased awareness of national problems in India. The use of such variables reduces three-way interaction effects, but not effects due to skill. National and situational exigencies must be taken into account in data interpretation. It is risky to apply a statistical test in routine fashion and conclude the author is wrong.

To conclude: I am enormously greateful to Spenner for calling to our attention a powerful tool for testing three-way effects. But I do think we have both engaged in an academic game: he found weaker support for my hypothesis and I found stronger support. Obviously, entire studies must be designed and executed using the best quantitative techniques available without ignoring the fact that interpretation requires understanding aspects of social organization not yet under quantitative control.

William H. Form
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ITEMS (*Continued*)

research on the effect of industrial contexts, sex and race differences and migration on occupational achievements and the effects of family origin on female college graduates' sex role choices.

■ **IVAN CHASE** (Men's and Women's Intergenerational Mobility) is a Visiting Scholar in Sociology at Dartmouth College. His primary research interest is social inequality. A recent article, "Models of Hierarchy Formation in Animal Societies," in *Behavioral Science,* challenges the view that infra-human hierarchies are based on differences in individual characteristics, such as aggressiveness and strength, exploring the alternative that sociological processes may be a more fruitful explanation.

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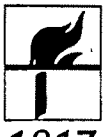

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Volume 40 Number 5

Hunter
Loss of Community

Hochschild
Disengagement Theory

Danzger
Validating Conflict Data

Hauser, Dickinson, Travis & Koffel
Structural Changes in Occupational Mobility

Meyer
Organizational Domains

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AMERICAN SOCIOLOGICAL REVIEW

Official Journal of the American Sociological Association

OCTOBER, 1975 VOLUME 40 NUMBER 5

ARTICLES

The Loss of Community: An Empirical Test through Replication ..... Albert Hunter 537
Disengagement Theory: A Critique and Proposal ..... Arlie Russell Hochschild 553
Validating Conflict Data ..... M. Herbert Danzger 570
Structural Changes in Occupational Mobility among Men in the United States ..... Robert M. Hauser, Peter J. Dickinson, Harry P. Travis and John N. Koffel 585
Organizational Domains ..... Marshall W. Meyer 599
Organizational Structure and Voluntary Participation in Collective-Good Decisions. ..... Anne M. McMahon and Santo F. Camilleri 616
Occupations, Labor Markets and the Process of Wage Attainment ..... Ross M. Stolzenberg 645

COMMENTS AND REPLIES

Comment on Cohen, Hazelrigg and Pope (ASR April, 1975)
On "De-Parsonizing Weber" ..... Talcott Parsons 666
Reply to Parsons ..... Jere Cohen, Lawrence E. Hazelrigg and Whitney Pope 670
Comments on van den Berghe (ASR December, 1974)
The Myth of "Territoriality" in van den Berghe's "Bringing Beasts Back In" ..... Claude S. Fischer 674
A Comment on van den Berghe's Biosocial Model of Aggression ..... Dennis J. Moberg 676
Cross-Species Comparisons of Aggression ..... Allan Mazur 677
Reply to Fischer, Moberg and Mazur ..... Pierre L. van den Berghe 678
Comment on Acock and DeFleur (ASR December, 1972)
Contingent Consistency and the Attitude-Behavior Relationship: A Comment ..... Charles E. Susmilch, Gregory C. Elliott and Shalom H. Schwartz 682
Reply to Susmilch, Elliott and Schwartz ..... Alan C. Acock and Melvin L. DeFleur 687

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ITEMS

October, 1975, ASR

▪ **ALBERT D. HUNTER** (Loss of Community) is Associate Professor of Sociology at the University of Rochester. His current research includes a case study of the effects of metropolitan ecological structure and change on suburban politics and a replication study (from which this article was taken) of twenty-five years of metropolitan change on an inner-city neighborhood, a suburb and a small town. His most recent book is *Symbolic Communities: The Persistence and Change of Chicago's Local Communities*, Chicago: University of Chicago Press, 1974.

▪ **ARLIE RUSSELL HOCHSCHILD** (Disengagement Theory) is Assistant Professor of Sociology at the University of California, Berkeley. Her current research focuses on the sociology of feeling and emotion. *The Unexpected Community*, Englewood Cliffs, N.J., Prentice-Hall, 1973 is her most recent book.

▪ **M. HERBERT DANZGER** (Validating Conflict Data) is the Chairman of the Department of Sociology at Herbert H. Lehman College and Director of the Graduate Center, City University of New York. He is presently completing a study of the Civil Rights Movement between 1955 and 1965 and, while on leave from Lehman College, teaching as Fulbright Scholar at the Hebrew University in Jerusalem, is engaged in a study of the religious elite in the community power structure in Israel.

▪ **ROBERT M. HAUSER** (Structural Changes in Occupational Mobility) is Professor of Sociology at the University of Wisconsin-Madison. The present collaboration grows out of his work with David L. Featherman on a 1973 replicate of the 1962 Blau-Duncan survey of occupational mobility in the U.S. He expects to test the ideas and methods developed here in comparisons between the 1962 and 1973 national surveys and, eventually, in cross-national surveys. **PETER J. DICKINSON** is a Project Specialist on the "Occupational Changes in a

(Continued on page 690)

AMERICAN SOCIOLOGICAL REVIEW

OCTOBER, 1975 VOLUME 40, NO. 5

THE LOSS OF COMMUNITY: AN EMPIRICAL TEST THROUGH REPLICATION*

ALBERT HUNTER

The University of Rochester

American Sociological Review 1975, Vol. 40 (October):537-552

This survey and field observation study replicates Donald Foley's Neighbors or Urbanites? *(1952) in the same urban neighborhood twenty-five years later to test the dynamic hypothesized "loss of community" in urban life. Three indexes reflecting three dimensions of community were explored. "Local facility use" declined, "informal neighboring" showed no change, while "sense of community" increased. The latter two did not decline because the area has attracted residents who economically and ideologically "value" the changes which have occurred in the area and the resulting "ecological niche" which the area has come to occupy. It is middle-class, racially integrated and urban. Residents have consciously sought out this area because of these characteristics and have consciously attempted to create community in part through an active local community organization. Drawing upon Mannheim's distinction between utopia and ideology, the area is defined as a consciously created "ideological community."*

Twenty-five years ago, Donald L. Foley explored empirically urban residents' "sense of community" in his study *Neighbors or Urbanites?* He was concerned with establishing the degree to which city dwellers expressed a local neighborhood orientation versus a more cosmopolitan orientation. This concern grew out of a central hypothesis within urban sociology that the outcome of continuing urbanization in modern industrial society would be the loss or eclipse of the local territorial community as a meaningful unit of social organization. This hypothesized loss of community was itself expressed in the polar concepts of earlier "classical" theories of social change. These included Toennies' (1957) shift from *Gemeinschaft* to *Gesellschaft,* Durkheim's (1949) from mechanical to organic solidarity and Weber's (1958) from traditional-sacred to rational-secular social orders. A more recent interpretation of these polarities is found in Parsons' (1951) elaboration of the "pattern variables" which further details Wirth's (1957) discussion of the shift from primary to secondary relationships in urban life.

It was noteworthy therefore that the empirical studies of Foley and other researchers (Janowitz, 1967; Gans, 1962) began to document the persistence of local community orientations that required a reassessment of the often too simplistic reading given to the earlier hypothesized loss of community in urban settings. As Foley (1952:57) concludes:

*This research is a product of the Rochester Area Project and was funded by grants from the National Institute of Mental Health and The National Science Foundation. I wish to thank Diane van Lier and Andrea Gilbert for their research assistance, and Dean Harper and Thomas Smith for their helpful comments and suggestions. Sociology colleagues who are, or have been, residents of the area include Raymond Murphy, Klaus Roghmann, Danforth Ross, James Watson and Paul Williams. The above individuals are, of course, not responsible for the interpretations and conclusions which I have drawn, though their information and insights have been invaluable.

> Contemporary city life, ecologically viewed, involves an intricate balance between certain vestigial "local community" patterns and an array of "metropolitan" level functional-territorial relationships that have been emerging as phases of the urbanization process. Men do not, it seems, easily abandon the traditional norms by which community life has been lived in the past. But there are nevertheless very impelling features of the urbanization process. It is in the dynamics of this basic cross current of local residential vs. metropolitan drifts that today's urban life must be viewed and assessed.

New "hybrid" concepts emerged to explain the persistence of this "vestigial" unit of community including Reimer's (1951) concept of "floating-neighborhoods," Gans' (1967) discussion of "quasi-primary" relationships among suburban Levittowners, Suttles' (1968) "segmental social order" in Chicago's West Side slum, and Janowitz's (1967) and Greer's (1962) "community of limited liability."

Numerous studies proceeded to document the variation to be found within the metropolitan environment, with dependent variables ranging from rates of social participation (Bell, 1968) to psychological states of alienation and anomie (Mizurchi, 1969). However, the manifestations of the hypothesized decline of community were as numerous and general as the definition of community itself, which all too often was left implicit in such discussions. This also often meant that the hypothesized "loss of community" was itself not tested directly because (1) community itself was not the focus of analysis and (2) the studies did not explore directly the dynamics of community change over time.

Attempts to define "community" have met with only a very general consensus in the sociological literature (Hillery, 1955; 1968), and it may be that the very looseness of the concept is valuable in providing a common whetstone on which to sharpen the cutting edge of competing ideas. Nonetheless, the consensus which has emerged would suggest the following three somewhat heuristic theoretical dimensions (Hunter, 1974):

1. community as a functional spatial unit meeting sustenance needs;
2. community as a unit of patterned social interaction;
3. community as a cultural-symbolic unit of collective identity.

These dimensions are heuristic in that in his original study, Foley developed three empirical indexes to differentiate "local" versus "cosmopolitan" orientation whose conceptual referents I would suggest are the three dimensions of community stated above. Foley's corresponding indexes were "use of local facilities," "informal neighboring" and "sense of community."

These three indexes are not simply heuristic, however, for aspects of all three are to be found in the earlier classical statements of the hypothesized loss of community. Furthermore, the *dynamics* of the loss of community hypothesis would suggest that these three dimensions would decline in a causally interrelated manner. From Toennies, Weber and especially Durkheim one finds the argument that technological and organizational increases in scale would reduce the degree to which local residents would or could rely upon the local territorial community to meet their day-to-day functional needs. Technological changes in communication and transportation would affect the "space time ratio" (Greer, 1962) and increase the mobility of both people and ideas (Webber, 1970) while new forms of aspatial interest groups and "bureaucratization" (Stein, 1960) would come to transcend the local community as a meaningful and encompassing territorial unit. In short, one would expect a decline in the number and use of local community facilities.

These functional and ecological changes were seen to lead in turn to new forms of social interaction as the primary relationships of place and kin gave way to rational, individualistic encounters typified by market transactions. This is the hypothesized shift from primary to secondary relationships in the urban context or the move from informal personal relations to formal status encounters. This shifting form of social interaction and its relationship to the previously mentioned shift in the functional institutional realm is clearly spelled out in the following passage by Wirth (1957:54):

> Characteristically, urbanites meet one another in highly segmental roles. They are,

> to be sure, dependent upon more people for the satisfactions of their life-needs than are rural people and thus are associated with a greater number of organized groups, but they are less dependent upon particular persons, and their dependence upon others is confined to a highly fractionalized aspect of the other's round of activity. This is essentially what is meant by saying that the city is characterized by secondary rather than primary contacts.

[and he adds]

> Max Weber, in recognizing the social significance of this fact, pointed out that from a sociological point of view large numbers of inhabitants and density of settlement mean that the personal mutual acquaintanceship between the inhabitants which ordinarily inheres in a neighborhood is lacking.

In short, to the degree that the above processes of urban change have occurred, to that degree one would expect a decline in the primary relationship of informal neighboring.

The final outcome of these two processes was hypothesized to be the decline of the local territorial community as a meaningful symbolic unit in the social and psychological life-space of its residents. Having lost both functional institutions and sustaining primary social relationships, the local area would be transformed from meaningful symbolic place to occupied physical space. The collective identity, the "we-feeling" of community, would occur at larger scales of superseding allegiances and within non-territorially based interest groups. In terms of Foley's study therefore, one would expect cognitive identification of and affective identification with the local community—in short, "sense of community"—to decline.

The period of the past twenty-five years has not witnessed the abating of the hypothesized casual factors leading to a loss of community. The continuing urbanization of the American population on a metropolitan scale and the related continuing "decline" of inner-city neighborhoods is clearly evidenced in the ecological data from the inner-city neighborhood originally studied by Foley. From Table 1 one can see that this neighborhood has experienced a decline in population, a build-up in multiple family dwelling units and an increase in the proportion of black residents. One should note, however, that the socioeconomic variables have not declined significantly relative to the remainder of the city. In short, this is an urban neighborhood which over this twenty-five year period has begun to experience some of the ecological changes associated with inner-city living.

The dynamics of the ecological processes

Table 1. Ecological Change in Study Area and City of Rochester (1940/1970)

	Census Tracts in Study Area			City
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% Professional	13.3/16.6	20.6/20.2	9.4/13.4	9.3/13.4
% Under Five	5.4/7.6	3.6/10.1	4.9/9.3	5.6/9.4
% Over 65	7.8/20.1	12.7/10.8	10.5/16.9	8.2/13.6
% Black	0.0/4.5	0.0/27.3	.01/5.5	1.0/16.8
% Foreign Born	11.2/8.5	11.4/6.4	13.8/13.3	18.5/10.5
Median Income ($1,000)	3.9/12.5	3.3/10.9	3.1/9.2	3.1/10.0
Median Education (years)	10.1/12.2	10.5/11.9	8.7/10.8	8.6/11.1
% SFDU	60.4/52.0	60.5/53.4	58.8/40.4	46.1/42.7
Total Housing Units (100's)	13.1/13.8	13.1/14.3	12.0/13.3	939/1055
Total Population (100's)	43.1/38.6	44.8/44.4	42.4/35.9	3250/2962

referred to are now beginning to be studied with appropriate time-series data (Schnore and Jones, 1969; Hunter, 1974). However, the consequent social and cultural-symbolic changes of such ecological change still too often are only indirectly "tested" by synchronic survey and field studies at single points in time. This then was the purpose of the present study: to replicate Foley's study of twenty-five years ago—in the same inner-city neighborhood of Rochester, New York—to test the dynamic, causal propositions of the ecological, social and cultural-symbolic "loss of community."

Methods

This study is based upon data from a sample survey of residents and over two years of participant observation as a resident in the study area.

The survey is based upon a random two stage, areal sample of blocks and households (154) from the same study area initially delimited by Foley in 1948. In addition to the usual background items, the interview instrument contained a series of questions and response categories identical to those used by Foley. The particular items are to be found in Tables 2, 3 and 4. Identical procedures were used in constructing the three additive indexes of "local facility use," "informal neighboring" and "sense of community." The data were analyzed using sub-routines of *Statistical Package for the Social Sciences* (Nie et al., 1970).

In addition to the survey data, I will present data from observations and informal interviews made from over two years of field work in the area as a resident and "community actor." My own observations benefited from discussions with colleagues who are also residents of the area and from an unpublished master's thesis by Pamela Hecht (1973), "Differential Perceptions of a Local Community Area," based upon research in the same area.

Findings

Turning first to the hypothesized "functional" or institutional loss of community we see that the data are consistent with the hypothesis. Over the twenty-five year period, there has been a general decline in residents' use of facilities which are in close proximity to their homes (see Table 2). For six out of the seven activities, there has been a decline in the proportion of residents performing this function within five blocks of home, and five of these declines are statistically significant.

Local banking is the only functional activity which has increased at the local level, and this may be due in large part to the recent

Table 2. Local Facility Use Index

Percent performing functional activity within five blocks of home:	Foley - 1949 N = 437	Hunter - 1974 N = 154	
Grocery shopping	77.4%	34.7%	*
Small purchase	71.2	61.3	*
Church	61.5	51.2	*
Movie	49.5	9.1	*
Doctor visit	46.7	19.3	*
Banking	25.6	47.9	*
Employment	17.0	11.4	n.s.

* Significant difference T_1 vs. T_2 at .05 level

building of a drive-in branch bank in the area. This is one activity which has experienced "decentralization" or a reduction in territorial scale over this period. The declining use of the local area by residents for the remaining activities is consistent with the concept of "increasing scale." For example, a number of respondents volunteered that they travel a mile or more to a large shopping plaza located in a neighboring suburb to do most of their weekly grocery shopping. Also, on numerous corners throughout the residential area one can see vacant and boarded-up "Ma and Pa" stores that attest to the declining local scale of this activity.

The decline in movie attendance, though perhaps a general trend over this period, is also due to the closing of one theatre in the area and the specialization of the remaining one in X-rated, "porno-flicks" at the time of this study. In addition, there has been a widespread proliferation of theatres in suburban shopping plazas in the Rochester area. The fact that many of these theatres show two or even four films simultaneously in adjacent theatres exemplifies the increase in scale of this form of recreation.

The declining proportion of visits to a nearby physician may similarly be interpreted as a function of the changing organization and scale of medical practice. The solo neighborhood practitioner has been replaced by group practice, hospital based medical care and, in the case under study, the recent opening of a prepaid community medical clinic in the area.

The decline in use of the local area for small purchases and church attendance, though statistically significant, are of a lesser magnitude than those mentioned above. Employment in the local area was the only activity not to decline significantly, though it showed some decline. At both points in time, this was the activity least likely to be carried out in close proximity to the home, a finding consistent with the general pattern of home/work separation in American society.

Turning next to the hypothesized decline of the local community as a unit of primary social interaction, the results do not confirm the hypothesis. Of the six comparable items comprising the informal neighboring index, five show an increase in the proportion engaging in this informal relationship; and of these five, two are statistically significant. None of the items shows even a moderate decline. The two items at the bottom of Table 3 must be interpreted cautiously as slightly different questions and coding categories were used. The general picture, therefore, is that over this twenty-five year period—even though functional use of the local area has declined significantly—informal neighboring has not declined and may have increased slightly.

The hypothesized decline in the third dimension—"sense of community"—is even more strongly rejected. All five items measuring identification of and with the local community show that a greater proportion of today's residents have a "sense of community" than their predecessors of twenty-five years ago, and three of the five increases are statistically significant. The two items tapping the affective dimension of attachment have not increased as significantly as the two cognitive items of community "name" and community "boundaries," and the one social item of community oriented activities.

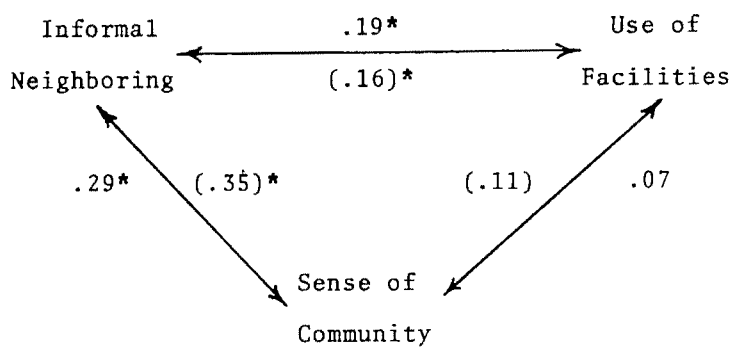
In summary, these findings show that over the past twenty-five years within this local area (1) there has been a statistically significant decline in functional use of the local area; (2) informal neighboring has not declined and may have increased slightly and (3) identification of and with the local area has significantly increased. Therefore, counter to the previously stated theories, we find that in spite of the functional and ecological "loss of community" the social and cultural-symbolic dimensions have not declined, and the latter has even increased.

The directions of these observed changes raise important questions about the nature of the causal relationships existing among the three indexes, questions which we will now explore.

In his original study, Foley intercorrelated the three indexes and in Figure 1 one can see that these "synchronic" intercorrelations remain relatively unchanged over this twenty-five year period. In both studies, the highest correlation is between "informal neighboring" and "sense of community," followed by a moderate but significant correlation between "local facility use" and "informal neighboring." Furthermore, in both studies the relationship between "local facility use" and "sense of community" remains statistically insignificant. These findings show, therefore, that in spite of the previously observed changes in the values of each of the indexes,

Foley - 1949 - Outside Line

(Hunter - 1974 - Inside Line)



* Significant .05 level

Figure 1. Zero-Order Correlation Coefficients among the Three Indexes

variable which is hypothesized to be affected by informal neighboring and local facility use. Informal neighboring in turn is hypothesized to be affected by antecedent functional and ecological changes measured by local facility use.

Six exogenous independent variables were selected to explore their effects upon these three indexes. These variables were selected because of their substantive significance as found in previous research and are posited as being causally prior to the three indexes. "Kids" (number of children at home) and "age" (age of respondent) were selected as measures of "life cycle" or "family status" (Greer, 1956; Bell, 1961; Keller, 1968) which has been found to have important implications for the degree of social and psychological orientation to local urban communities. "Education" (number of school years completed by respondent) and "homeowner" (home ownership—a "dummy variable" with 0

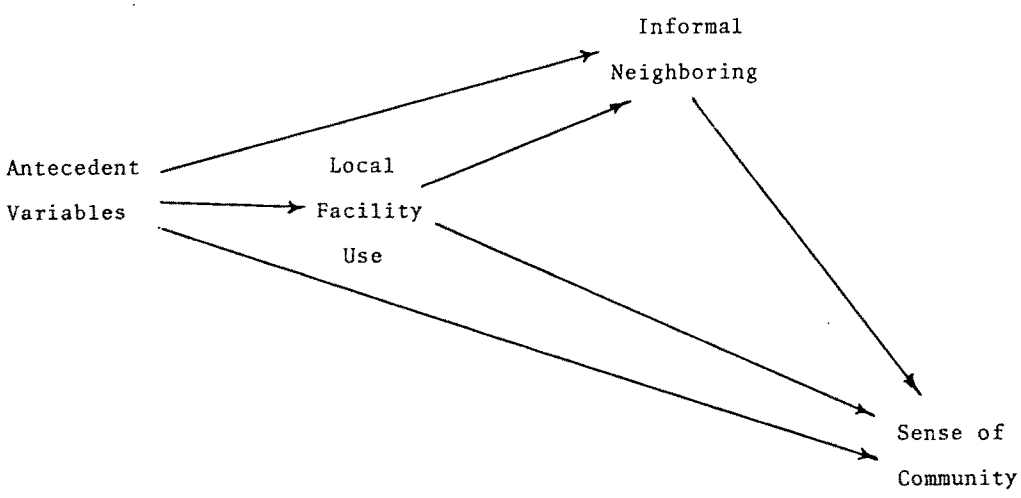


Figure 2. Causal Model of Indexes

= renter, 1 = owner) are taken as measures of "social class," and as the degree to which there is a direct "economic" investment within the local area (Janowitz, 1967; Gans, 1967). Race (also a dummy variable with 0 = black, 1 = white) is an obvious measure which has recently been studied in terms of its effects upon community orientation by Bradburn et al. (1970) in their national survey of *Racial Integration in American Neighborhoods.* "Years" (years of residence in the area) is a "summary" variable which has been found to be of critical importance in affecting local community orientations by Kasarda and Janowitz (1974) and Hunter (1974).

These variables were also selected because they are indicators of the three dimensions of residential segregation which have emerged in social area analysis or factorial ecology (Berry and Rees, 1969; Hunter, 1974). The three general dimensions of socioeconomic status, family or life-cycle status and racial-ethnic status correspond respectively to the following variables: (1) education and home ownership; (2) age and number of children and (3) white versus non-white. Given that these dimensions serve on the aggregate level to selectively distribute population throughout a metropolitan area, it was hypothesized that they would also affect local community orientations of individual residents within the study area (Bell, 1961; 1968).

The intercorrelations among these six independent variables are presented in Table 5, and the only possible problem of multi-colinearity is found with the moderately high correlation between "age" and "years," though even this is within usually accepted limits.

Turning to Table 6, we see that none of the six independent variables has a statistically significant effect upon local facility use. Informal neighboring, however, is affected by three of the variables and in addition is positively affected by the degree to which residents make use of local facilities. It appears that residents who are younger, white and homeowners are significantly more likely to engage in informal neighboring than older residents, blacks and renters.

Finally, we see that the degree to which residents of the area express a "sense of community" is significantly and positively related to their degree of informal neighboring. Again, we find that whether or not residents utilize the local facilities of their area does not affect the degree to which they express a sense of community. It is noteworthy that of the six independent variables, only education, which had negligible effects on facility use and informal neighboring, has a significant positive effect on sense of community.

Field Observations and Discussion

The observations and discussion will focus primarily upon this question: In the face of the previously stated theories and in light of the observed ecological changes occurring within this local urban community, why has the social and cultural symbolic sense of community not declined, but instead increased? The answer is that (1) community was consciously sought after and (2) consciously created by its local residents.

It was consciously sought after precisely because of the ecological changes and

Table 5. Zero-Order Correlations among Independent Variables

	Years	Age	Own (Own)	Race (White)	Kids
Education	-.14	-.19	-.03	.10	-.03
Years		.64	.25	.16	-.35
Age			.20	.30	-.32
Own (Own)				-.14	.10
Race (White)					-.16

Table 6. Standardized Multiple Regression Coefficients (Betas) of the Three Indexes upon Antecedent Variables

Antecedent Variables	Local Facility Use	Informal Neighboring	Sense of Community
Education	.04	.01	.17*
Years	.13	-.15	.04
Age	.16	-.22*	.12
Homeowner (Owner)	.14	.30*	.10
Race (White)	.16	.21*	.02
Kids	.02	.12	-.01
Local Facility Use		.20*	.03
Informal Neighboring			.36*
($I-R^2$)	(.862)	(.781)	(.798)

*F test significant at .05 level

characteristics of the local area and because of the particular "functional niche" which it occupies within the Rochester metropolitan area.

One may view a metropolitan area as a spatial matrix into which people will differentially distribute themselves by choice and by constraint in an attempt to maximize a constellation of values. Their capacity to realize certain values is dependent upon matching them to characteristics of given local communities in which they might reside. As Berry and Rees (1969:460) say:

> The inhabitants of the city are faced with a fundamental decision: where to live. The principal determinants of such a housing choice are three in number—the price of the dwelling unit (either in rental or in purchase value terms); its type; and its location, both within a neighborhood environment and relative to place of work. These determinants have parallels in the attributes of the individual making the housing choice: the amount he is prepared to pay for housing, which depends on his income; his housing needs, which depend on his marital status and family size, that is, his stage-in-life cycle; his life-style preferences, which will affect the type of neighbor he will want; and, finally, the location of his job. When the values of the two sets of characteristics match, a decision to purchase housing will be made.

The Search for Community

By exploring the motives and rationales given by residents for moving into, continuing to reside within or moving out of this particular area, we may begin to understand why social and symbolic community has persisted and even increased in this area over the past twenty-five years.

One of the first reasons people give for moving into the area is the economic housing value found in this community. It is predominantly an area of older, single family homes where simply put—"you get a lot of house for the money." Compared to housing in middle-class suburbs—a comparison which we will see many residents consciously make—homes in the area are a bargain. There are other areas in the city and lower-middle-class and working-class suburbs where one could find equivalent housing for similar cost, but these are simply not the comparative "reference communities" for the middle-class residents of the area.

Second, there is a very conscious rejection of suburbia, or rather a conscious rejection of the somewhat sterotyped "image" of suburbia (Strauss, 1960) by residents in the area, and a correspondingly positive assertion of the values of "urban living." To borrow from Charles Abrams (1967), there is a view that "the city is the frontier," and that not only is one living "where the action is" (Goffman, 1967) but that one is ideologically and personally countering the general decay of urban America. From this perspective, suburbia is seen as a retreat from the reality of major social problems facing American society; and residents who for one reason or another decide to move from the area are defined by others and to a degree themselves as "selling out"—not simply a house—but an ideology and a movement.

A third major reason given for moving into the area is the fact that it is seen as a stable, racially integrated community. The distinction between a stable, integrated neighborhood versus one that is simply integrated at a given point in the succession from white to black is dependent upon the ability of such areas to continue to attract whites to move into them. This community, like so many other similar areas throughout urban America (Molotch, 1972), is consciously advertising itself as a stable, racially integrated area in an attempt to attract white residents who view this characteristic as a positive ideological value or ideal. Many of the middle-class white residents of the area proclaim the fact that they are living the value of racial integration while many of their "liberal" friends from suburbia hypocritically espouse one ideal and live a different reality.

For the middle-class blacks living and moving into the area, there is a more conscious move out of the ghetto—but coupled with a positive assertion of racially integrated living. Some of the blacks, as well as a few whites, have moved back to the area after having lived in neighboring suburbs.

Finally, for a few families the value of living in a racially integrated neighborhood is particularly poignant as they are living in racially integrated families. There are a number of racially mixed couples and white parents who have adopted black children for whom the matching of family and neighborhood racial characteristics makes this community uniquely ideal.

A fourth major reason given by some residents for moving into the area is the institutional proximity of the University of Rochester and its affiliated large medical complex. The University and the hospitals are a source of employment and education for many residents, and they are countering the home/work separation and commuting pattern of suburbia by reestablishing home/work proximity. Reduced reliance on the automobile and the capacity to "walk to work" are seen as especially valuable by the increasing number of two career families who live in the area.

Finally, the move to this area was for some precisely the fact that it was seen to be a meaningful social and symbolic "community." The search for community, in short, has become a *conscious* search and the prior proselytizing done by existing residents are apparently sufficient to convert them into fellow residents.

However, the area is not solely composed of ideologically oriented newcomers. At the time of Foley's study, the area was a middle- and lower-middle-class white area with a large ethnic population, the largest single group being Irish. (See Table 1, census tract 75.) Over the intervening years, this population has aged and a large number of these residents have either moved or died. However, a sizable proportion continue to reside in the area. Their reasons given for staying tend to be more personal and home oriented, and they stay *in spite of* the changes which have occurred in the area in the past quarter century. Many are "trapped" in the sense that they own their homes outright and financially could not afford to move. However, this rationale is often coupled with a statement of the social and symbolic significance of the area and especially the house—as the place in which their family was raised—in short, it is home. More than once during the interviews, elderly couples would haul down from the mantel or pick up a public display of photographs of children and grandchildren who had at one time lived in the house, played in the neighborhood and graduated from local schools and who still returned for visits and holidays. For these individuals, the local community is a symbolic community of the past, but one which they are at times actively and publicly trying to preserve and maintain.

There are residents who do move from the area and many more who have at one time or another contemplated moving from the community. For many of the residents it appears there are two primary reasons—safety and the schools which are interrelated in the common concern for the welfare of children. Borrowing from Wendell Bell's typology (1968), we can say that coupled with the "career" orientation of many residents of the area is a persistently strong "family" orientation which has been the traditional argument for the movement to suburbia.

Crime, especially in the form of house burglaries and store robberies, has increased in the area. Given the density of social ties and informal and formal communication links in the form of a local community press (Janowitz, 1967) and a local community organization (Hunter, 1974), these events become fairly quickly transmitted throughout the area. Furthermore, for those with a strong community orientation the use of "the house as a haven" from fear (Rainwater, 1966) is anathema to their "public" and "pedestrian" values.

The schools were also given by many residents as a primary problem which was prompting them to consider moving from the area. Especially for the educated and middle-class residents who place a high value on "quality education" this becomes a problem at the larger scale of the high school which draws in a larger proportion of blacks and lower-class pupils than do the local grammar schools. It should be noted that such "quality education" is one of the more common rationales given for the traditional move to suburbia, and especially to the comparable, middle-class, reference suburbs for residents of the area. Aside from moving to the suburbs, a possible solution which a number of residents are choosing is to send their children to private schools. This solution is, however, costly; but its cost is often consciously weighed against both the ideological and economic costs of a suburban move.

In summary, I am suggesting that the ecological changes occurring in this local community over the past twenty-five years have resulted in its coming to occupy a particular and somewhat unique ecological niche within the Rochester metropolitan area. It is middle-class, racially integrated and urban; and therefore, it has attracted residents whose values—ideological as well as economic—match those of the local community. Though such an ecological niche may be relatively unique within a given metropolitan area, there are findings which suggest that it is a nationally occurring phenomenon (Bradburn et al., 1970). Whether the phenomenon is unique to this historical period or whether it merely represents a more general but temporary phase in the overall pattern of neighborhood succession from white to black, from middle-class to lower-class, time and further research will determine.

The Creation of Community

Not only have many of the residents of the area consciously selected the area because of its ecological and "community" characteristics—but they have also been involved in creating and mantaining a more formal structural embodiment of community—a local community organization—the Nineteenth Ward Community Association.

This local community organization was founded in the early 1960s as a community response to two pressing "ecological" problems: (1) noise and air pollution from the neighboring county airport and (2) "block busting" in the area by local realtors as the area first began to experience racial change. The organization's standing Airport Committee and Real Estate Committee point to the persistence of these problems since that time. The Real Estate Committee operates with the same goal, and by similar procedures, as the South Shore Commission in the South Shore community of Chicago studied by Molotch (1972). It attempts to attract white homeowners into the area by providing a free listing service of homes for sale which potential residents are shown by volunteer members of the committee. Often, therefore, a resident's first contact with the community is through the local community organization.

In addition to the more formal activities such as continuous surveillance for zoning violations and the distribution of a local community newsletter, the association also sponsors a number of social and community activities which in an earlier era were often carried out by other institutions such as the church, school and local political organizations. For example, the annual "town

meeting" is dutifully attended by local politicians and the local congressman. The association also holds an annual Community Citizen of the Year Award Banquet, a day-long summer fair at which local "artisans" sell wares and an annual Thanksgiving communal dinner held in the basement of one of the local churches. In short, the association has come to be for many residents the formal embodiment of local community activities and local community sentiment which in a previous era were often provided by other institutions.

To explore the effect of membership in the local organization upon the three indexes of local facility use, informal neighboring and sense of community, we introduced it into a series of regression equations as an antecedent, independent variable which it appears to be for many residents.

One can see that those residents who are somewhat older, homeowners, more highly educated and have lived in the area for a *shorter* period of time are more likely to belong to the organization. We also see that such membership does not affect "local facility use," but it does increase significantly the degree of informal neighboring among residents. Finally, belonging to the organization significantly increases the sense of community for members, though informal neighboring remains the most significant in its effect. Two additional points should be noted. First, when one "controls for" belonging to the organization, education no longer has a significant direct effect upon sense of community. Second, informal neighboring and belonging to the organization, the two variables having the most effect upon a sense of community, appear to be two somewhat distinct modes of social interaction for two somewhat different types of residents. Informal neighboring is found among younger, white homeowners, while membership in the local community organization is found among older, educated homeowners who are newcomers to the area. In short, it appears that the local community organization draws its membership from those residents who value the organization's ability to create, in part, a sense of community.

The rise of such local community organizations and their role in community development in the 1960s was, of course, not an

Table 7. Standardized Multiple Regression Coefficients (Betas) of Belonging to Community Organization and the Three Indexes upon Antecedent Variables

Antecedent Variables	Belong to Community Organization	Local Facility Use	Informal Neighboring	Sense of Community
Education	.17*	.03	-.02	.13
Years	-.25*	.14	-.10	.08
Age	.24*	.15	-.26*	.06
Homeowner (Owner)	.30*	.12	.24*	.04
Race (White)	.13	.16	.19*	.00
Kids	-.07	.02	.14	.01
Belong to Organization		.04	.20*	.21*
Facility Use			.19*	.03
Informal Neighboring				.32*
($1-R^2$)	(.835)	(.860)	(.749)	(.762)

*F test significant .05 level

isolated phenomenon. However, the relative uniqueness of this local community organization within the Rochester metropolitan area and, at the same time, its general similarity to other groups throughout the country is evidenced by its membership in National Neighbors, a federated nationwide association composed of local community organizations operating in racially integrated neighborhoods. This national federation, based in Philadelphia and with over one hundred member organizations, began as an information clearing house sharing various communities' attempts and strategies to maintain their racially integrated status. More recently, the federation has moved into the political arena as a legislative pressure group for "fair housing" practices. Therefore, though the Nineteenth Ward and similar communities may be relatively unique within a given metropolitan area, there is no doubt that they represent an organized and nationally occurring phenomenon.

Summary and Conclusions

In summary we have found that the dynamics of the hypothesized "loss of community" must be modified, or at least qualified, within this local community area initially studied by Donald Foley twenty-five years ago. Consistent with the hypothesis of an increasing scale leading to an ecological and institutional eclipse of the local community as a functional territorial unit, we found the use of local facilities within this area has significantly declined over this twenty-five year period. However, the related social-interactional and cultural-symbolic dimensions of community have not shown a corresponding decline. The degree of informal neighboring has remained relatively constant while the sense of community has increased. It appears that the hypothesized consequences of an ecological and functional increase in scale have not resulted in a social and cultural-symbolic loss of community in this area.

In addition, we found that the relationships (correlations) among these three indexes have remained relatively constant over this twenty-five year period. In further exploring the nature of these relationships, we posited a causal model consistent with previous theories and found that use of local facilities had no significant independent effect upon sense of community but it did have a positive independent effect upon informal neighboring. Informal neighboring in turn was found to have a significant effect upon sense of community. When we later introduced belonging to the local community organization into the model as an antecedent variable, we found that it, too, had a significant independent effect upon sense of community, but that membership in the organization and informal neighboring appear to be two distinct modes of local participation used by two somewhat different types of residents. Informal neighboring appears to occur among younger, white homeowners; while homeowners who are somewhat older, more highly educated and *newcomers* to the area are more likely to belong to the local community organization.

In trying to explain these observed changes and relationships, I drew upon over two years of participant observation in the area as a resident and analyzed the motives which residents give for moving in and out of the area in their attempts to maximize or match personal values in relationship to the demographic characteristics and ecological niche of this given local community.

Several of the values which residents gave were traditional economic and other motives consistent with previous research on housing choice—housing value, single family homes consonant with a family oriented life style and a concern about journey to work which a number of residents had "minimized" by moving to the area in part because of its close proximity to the University of Rochester. However, three very consciously articulated motives were given for moving to the area which can be seen as values that generally run counter to the dominant reality of present-day American society: (1) a conscious rejection of suburbia and a positive commitment to urban living; (2) a conscious move to a racially integrated residential area and (3) a conscious search for a residential area which was seen to be a community. These values may be seen to have grown in part out of the social problems and social movements of the 1960s.

Furthermore, we found that this conscious "social construction of community" (Suttles, 1972) has led to the creation and maintenance of a local community organization whose

structure and activities are mechanisms which heighten both the social and symbolic "sense of community" for local residents.

In conclusion, it appears that we are dealing with individuals who are attempting to live a community existence based upon "transcendent values"; that is, values which are counter to existing reality. While most Americans are choosing to move from the city to relatively segregated suburban communities with their home/work separation, these individuals are espousing and living a counter set of values. However, the result is not the creation of a "utopian community" as recently studied by Rosabeth Kanter (1972), for these residents are not espousing an all encompassing belief system nor attempting to live in an autonomous and isolated social system. Rather they have selected a limited set of values which they are attempting to realize within the context of ongoing American society. That is, they are continuing to pursue careers, raise families and, in general, participate in most aspects of middle-class American life. Therefore, to draw upon Mannheim's (1936) distinction between utopia and ideology, these individuals are not living in an utopian community, but rather they have created or are attempting to create a community based upon a "partial ideology." Utopia and ideology are considered by Mannheim as two types of "transcendent belief systems"—and although utopia is usually associated with "community" as a type or form of social structure while ideology is more commonly associated with "social movements," it appears nonetheless that these individuals are attempting to live within a consciously created "ideological community." It is a partial ideology in which "community" itself is one of the central values of the ideology. Similar to Weber's (1958) process of "the routinization of charisma" in the transformation of supporting beliefs of authority from social movements to established formal organizations, we are dealing here with a process which appears to be the transformation of partial ideologies from social movements to communities, or the "communalization of ideology."

Though this community may exemplify only a limited specification of the more general "loss of community" thesis, the indications that it is a nationally occurring phenomenon suggest that emergent and perhaps persistent counter forces do exist for the creation and maintenance of local community solidarity. The prevalence of "ideological communities" around major institutions located in older areas of central cities—such as medical complexes and universities—though relatively unique within a given metropolitan area may be sufficiently general nationwide to provide a limited but persistent set of counter values to "the loss of community" in urban settings. Though it differs in form and content from the "utopian community" with its total commitment, the more limited "ideological community" as a partial counter force may prove to be more prevalent and persistent.

It remains to be seen whether or not that ultimate distinction which Mannheim draws between utopia and ideology will be realized—the success of the former and the failure of the latter. As he says (1936:194):

		Type of Transcendent Belief System	
		Utopia	Ideology
Type of Social Structure	Social Movement	Communism	Civil Rights Movement
	Community	Oneida	Nineteenth Ward

Figure 3. Belief Systems and Social Structure

Ideologies are the situationally transcendent ideas which never succeed *de facto* in the realization of their projected contents. Though they often become the good-intentioned motives for the subjective conduct of the individual, when they are actually embodied in practice their meanings are most frequently distorted. . . .To live consistently, in the light of Christian brotherly love, in a society which is not organized on the same principle is impossible. The individual in his personal conduct is always compelled—insofar as he does not resort to breaking up the existing social structure—to fall short of his own nobler motives.

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DISENGAGEMENT THEORY: A CRITIQUE AND PROPOSAL*

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This article examines the central theory in the sociology of aging–disengagement theory. It asks why the last decade of research bearing on it has been so inconclusive. The answer, the author suggests, lies (a) in the overlooked flaws in its underlying logic–the escape clause problem, (b) in the overly inclusive nature of its central variables–the omnibus variable problem and (c) in the level of reality it selects for study–the assumption of meaning problem. These three problems appear not only in disengagement theory but in much of the later research bearing on it. In an attempt to avoid these problems, the author sketches an alternative theory. This theory redefines disengagement and proposes new structural determinants of it.

The beginning of systematic theory in a new field is often marked by the publication of some global theory which sets up a central question, makes a good try at answering it and simplifies reality in some useful way. This is often followed by a period of critical reaction, which is followed in turn by a third stage of trying to develop a general theory again. In the field of aging, we seem to be in the middle of the second stage. Our global theory was Cumming and Henry's now classic disengagement theory published in 1961.[1] Throughout the 1960s, there was a plethora of research reports criticizing and confirming this or that part of the theory, disassembling the variables, suggesting alternative explanations and testing it on other populations. Now the question is: where do we go from here?[2]

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[1] Historically, disengagement theory evolved from an article published in 1960 (Cumming et al., 1960), later developed in *Growing Old* (1961) by Elaine Cumming and William Henry. Twice revised, once by Cumming (1963) and once by Henry (1964), the theory has attracted several replications, and many bodies of data have been linked to various parts of the theory. As various review articles point out (Streib, 1968; Maddox, 1969; Koller, 1969; Youmans, 1969), critics often disagree not only with the theory, but with each other, thus posing a secondary problem of reconciling the different criticisms with each other. For those readers who are not familiar with disengagement theory, the outline is briefly this: growing old involves a gradual and "inevitable mutual withdrawal or disengagement, resulting in decreased interaction between an aging person and others in the social systems he belongs to" (1961:14). In every culture and historical period, the society and individual prepare in advance for the ultimate disengagement of death by an inevitable, gradual and mutually satisfying process of social disengagement prior to death. This is a double withdrawal–of the individual from society and of the society from the individual. On one hand the individual "wants" to disengage and does so by reducing the number of roles he plays, lessening the variety of roles and relationships and weakening the intensity of those that remain. The remaining relationships are, in addition, qualitatively different (i.e., more socio-emotional and expressive). On its side, society offers the individual freedom from structural constraints and "permission" to withdraw. Once set in motion, the process is irreversible. The individual retreats from the social world, which in turn relieves him of normative control. He becomes de-socialized. This insures the linear progression of disengagement. This process is functional for the individual in the sense that it goes with having a high morale, and it is functional for society in that it retires an age echelon from roles which young people may then fill. The process is universal and inevitable, although variations in timing and style occur according to the individual's physiology, personality, initial type of engagement and life situation.

[2] To this day the assessors disagree. In the late 1960s, we find Maddox (1969) saying, "In balance, disengagement theory has been found wanting empirically and its original formulation is rarely

If we are not to start in the wilderness again with new beginnings detached from past efforts, we must take a hard look at the theory with an eye to the following fundamental questions: (a) Did the theory pose an important question? (b) Was the theory logically constructed in a tight and consistent way? (c) Are the borders of its categories drawn in an analytically useful way? (d) What are the heuristic assumptions which determine the level of reality that is to be the domain of social gerontology? I shall try to answer these four questions before, in the final section of this essay, sketching an alternative theory addressed to the same question that Cumming and Henry have raised.

THE BASIC QUESTION

The theorists addressed themselves to the basic question: how is age related to engagement? This important question was at the time a new one. It represents a social gerontological approach to the question of social order and suggests such further questions as: how does the relation of age to engagement vary between one society and another, between one social class and another or between one sex and the other? The basic question has become a point of focus for theories of age stratification and could become one for gerontological social psychology and developmental theory.

I do not think Cumming and Henry answer the question they pose altogether successfully for logical reasons, as in the escape clause problem, theoretical reasons, as in the omnibus variable problem,[3] and heuristic reasons, as in the assumption of meaning problem. I will address these problems in the theory and in the research which follows. There are methodological problems in much of the follow-up research, especially in the confounding of age and cohort effects in the cross-sectional studies, but this essay will not discuss those problems.

The Escape Clause Problem

One problem with the answer disengagement theorists offer to this question lies in the very logic of the theory.[4] The theory is unfalsifiable. Let me start with one of the most important and controversial series of propositions in the theory: that disengagement is universal, which I take to mean that it happens everywhere and in all historical eras;[5] it is inevitable, which I take to mean that it must happen sometime in the individual's future if it is not happening now; it is also intrinsic, which I take to mean that social factors alone do not cause it. These claims have come under vigorous attack throughout the 1960s (e.g., Desroches and Kaiman, 1964; Maddox, 1969; Prasad, 1964; Rose and Peterson, 1965; Tunstall, 1966; Videbeck and Knox, 1965; Youmans, 1967; Brehm, 1968; Kutner, 1962). Curiously, the attack has focused on the *substance* of the claim. It has not focused on the *logical* connection of the above claims to other more flexible propositions presented elsewhere in the theory; namely that there will be variations in the "form" and "timing" of disengagement. When we put these two sets of propositions together, the question becomes: what about disengagement is constant (i.e., universal, inevitable and intrinsic) and what about it is variable (i.e., form and timing). Unless this is specified—and it is not specified

defended by anyone," while, on the other hand, Streib (1968:76) defends the claim to universality (the usual target of critics of the theory) and concludes that the theory "does have value in understanding aging and the aged in settings very different from that in which the theory was originally developed."

[3] Cumming and Henry do not discuss power and the word "power" does not appear in the index. They focus on the extent to which old people are normatively controlled but not on whether or how they control others. The lack of power of old people, especially the retired and widowed, becomes in their theory a parameter, not an independent variable, and the low status is an unspoken assumption underlying the notion of a syntonic relation between the old person and society.

[4] I shall refer to the theory in its latest, revised version although this is not the version to which much of the research of the 1960s addresses itself. Much of the latter is not really a reaction to disengagement theory but is, rather, two or three prefatory paragraphs attached to on-going research. The operationalization of the concepts and sometimes the concepts themselves differ from the original theory. Actual replications often apply only to fragments of the theory.

[5] Cumming (1963) in her revision of the theory ignores the universality claim while Henry (1964) defends it.

in the theory—there is an escape clause problem.

The escape clause problem is essentially this: what would constitute *counter*-evidence? Even in the Kansas City sample, not *all* very old people were disengaged: 22% of those between the ages of 70 and 74 had "a large number of roles." Over a third (34%) had "high daily interaction with others" and nearly a fifth (18%) had a "large life space" (1961:40), while 16% did not perceive their lifespace as constricted (1961:97).[6] Since the data is cross-sectional, we do not know how these particular people would have come out on these measures 20 years ago. We do know it would be hard for them to have been even more active formerly than they are now.

In *Growing Old*, there are four types of "back door" explanations which prevent these anomalous older people from being counted as evidence against the theory. First, old engagers are said to be "unsuccessful" disengagers. They have not "achieved" the final psychological stages of "fixed conformity" or "ego defect" for men and "internalized rigidity" and "externalized ego defect" for women (1961:120, 125). According to the theorists, these older engaged people are not evidence of the lack of universality, but evidence bearing on "success" defined as it is in this unique way. Either a person is interpreted as a *successful* disengager or an *unsuccessful* disengager. The possibility is overlooked that unsuccessful disengagers might just as well be considered engaged.

Second, the older engager can be said to be off on his or her "timing."[7] Again, as their data show, a higher proportion of women in their sixties actually have a larger "life space" than do those in their fifties (1961:40). Here one is either a disengager on time or a disengager off time, but in either case, these women are "on the way" to disengagement.[8] Third, the old engager can be explained away as a member of "a biological and possibly pyschological elite."[9] Bertrand Russell appears in the formal statement of the theory (1961:215) as an example of re-engagement as if his case did not bear on the statement that disengagement is intrinsic, inevitable and universal. However, if we are going to talk about exceptional individuals, we might as well also entertain the possibility of "exceptional" opportunity structures (Atchley, 1971; Roman and Taietz, 1967).

If counter-evidence cannot exit from any of these three back doors, it has a fourth option of being an example of "variation in the form" of disengagement. As the authors put it, "The concept of disengagement is culture-free, but the form it takes will always be culture-bound." (Cumming and Henry, 1961:218). The question of form is, in turn, related to the omnibus variable problem. For example, like most studies which claim to refute disengagement theory, the three-nation study of 2500 older people in the United States, Great Britain and Denmark offered evidence of continual *social* engagement. On none of their many measures of social contact

[6] On the psychological measures, 5.9% of the men aged 64-70 fell into the "engaged" pattern of "focused active mastery" whereas 17% of the women fell into the "externalized personal mastery" pattern, the state said to be roughly corresponding for women.

[7] Although it does not appear in the formal statement of the theory, Cumming and Henry (1961:202) actually describe regularities in time variation: "Our findings . . . suggest that, following a plateau in the late forties and through the fifties, there is a crisis, marked by anxiety, between sixty and sixty-five. After this, most people become more contented. Again, in the seventies, there is a restlessness and irritability that, if not resolved by death, may lead, for the special few survivors, to a period of tranquility and satisfaction in very old age."

[8] In any case, as we shall see, not all of the many dimensions of disengagement need have the same "timing." Ego energy could be hypothesized to obey one schedule, affectivity another, self absorption yet another and social role loss or social construction still other schedules. Just as all the dimensions of the omnibus variable, disengagement may not exist together, so too, they may not occur at the same time.

[9] As the authors put it, "Many observers have commented on the special qualities of the very old. Riesman draws attention to Bertrand Russell and Toscanini; others talk of Bernard Baruch and Bernard Shaw. There is some evidence that living to be over eighty, whether or not you are famous, is associated with being a member of a biological, and possibly psychological, elite. Furthermore, very old people often have a surprisingly high level of social competence and seem able to maintain high spirits" (1961:201). They take Bertrand Russell to be an example of re-engagement and they mention all of his activities except those at the very end—being an ardent political activist and political prisoner.

are more than two-fifths of those in their eighties isolated, and no more than a quarter of those in their sixties. In fact, the older the person, the less likely he is, if single or widowed, to be living alone. Older women were more likely than younger women to have seen one of their children the same or the previous day (Shanas et al., 1968:281).[10]

However, being *active* in the sense of "seeing people" is not the same things as being *engaged*. As Cumming points out in a footnote in her later article on disengagement theory, active people can actually be "relatively disengaged impingers."[11] To say that no more than two-fifths of the eighty-year olds (in Shanas' sample) were isolated is not to say that no more than that were disengaged. The burden of proof seems to be on those who wish to make a case for continued engagement. If, as Cumming tells us, activity is not the same thing as engagement, then much of the counterevidence is invalid (Zborowski, 1962; Rosow, 1967; Hochschild, 1973). Like Shanas et al., many investigators are essentially concerned with isolation, not disengagement. Despite what such researchers say it means, their data do not measure up to the stringent requirements of counterevidence.

Henry (1964) likewise discounts counterevidence by stressing some criteria and belittling other criteria for disengagement. It is interesting to note just how Henry deals with proffered counterevidence from a study by Reichard et al. (1962). The study cities examples of older people (the "mature" in their typology) who maintain high activity levels with good morale in roles indistinguishable from those of middle age. Henry (1964:25) describes one engaged man in his sixties who is an "enthusiastic breeder of chinchillas." The man says:

> I saw one of the most beautiful chinchilla herds; I didn't know they existed. This fellow has been in it eight years. And he has the most beautiful animals. I could just sit there and watch them all day. And I was glad I went down to see him. It gives me something to work for.

Henry (1964:26) comments:

> The chinchilla breeder . . . in this quotation, strongly suggests that his ego energy has been displaced from active instrumentality and that the still active work involved in chinchilla raising serves him quite differently. Its meaning is not instrumentality in the usual sense, but rather an occasion for unassertive emotional experience and for sensuous, even voyeuristic, observation.

It remains unclear from this whether (a) a similar slippage in focus could discount every other respondent of the same sort or whether (b) this man is engaged in one way but not in others. If failing one aspect, we can always resort to one of the many other aspects in search of some change since youth, and if such extravagant imputations of the *meaning* of chinchilla raising for such a man, when he was 50 or 60 or 90 can be devised, then counterevidence will always be hard to come by.

One could argue that a full-scale replication is the only answer, and a few have been done; for example, Chellam (1964) which supports the theory and Tallmer (1967) and Lipman and Smith (1968) which mainly do not. But since the replication is of the logic as well as of the methodology, we have a comparison of

[10] "The data ... do not suggest that, independent of growing infirmity, social disengagement is a widespread phenomenon" (1968:442). As the authors put it, "The evidence on attitudes is sparse, but does not in general suggest that as old people get older they disengage subjectively .... There may be a contraction in the number of 'peripheral' social relationships ... but there is little change in the number of central relationships" (1968:442). Only 3% more of the very old (80 and over) had seen no relative in the last week than of the moderately old (65-69). While fewer of the older sample went out to visit, almost as high a proportion were visited by others.

[11] She states that there is a "difference between the appearance of engagement and the experience of it. This problem is enhanced by the tendency to contrast disengagement with activity. In fact, activity and engagement are not in the same dimension. A disengaged person often maintains a high level of activity in a small number and narrow variety of roles, although it is doubtful if it is possible to be at once firmly engaged and inactive. In any event, the opposite of disengagement is engagement, a concept different from, though related to, the concept of activity ..." (1963:380). It may be that Cumming and Henry distinguish between activity and engagement by guessing or intuiting how respondents feel about their activity, but they have collected no data on feelings.

some replications which interpret counterevidence one way, with other replications which interpret it another way. If we were to do yet another replication on, say, the Mundugumor of New Guinea, and to find old people engaged, we might also "find" that they were an example of cultural variation, of "off-timing," "unsuccessful engagement" or a tribe of "biological specialities." We do not need more replications of an unfalsifiable theory, but a theory with clear criteria for counterevidence. This is an important lesson to bear in mind when we go about the construction of a new theory based on different assumptions.

The Omnibus Variable Problem and Its Consequences

Essentially, disengagement theory posits two independent variables and one dependent variable. One independent variable is the individual's age with its implied relation to death, and the other independent variable is society's stance toward disengagement. The dependent variable is disengagement. Society's stance toward disengagement—the available freedom from constraint and normative "permission to disengage"—is, ultimately, not a variable but a constant. One type of promising sociological criticism has taken the form of converting this constant into a cross-cultural or historical variable.

In between the independent and dependent variables are many intervening variables which are said to modify the relation of age to disengagement. These are posited as "sources of variation" in the "form" of disengagement rather than sources of variation in disengagement itself. (Such sources include physiology, personality, type of initial engagement, life situation and sex role.) These modifying variables, such as "life situation," seem to be giant "catchall" categories which encompass most of the classical sociological categories such as class, race, urbanity, religion and mobility. A second type of follow-up research has promoted these intervening variables to the full status of competing independent variables.

The omnibus variable problem is most pronounced with regard to the other independent variable, age, and with regard to the dependent variable, disengagement. In fact, the characterization of disengagement as a unitary process has proved to be one of the weakest points in the theory and the discovery of its complexity the most significant result of the follow-up research. Both age and disengagement turn out to be "umbrella" variables which crowd together, under single titles, many distinct phenomena. When researchers began to unravel the parts from the whole, we found that (a) different dimensions of disengagement become dozens of distinct dependent variables and (b) when broken up by this continual fission, former parts of these whole variables can be transformed into promising new independent variables. In addition, (c) different parts of the independent variable (age and related phenomena) have separate links to various aspects of disengagement. It may be helpful to illustrate these patterns from some of the research of the 1960s.

First of all, what the theory says goes together—all the psychological and sociological forms of disengagement—apparently do not do so empirically. This seems to be more than a variation in "form." Atchley's (1971) study of emeritus professors, for example, shows that there can be social disengagement without psychological disengagement. Carp (1969) further distinguishes between different types of social disengagement. For example, disengagement from family, in her study, was negatively related to disengagement from material possessions, mental stimulation, social activities and relations with other people. Carp concludes, "Some inconsistencies from study to study regarding the nature of disengagement . . . may result from the use of different partial criteria and of compound criteria with different elements" (1969:346).

The same problem exists within the various components of the psychological side of disengagement (Neugarten et al., 1964). This raises the question of precisely what aspect of psychological disengagement we are talking about. If we are talking about "adaptive powers and personal adjustment" (Peck and Berkowitz in Neugarten et al., 1964), we find no age differences. If we are talking about "ego energy available to the ego for maintaining involvements in the outer world" or "affect intensity," we do find age differences. If we are talking about active or passive or magical mastery, again we find age differences (Gutmann, 1969). If we mean "seclusiveness and repression in the service of

the ego," we do not find age differences. Neugarten suggests that whether we find age differences (in four studies in her book) or do not find them (in three) depends on the overtness of personality expression. But as Rabin (1965) points out, many of the more than one hundred different variables, types, dimensions and factors introduced in the quest for "disengagement" are often idiosyncratically defined.

I would like to disgress briefly to discuss an assumption one often finds in the literature on psychological disengagement: that social conditioning is associated mainly with consciousness and innate tendencies with the unconscious. If there are intrinsic, developmental features in aging, the data from covert measures suggests they are different for men and women (Neugarten, 1968). The T.A.T. data suggests that men, as they age, become increasingly passive and dependent, whereas women become increasingly active and dominant. Neugarten reasoned that, with old age, there is a "return of the repressed." If this is so, it may be that what was repressed in young women (dominance and mastery) differs from what was repressed in young men (passivity and dependence) for *social* reasons. That which comes up in old age, in these covert measures, may be what was earlier "put down." The crossing of paths in old age may be an unconscious expression of equally unconscious social conditioning. Thus, when we examine data which tells us about unconscious social processes in youth, we are not necessarily discovering innate or intrinsic psychological givens. Rather, we may be seeing the unconscious expression of social conditioning.[12] We cannot automatically equate social conditioning with conscious behavior, nor innate tendencies with the unconscious.

In addition to the problems of measuring and interpreting covert processes, the fact that we no longer have one unitary phenomenon—disengagement—means that new causal arrangements become apparent. For example, Rose suggests that continued economic engagement leads to continued social and psychological engagement (Rose and Peterson, 1965:363). Those who continue to work generally do not retire from associations linked to work, such as trade unions, businessmen's associations, professional organizations and service clubs auxiliary to work. Frankel (1962), pursuing this line of argument (also see Carp, 1966:5-6), compares the psychological indicators of disengagement in old people with the psychological traits of the unemployed in the village of Marienthal, Austria, during the depression (Lazarsfeld, 1932). Young as well as old unemployed men showed "a narrowing of the psychological sphere of wants . . . so that the pressure of external conditions is not felt in its full force. We defined this psychic attitude as resignation" (Frankel, 1962:149). In spite of increased leisure time, the unemployed gave up many of their former activities. They did not read newspapers, even though they received them free, and the number of books borrowed from the library decreased by 50%. In a subsequent study of 57 cases of unemployment, Zawadski and Lazarsfeld (1935:250) found the following reactions ". . . an especially depressing feeling of humiliation and of being superfluous; an increased sensitivity; aggressiveness, which is nevertheless marked by a peculiar inertia (also see Lewin, 1948:103). With an increasing duration of unemployment, the individual progressed from optimism to pessimism to fatalism while his life space gradually narrowed.

If economic disengagement leads to social and psychological disengagement among young unemployed men, Frankel argues, the same can be said for old men who are forced to retire. Thus, it is not age per se, but involuntary economic disengagement itself

[12] Support for this comes from Mark Zborowski's study of the response of old Americans, Jews, Italians, Protestants and Irishmen, to pain in a Bronx hospital. He found that, even controlling for the type of illness or ailment, different ethnic groups expressed their pain in different ways (the Jews moaning and groaning, the Protestants stoically and silently withdrawing). But it was not only expression but actual experience of pain as reflected in physiological tests that varied by ethnic group. Some ethnic groups had higher pain thresholds than did others. If as common a sociological category as ethnic group is correlated with as profound a variable as pain threshold, it is entirely possible that sex or class are correlated as much with unconscious processes (which are as impossible to control as is one's experience of pain) as they so obviously are with conscious and behavioral characteristics (Zborowski, 1962).

which fosters passive and magical mastery, a loss of affect intensity—the hallmarks of "successful" psychological disengagement.

A similar argument concerns social isolation which, though the disengagement therapists don't use the term, is an essential part of social disengagement. Again the researchers of the 1960s have chipped off part of the dependent variable and set it up as a possible explanation for the remainder of the dependent variable. As Rosow (1967) and others have shown, when old people, for various reasons, are socially isolated, as when living in old nuclear families surrounded by young nuclear families, this has social and psychological manifestations. (See Messer, 1966; Blau, 1961; Carp, 1966; Lowenthal and Boler, 1965.)

Another way of dividing the omnibus dependent variable is to separate role loss from what remains of the indicators of disengagement. Tallmer and Kutner (1969) and Tallmer (1967), for example, have cognetly argued and empirically shown, in a sample of 181 people aged 50 to 90, using Cumming and Henry's measures, that when role loss (that is, widowhood, poor health and retirement) is held constant, the relationship between age and disengagement disappears. In their partial correlations between engagement meaures, age and role loss, they found that even with age partialled out, the association between disengagement and role loss remained. In fact, when the association between age and role loss was partialled out, the link between age and disengagement dropped to zero.

Tallmer has essentially interposed such factors as widowhood and poor health as independent phenomena mediating the relation between age and disengagement. Gutmann's argument is similar (1969:35-6): "We do not automatically prove the developmental theory by refuting the sociocultural alternative. There are existential necessities that impinge on most men, especially in later life—the exigencies of illness, of failing strength, of approaching death, of reduced opportunity and hope—that are independent of specific cultural circumstances just as they may be independent of any prior psychological or developmental events. These existential imperatives may be the independent variables, the independent engines of psychological change in later life."

The same sort of differentiation and re-integration has gone on with the variables of age, closeness to death and awareness of death. Cumming and Henry treated age, closeness to death and awareness of death as if they were the "same thing." They therefore could not ask: is it age per se or distance from death or awareness of death which induces disengagement? Lieberman and Coplan (1970) conducted a series of studies comparing people who, as it turned out, were going to die two years later, with a matched group who two years later were still alive.[13] They found the individual's distance from death to be more important than his age per se and actual distance from death to be only loosely linked to the individual's conscious concern about it. Age, distance from death and awareness of death appear to be crucially different.[14]

Furthermore, Cumming and Henry assumed that nearness to death and awareness of death were crucial for some twenty-five or thirty years prior to death. But Lieberman and Coplan (1970) found that those in their study who were two years away from death did show the telltale signs of disengagement, while those who were not two years away from death, did not. Their study suggests that dissengagement may be closer to a two-year than a twenty-five or thirty-year process.

In sum, there are theoretical uses in dividing up the umbrella variable. It allows us to see that the aging individual may become, say, more expressive, but no more oriented toward his psychic interior. He may become less oriented to others, but no more passive. He may be more internally oriented, but no

[13] The 80 subjects (average age 83) came from a variety of socioeconomic backgrounds and social contexts. They did find differences between those who were, it turned out, close to death and those who were not. These differences showed up in attitudinal scales and T.A.T. tests, and most were compatible with disengagement theory. Those near death were less affectively complex, less assertive, less aggressive and more docile, dependent and intimacy-oriented than those far from death. However, they were also less introspective, which disengagement theory would not predict.

[14] Distance from death can again be distinguished not only from age but also from the *awareness* of it. It was this awareness that Chellam (1964), in a replication of the theory, found related to both social and psychological disengagement. It is always possible, of course, that disengaged people tend to be the sort of people who think about death.

less socially involved. He may be more involved with the church, but less with his former work buddies and so forth. Moreover, intricate new causal webs, yet to be fully explored, come into view, where there once stood an apparently solid, seductively simple, omnibus variable. This may not be the story of how a theory declines but the story of how any new theory can grow into something more complex than the simple propositions with which we inevitably begin.

The Assumption of Meaning Problem

Whereas the escape clause problem dealt with the logic of disengagement theory and the omnibus variable problem with the over-inclusiveness of its central variables, the assumption of meaning problem deals with the question: how does the researcher approach the actor's own conception of aging?

Here the researcher must address two questions. First, do we take the actor's own conception of aging into account and collect evidence on it? Second, how important is that evidence relative to evidence about externally observable behavior or unconscious intrapsychic processes?[15] I shall show how the answers to these questions affected Cumming and Henry's conclusions before discussing another study which answers these questions differently.

Like many of their critics, Cumming and Henry did not collect much data on the actor's own conception of aging, and when they did, they did not accord that evidence importance. Between the T.A.T. "X-ray" approach to the actor's interior (e.g., what do you see in this picture?) and the survey of the actor's behavioral exterior (e.g., how often do you get together with friends and relatives?), we are missing the actor's *own conscious conception* and perception of aging and disengagement.[16]

In the formal statement of the theory, the actor's own perceptions seem to come casually in and out of focus. Some of the formal propositions talk about the actor's "expectations" (postulates 1, 5, 6, 7), while others do not (2, 3, 4, 8, 9) and other postulates are simply ambiguous on this point. For example, postulate 5 tells us: "When both the individual and society *are ready* for disengagement, complete disengagement results . . ." (Cumming and Henry, 1961:214). But on this point we have only a reading of his or her inner psyche and outward behavior, with no evidence of his or her *own conscious* perceptions and reflections about "being ready" to disengage. Off and on, the authors talk about the individual's "wanting" to disengage, but there is no conceptual room (even in the discussion of "qualitative" changes) for "wanting" or thinking about wanting, and little corresponding data.

If conscious reflections were fully taken into account and given weight, I think the authors would find themselves with evidence which refutes the theory, were it posed as a refutable theory. They would also interpret differently what evidence they did gather. In their discussion of the effect of age on mobility, they note with reference to another study, "Here we see that the aging people themselves are frustrated by decreased mobil-

[15] Herbert Blumer (1969:2) sums up this problem in disengagement theory when he notes of sociology in general: "Meaning is either taken for granted and thus pushed aside as unimportant or it is regarded as a mere neutral link between the factors responsible for human behavior and this behavior as the product of such factors." Once we take meaning into account there is a third question the researcher must answer: what inferences do we make from statements of meaning? Do we take them as statements about the content of an individual's experience (as symbolic interactionists do) or do we take them as statements which illustrate the actor's process of theorizing about the data of everyday life (as ethnomethodologists do)?

[16] There are a few questions about relational rewards and about what the respondents like most and least about being the ages they are—six of the 12 closed-ended "morale" questions concern evaluations of personal experience. The authors also use two measures of the individual's social world. One was "social life space" ascertained by asking about social contacts and comparing answers to those of older and younger respondents. The other was "perceived life space," ascertained by asking questions such as "Did you see more or less of the relative to whom you feel closest when you were forty-five than you do now? More now, about the same, more than." Or, "Would you say you are more or less of a church goer now than when you were forty-five?" Interestingly, the two measures did not in all cases coincide: most (about two-thirds) of those the researchers designated as "constricted" were women. Of all the women, a fourth did not *see themselves* as constricted.

ity and apparently subscribe to the activity value that the scalemakers impose upon them" (1961:72).

For all we know, this statement might well apply to the Kansas City respondents. As they note in another context, "it is not the loss of the specific content of the occupational role which traumatizes men, but the loss of general instrumentality. Indeed, all evidence suggests that men are pleased to disengage from specific tasks, to withdraw their cathexis from the content of work, *but to change their entire manner of relating was apparently more than they are able to manage*" (1961:152, emphasis mine).

Disengagement, if it means anything, means changing one's entire manner of relating. Yet the evidence which led the authors to the particular conclusion underlined above apparently had no bearing upon their ultimate conclusion that disengagement is natural and, by implication, easy. The actor's capacity to be consciously "traumatized" by general disengagement was not counted as evidence either for or against the theory. As a level of reality, it was curiously circumvented.

Let me offer an example of how this sort of evidence, once admitted, would make it more difficult to render virtually every case into evidence confirming the theory. In *Growing Old*, a woman described as "active" is said to be in a role "appropriate to the disengaged state" (p 158).[17] This may be a good characterization of her *role*—horizontal, somewhat fragile and temporary. But how do we characterize an active *person* in a disengaged *role*? Consider the following case: a woman finds that her lifelong friends and her spouse have died. She moves into a public housing project for older people, goes to meetings, finds new friends and takes up some new hobbies with these new friends, one of whom she is seriously thinking of marrying. Cumming and Henry would point out that her new relationships do not compare in depth to her former ones, that this stage of re-engagement is to be temporary. But what evidence is there, apart from the fact that her friends are necessarily new, that the woman is not *asking* of her new relationships what she asked of her former ones? In such a case, the decision of whether to characterize her as merely "active" or as "engaged" rests on what she *wants* from these relationships that she also *wanted* from earlier ones. Disengagement is defined by the character of an actor's relationships to people. One dimension of a relationship is the actor's own conscious feelings about it. If the actor consciously feels committed or wants to feel committed to another person and is also socially active in her behavior, I think we can characterize her as "engaged." If she happens to die the next day, she can be said to have died in the engaged state.

Again, in the discussion of desocialization, the authors look to behavior and ignore conscious feeling and thought. The authors tell us that as a person grows older, he or she interacts less and less with others. As interaction decreases, so does normative control. As normative control decreases, a person becomes desocialized and shows eccentric behavior. Desocialization prevents any turning back to previous stages of engagement; it is a secondary mechanism that holds disengagement in place. Presuming for the moment that social interaction does decrease with age, how do we know that normative control automatically decreases as well? Is it reasonable to assume that a 60-year old adult, capable of experiencing guilt for violations invisible to others, is normatively controlled in proportion to the number of visitors he or she has each week? Is it reasonable to assume that all eccentric behavior is due to a lack of normative control? The causal links here depend on the actor's construction of reality. In my own study (Hochschild, 1973), eccentric behavior (wearing diamond earrings and tight slacks in the recreation room) was due more to a resistance to acknowledged normative control than due to the absence of it. This resident told me that she "liked getting a rise" out of her more conventional midwestern neighbors. She knew and cared what they thought. She was not desocialized; she was just eccentric. I know this because I asked her directly.

[17] "Women lose an intimate relationship and become free to join less demanding horizontal groupings—thereby moving in the same direction as the disengagement process" (p. 159). But elsewhere they note: "There is, in fact, some evidence in our interviews of considerable frustration among some married women over being unable to join the society of widows, a frustration especially true of the wives of retired husbands" (p. 157).

A different position concerning the actor's own conception of reality is taken by Marion Crawford (1971) in his exploratory study of the retirement of 99 couples in the English working and middle classes. He takes the actor's conception of reality into account and accords it importance relative to other sorts of evidence. Crawford elaborates three ideal types of meaning which men attribute to retirement.[18] Quite apart from "objective" or "subjective" disengagement, all the men are about to do the same thing—retire; but they attribute distinctly different meanings to this event.

The first type[19] perceives retirement as *retiring back to something*—the family. In behavioral terms he may appear disengaged, but his relinquishment of work, church committees and union offices is given meaning with reference to this return to the family (1971:273). The second type perceives retirement as *retiring from something.*[20] Here the man tends to dread retirement, to see it as a loss—of money, of things to do, of a place in the larger social world. Retirement is a regretted, externally imposed removal from the social life of working men, and the company of their wives is perceived as a dreaded retreat (Crawford, 1971:275). By contrast, the third type is *retiring for something*. He feels released from work and past obligations, enabled to re-align with social life outside work and family.

The three views of retirement are not equally distributed between the working and middle classes.[21] A higher proportion of the working class see themselves as retiring back to the family (Type 1), whereas a higher proportion of the middle class see themselves retiring for some other involvement (Type 3). About half of both classes see retirement as dreaded retreat (Type 2). In a similar way, various types of conscious orientation toward disengagement may be socially distributed. Further, they may be distributed in ways independent of the social distribution of other social and psychological aspects of engagement.

Thus, if we ask the older person what he or she asks of a relationship (e.g., commitment), or asks of an interaction (e.g., "getting a rise") or asks of a period of life (e.g., "re-engagement"), we might find more signs of engagement than the theorists found and more discrepancy between what old people are asking for and what they are getting.

TOWARD AN ALTERNATIVE THEORY

The disengagement theorists have posed a vital question and made an important contribution in their attempt to answer it. But as it now stands, the theory will not do. In the theory sketched below, I assume that aging is a biological process, disengagement primarily a social process. Aging is not, but disengagement is, profoundly affected by socioeconomic conditions. Aging has to do with the individual's passage through psycho-

[18] In this exploratory longitudinal study, the first wave of interviews took place before the men had actually retired, and it is this first report to which I refer.

[19] Crawford combines two dimensions in these ideal types, one involving orientation toward something and one involving the positive or negative value in that orientation. Thus, logically, there are two evaluative possibilities for each of the three orientations, even though Crawford found an empirical clustering in the three types.

[20] This second type perceives retirement as an "ending" of something desirable, which is comparable psychologically to other forms of ending—divorce, quitting a job, moving from home—insofar as that meaning is attributed to such events. It may well share certain structural properties with other types of ending. (I am indebted to Professor Marcia Millman for this idea.) Some of these structural features, in turn, may resemble the telltale signs of disengagement—a withdrawal of interests from objects before the actual end, a de-differentiation of psychic and social aspects of the individual, so as to reconstitute a more self-sufficient unity, more fully separate from those with whom one is parting.

Crawford also interviewed wives and found that women assigned very different meanings to retirement than did their husbands. (Of the men, 33% fit the first type, 54% fit the second and 12% fit the third. For the women, the corresponding figures were 52%, 44% and 3%.) The women were much more likely to see retirement as a return to the family and much less likely to see it as a re-alignment to a new activity.

[21]

	Type 1 Retiring Back To	Type 2 Retiring From	Type 3 Retiring Toward	Total
Manual	43% (62)	49% (71)	8% (11)	100% (144)
Non-Manual	27% (14)	48% (25)	25% (13)	100% (52)

(Recomputation from Crawford, 1971:275.)

biological stages; disengagement has to do with individual changes in social position.

I assume, then, that it is not aging per se which determines disengagement, but a combination of factors associated with aging (e.g., poor health, widowhood) and other factors associated with the nature of society and one's location in it which together influence disengagement or engagement. The schema below specifies the relations between various sociological variables and engagement, *barring physical disability and widowhood.* Poor health may lead to disengagement under almost any social conditions, and widowhood may lead to it under certain conditions (see Blau, 1961). Barring both these events, I expect the social conditions outlined below to affect disengagement (see Tallmer and Kutner, 1969). Only in the two or three years prior to death is disengagement, as Cumming and Henry describe it, likely to occur (see Lieberman and Coplan, 1970). Even then, for social reasons, it is more probable for some people than for others. Thus, disengagement is not an innate, universal and inevitable process; at least we do not start with that assumption and thus we eliminate the problem of unfalsifiability.

Second, it seems useful to think of engagement as both a social and a normative process. *Social* engagement refers to social bonds with other people in three main spheres—work, family and leisure social spheres. (The leisure social sphere here refers to non-family and non-work related social bonds.) In future work it may be useful to discriminate between various types of social engagement: (a) those involving much power and importance of function versus those involving little power or importance of function; (b) those involving participation in a social network versus those involving participation in a social circle; (c) those involving a relatively separate old age subculture versus those involving age–integrated networks or circles; (d) those characterized by deep social bonds versus those characterized by shallow or superficial bonds. But for the time being, I will simply state that social engagement is one of two important aspects of engagement in three main spheres of life.

Normative engagement refers to the individual's active imputation of emotional importance and meaning to these various social bonds. Again, in future work it may be useful to discriminate between various types of normative engagement: (a) those involving primarily positive meaning and those involving primarily negative meaning; (b) those involving socially shared meaning and those involving non-socially shared meaning; (c) those involving meanings which are socially rewarded and those involving meanings which are socially punished; (d) those involving meanings associated with enjoyment and those associated more purely with accomplishment. For the time being, normative engagement refers to the degree of emotional investment in and meaning attributed to various social bonds.

Normative and social engagement[22] may vary independently with age. They may be postively or inversely related to each other, for a person can ritualistically maintain a broad spectrum of associations which have long been robbed of inner importance or meaning. While this formulation is overly simple, it is an attempt to avoid the omnibus variable problem. The concept of normative engagement, which re-introduces the actor's own attribution of meaning and importance, addresses a third problem pointed out earlier in disengagement theory.

The engagement of old people varies with the over-arching character of society and with the individual's particular location within it. Work as a major organizing principle governing the individual's tie to the social world does not suddenly vanish at age 65, or 70 or 75.

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[22] In disengagement theory, there is also another important aspect: psychological engagement. Psychological disengagement refers to a number of intra-psychic tendencies: (a) a growing involvement with the self and an attenuation of involvement with others, i.e., a growth of narcissism; (b) a tendency toward a loss of ego energy, or affect intensity; (c) a tendency toward passive or magical mastery and away from active mastery over the world. While, as I have pointed out, each of these should be treated separately and may vary independently, each as a tendency is, as later research suggests, probably related to nearness of death and may not be of crucial importance until the last years. Before the very final years, I would suggest that psychological engagement is a consequence of normative disengagement. Research has shown that psychological disengagement can precede social disengagement. It has not been shown to precede normative disengagement (e.g., a loss of meaning in work). Since I consider it to be closely linked to normative engagement, I will not give it independent treatment here.

Even when a person does not work, his or her orientation to work remains crucially important influencing the character of a person's social and normative engagement in the other two principal spheres of life.

We might posit three ideal types of society and relate them to inter-societal differences in the engagement of the old: the pre-industrial, the industrial and the post-industrial society. While Cumming and Henry focused on differences between respondents in Kansas City in the late 1950s, they ignored (i.e., assumed) the over-all features of the American economy which might account for the over-all similarity they describe. These features are converted here into macro-variables associated with gross differences in the engagement of old people in each type of society. Any real society only approximates one or the other of these three models.

In the pre-industrial model, a large proportion of old people own or control the mode of production—their land, tools, craft implements. The economy is undifferentiated from the family,[23] and the family as an economic unit offers access to work. Old people can thus determine themselves whether or not, or how much to work in later life. In addition, there are normative rewards for continued work in old age. Consequently, there is a *congruence* between what the economy allows and what social norms reward.[24]

The post-industrial model (or future leisure society model) somewhat resembles the pre-industrial model. Here old people do not control or own the mode of production; with the expansion of large corporations very few do so, and the decision of whether or not to work is therefore largely out of their personal control. Family and economy are differentiated and retirement policies are set by universalistic criteria outside the purview of the family. The market economy has developed so far and production become so efficient that many people of all ages are excluded from work in the market economy. But in this model, there are positive normative rewards for disengagement for the old as well as the young. Consequently, there is a *congruence*, as with the first model, between what the economy allows and what norms reward.

The industrial model differs from the other two. Most old people do not control or own the means of production and cannot themselves determine whether or not to work. Most old people are forced out of the market world into the realm of activities with use value—care for the home, hobbies, volunteer work. Yet the old are judged, and judge themselves, against the standards set by those who work (Clark, 1968). Like housewives, the old find themselves "in a society in which money determines value," yet they as a group remain outside the money economy. Their work is not worth money, is therefore valueless, is therefore not even real work" (Benston, 1972:121). In a society where most work in the market economy, those who retire can be valued for who they are or what they do—or do not do. Old people in the People's Republic of China retire (men at 60, women at 50), but they do work with "use value" and this work is highly valued (e.g., care of children under three and socialization of the young about the "bitter past"). Only in the industrial model is there an *incongruence* between what the economy allows and what social norms reward. The Kansas City respondents found themselves in a society which closely fits this industrial model.

If we compare societies, rather than, as Cumming and Henry did, sub-groups within a given society, we might find in pre-industrial societies, more over-all engagement in the sphere of work and family (since family and economy remain undifferentiated) and less in the leisure sphere. In societies closer to the post-industrial model, we might find more over-all disengagement from work and more

[23] Also, the three models vary in degree of structural differentiation. In the pre-industrial model, the spheres of work, family and leisure are most integrated. In the post-industrial model, these three spheres are most differentiated, not only family from economy, but family from leisure sphere.

[24] The three models also vary in the degree of similarity between the status of the young and the old. In both the pre- and post-industrial models, the young and old have similar relations to work, but for different reasons. In the pre-industrial societies, both young and old do work with use value; the old are not thrown off the market, for they were never in it. In post-industrial societies, both young and old form a large and culturally accepted sector of the non-working population, supported by an expanded welfare system. It is only in the industrial model that the old are devalued *relative* to the young, for economic reasons.

corresponding engagement in the family and leisure spheres.

Moreover, we might find a different distribution, in each model, of various types of discrepancy between social and normative engagement, types of discrepancy which are not, as in Cumming and Henry, "deviations" from an "ideal" case of disengagement (1961:214). In societies which fit the pre-industrial model, some old people will be "reluctant engagers"—i.e., socially engaged but normatively disengaged. On the other hand, in societies closer to the post-industrial model, proportionately more of the old will be reluctant *dis*engagers, i.e., socially disengaged but normatively engaged. Thus the distribution of both congruent and incongruent engagement will vary according to the over-all fit of a given society to one of these three models.

INDIVIDUAL LOCATION WITHIN THE INDUSTRIAL MODEL

If we now focus on the industrial model, we can look at internal variations in engagement associated with the old person's *location* within that sort of society. Here the primary variables are social class and sex role. Both locate the individual in the social world. Both influence access to and orientation toward work and family. Since patterns of life in youth are closely related to those in old age (Williams and Wirths, 1965) and since work life is primary in the lives of young men, social class and sex role will affect the relation of age to engagement. Cumming and Henry did not examine the effects of social class, given their homogeneous middle-class sample. They did examine sex differences and found different avenues to the same final state of disengagement. I will focus on sex role rather than sex, and only on male sex roles as they influence orientation toward work and family, and consequently engagement. The theory very much needs to be elaborated to account for the impact of social class and sex role on woman's engagement in work and family.

In industrial society, access to work in the market economy is closed for most, but not all, old people. In such a society, there is a rough link between social class and access to work. Those in the very top bourgeoisie and the old middle class of petty entrepreneurs own or control some part of the mode of production. Thus, they have access to work in old age. Those in the new middle class (e.g., non-self-employed professionals), working and lower classes do not own or control the mode of production and thus do not have access to work. (Social class refers here not to occupational prestige nor income, but to one's relation to the mode of production, which to some extent cross cuts prestige and income.)[25] One's relation to the mode of production determines access to work. Access to work, under certain conditions, determines the likelihood of working in old age, and this affects the character of engagement (Frankel, 1962).

Given access to work, the financial necessity to work in part determines the likelihood of actually working past age 65, though work under threat of poverty may lack a large component of normative commitment. In addition, past satisfaction in work is related to work engagement. Worker satisfaction studies suggest that men with high occupational prestige, with control over the conditions of work, integrated work groups and occupational communities (Blauner, 1966:476)[26] tend to enjoy and want to continue their work (i.e., be normatively engaged). Thus social class in combinations with other occupational factors influences into which of four categories an old man may fit: (a) has access to work and wants to continue; (b) has access to work and does not want to continue; (c) lacks access to work and

[25] It is useful to conceptualize class position with reference to the individual's relation to the mode of production. We can distinguish between the bourgeoisie (those who own the mode of production on a large or small scale), the new middle class (professionals who sell their labor for wages but have authority over other workers), the working class (those who sell their labor for wages but have little or no authority over other workers), the lower classes and the chronically unemployed. This classification schema was suggested through personal conversation with Erik Wright, University of California, Berkeley. Only part of the total schema is used here.

[26] The proportion of men who wanted to continue working or had actually continued working after age sixty-five was more than 67% for physicians, 65% for department store salesmen, 49% for skilled printers, 52% for coal miners and 32% for unskilled and semi-skilled steel workers (Blauner 1966:476).

Chart I. Access to Work, Work Satisfaction and Social and Normative Engagement

			Access to Work			
			Yes		No	
Work satisfaction in middle age			Social	Normative	Social	Normative
	High					
		Work[a]	+	+	—	+
		Family	—	—	+	—
		Leisure	—	—	+	+
	Low					
		Work[b]	+	—	—	—
		Family	+	+	+	+
		Leisure	±	±	+	+

[a]About a quarter of all men over 65 work.

[b]Old people who must work out of financial necessity despite the meaninglessness of work.

wants to continue; (d) lacks access to work and wants to retire. Since Cumming and Henry's sample was not broken down with social class or occupational factors in mind, it is hard to apply this schema to their data.

As Chart I shows, I would hypothesize that those men with access to work; and a history of imputing importance in work, will essentially continue the pattern of middle age, i.e., relatively greater social and normative engagement in work than in family or leisure. (See the top left corner of Chart I.) Men with access to work, but a disinclination for it, will work depending on their financial necessity for doing so—social but not normative engagement in work. (See the bottom left corner of Chart I.) They will be both socially and normatively engaged in the family, resembling Crawford's "retirement to" type. And their involvement in leisure will be in association with the family. Less often it will take the form of a "substitute work involvement."

For the majority who are barred from access to work but who enjoyed their work (the top right corner of Chart I), I would hypothesize a post-retirement experience of social disengagement but normative engagement in work. This may take the form of either (a) negative withdrawal to the family—social but not normative engagement in the family—or (b) realignment to leisure occupations which resemble work. For those who lack access to work and are unoriented to it, I would hypothesize a pattern of "retiring to" the family and/or to leisure involvements. (See the bottom right corner of Chart I.)

There are naturally many types of meaning which men attach to work, whether they actually do work or not. One kind of meaning work has for men lies in its connection to his role in the family—the principal remaining social sphere in the life of old men. This, in turn, depends on the particular definition imputed to manhood. Sex role ideology is a modifying variable which would differentiate between men in each cell of Chart I. For some, "being the man of the house" depends almost totally on being a wage earner outside it. This is a matter of degree, but an important matter. According to Goode (1970:22), the ability to define the status of the family and mediate its relation to the outside world is more characteristic of middle-class professional men. In old age, deprived of work, we might expect such men to seek out "substitute work" spheres. These leisure pursuits, to the extent that they simulate work, provide a substitute mediating and status-giving function. Via these outside attachments, the inner family roles of such men are maintained. Being a "good husband," bringing the outside

world home, will go on *via* new leisure social bonds.

For other men, "being the man of the house" depends less on work outside it and more directly on being a father and husband. According to Goode, this is more characteristic of working-class men. Such men will feel less need for a mediating link to the outside world in order to maintain an understanding of their role in the home. Their leisure will take on the character of *broadening* engagement in the family, rather than *enabling* it.[27]

CONCLUSION

Cumming and Henry posed an important question: how is age related to engagement in social life? But their answer suffers from three problems. First, the theory is unfalsifiable. Second, their major variables are composed of sub-parts which do not vary in a unitary way. Third, they ignored the meanings actors attach to what they do.

The alternative proposed here tries to solve at least these problems. Disengagement is seen as a variable process, not as a constant which varies only in form and timing; thus, the major thesis is falsifiable. Engagement is conceived as having two aspects—a social aspect which refers to the existence of social bonds in the spheres of work, family and leisure life, and a normative aspect which refers to the meanings the individual imputes to these social bonds. Since social and normative engagement are independently related to age and to each other, the second problem is somewhat reduced. Since meaning assumes an important place in the very definition of engagement, the third problem is also reduced.

The engagement of old people varies with the general character of the society (i.e., its fit with the pre-industrial, industrial and post-industrial models). It also varies with the individual's particular location in any one society. I assume throughout that, for men, work as a major organizing principle governing ties to the social world does not suddenly vanish when one grows old: that even in the absence of work one's orientation toward work remains crucial. A person's likelihood of forging meaningful ties in work, family and leisure spheres rests on variables (such as his relation to the mode of production) which impinge on his *access* to work, and factors (such as financial need, worker satisfaction, sex role ideology) which impinge on his *orientation* to work. The resulting engagement in work has implications for one's engagement in the remaining two spheres of life.

This formulation lacks several things. To mention a few, it does not account for the distinctive patterns of engagement for old women nor point to the changes we can expect with the changing role of women. It pays no attention to the problems of measuring social and normative engagement. It leaves unclear the relation between social and normative engagement on one hand and psychological engagement on the other. However, if there is one thing we can learn from disengagement theory and the subsequent research bearing on it, it is the process by which once uniform propositions are recomposed and elaborated until we find some that work empirically in the social and phenomenological worlds of old people.

[27] While sex role ideology influences the meaning of work or worklike leisure to social bonds at home, other factors determine a man's access to family life. A widower or a divorced man has no access to the family, even if he is oriented toward it, just as the man forced to retire has no access to work even if he wants to work. Not only the factors influencing normative engagement in the family but those influencing access to it need to be introduced as a modifying variable in the schema traced in Chart I.

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VALIDATING CONFLICT DATA*

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Despite differences in operational definitions of conflict, strikingly similar findings have been reported by myself (Danzger, 1968a) and Spilerman (1970) on factors precipitating racial conflict. Spilerman, I, and numerous other researchers on conflict, have relied on data which closer inspection reveals to have been wire service reports in different guises. Results derived from this data without taking the pattern of wire service reporting into account, may be spurious. This paper addresses itself to the question of what, if any part, of the reports of conflict appearing in newspapers may be accepted as valid data. Some mechanisms reducing distortion are identified. Types of facts more or less subject to bias are distinguished. Finally, a statistical analysis indicates that a lack of conflict reports may not mean that conflict has not occurred. However, if a sample is limited in the manner to be specified, then the fact that no conflict is reported may be accepted as valid.

In 1968, I reported finding three factors to be critical in contributing to conflict between blacks and whites over civil rights: (1) the number non-white in a city; (2) whether or not a city was located in the South; (3) the age of the city. Furthermore, there was a critical mass effect and a ceiling effect for number non-white. Ninety percent of cities with less than five thousand non-whites did not experience conflict. Above this number, however, seventy percent experienced conflict. Beyond about 35,000 non-whites, the addition of non-whites seemed to produce no noticeable increase in the likelihood of conflict.

Seymour Spilerman (1970:627-49) reported a set of findings which were remarkably similar in 1970. He found too, that number non-white and South were factors most important in conduciveness to conflict. He also reported a critical mass effect as well as a ceiling beyond which the addition of non-whites did not seem to increase the likelihood of conflict occurring. We both used a multiple regression analysis to explain the occurrence of conflict. Figure 1 indicates these similarities of findings, presenting them side by side.

This similarity of findings is all the more striking when one considers that *the de-*

*A number of people assisted in this work by supplying valuable information and useful insights: Fred Powledge, formerly of the *New York Times;* Jesse Bogue, Assistant Managing Editor and Irving Peck, Promotion Manager of UPI; Douglass Lovelace, New York City Bureau Chief and Austin Scott of AP; Gershon Jacobson of the *Day-Morning Journal*; Luther Jackson, formerly of the *Washington Post*; Irving Horowitz, Assistant National News Editor of the *New York Times*; Philips Davison, Professor of Journalism, School of Journalism of Columbia University; Benjamin Holman, Assistant Director for Media Relations, Community Relations Division of the U.S. Dept. of Justice; Fred Wright, Director of Research, Community Relations Division (in Washington, D.C.); Jean Roberts and Roy Reed of the *New York Times* Atlanta, Ga. office, assigned as liaison to the Southern Regional Council. Interviews were conducted from the summer of 1967 through the early spring of 1968. Jeffrey Hadden graciously provided the census data used here.

The author is indebted to several of his colleagues who have read earlier versions of this manuscript: Nathan Joseph, John Lally and particularly Murray Hausknecht. My thanks also to Lee Heimlich for her assistance in technical and clerical matters.

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	Danzger's Findings	Spilerman's Findings
First variable entering the regression	Number non-whites	Number non-whites
Second variable entering	South/non-South	South/non-South
Percent of variance explained by first two variables	.41	.47
All variables	.51	.51
Critical mass	5,000 non-whites	6,800
Ceiling effect	35,000 non-whites	40,000
Data	1960 census data in continental U.S. cities with populations of 25,000 or more	same
	N = 644[a]	N = 673
Assumption	"Conflict is more dependent on the existence of a 'critical mass' than on the proportion of population with a given characteristic. A city with a Negro population of 10,000 is far more likely to have encountered conflict over civil rights than one with a Negro population of 1,000--even if the proportion of Negroes in the population is identical" (Dangzer, 1970:1).	"the likelihood of racial disturbance in a city is proportional to the numerical size of its Negro population..." (Spilerman, 1970:638).
Approach	Use of numbers rather than percentages non-white	same

[a]Based on data provided by Jeffrey Hadden which excluded those 29 cities for which full data were not available.

Figure 1

pendent variables which each of us utilized--the type of conflict--are not at all the same. I examine the eleven-year period between 1955 and 1965; Spilerman examines the eight-year period between 1961 and 1968. Again, while I utilize all conflict events, initiated from either side and by as few as one person, with the prime emphasis and concern being on civil rights demonstrations, Spilerman includes only instances of Negro aggression involving 30 or more participants. Civil rights demonstrations and other planned activities were excluded (1970:630). Despite these differences the findings are almost identical.

It would appear, then, that the weight of the data is so overwhelming that no matter which time-frame is utilized or how the dependent variable is defined, the same results will be obtained.[1] Given, on the one hand, the

[1]Morgan and Clark's more recent report (1973:612) which finds two other measures to be

remarkable similarity in the findings and, on the other hand, the major differences in the definition of the dependent variable, this explanation seems suspect.

The configuration suggests the possibility that some other variable is the source of these relationships, and that the relations of the dependent variables to non-white, South and so forth are in fact spurious.

A likely source of spuriousness is the data source for the dependent variable, the conflict events. My data were drawn from the *New York Times.* If the reporting process distorted the data so that conflicts were more often reported in some cities than others, not because conflict occurred more often in fact, but because it was better reported, then this would explain the remarkable similarity between my findings and Spilerman's. For then, despite the difference in the type of conflict event or the time-spans studied, more conflicts would be reported to have occurred in those cities with fuller reporting services than in others. We would both be describing the effects of the news gathering and reporting process, which would be identical, rather than conflict variables which are differently defined.

On considering the news gathering process, which is to be detailed below, it appeared to me that one factor might have affected the amount of conflict reported for a given community: whether or not that community had a wire service office in it. About 100 communities in the U.S. had wire service offices in the 1960s. In other communities the wire services depended on the local news service or on stringers to supply them with news. Conceivably, news of civil rights conflict might be suppressed by local services or stringers. Wire service bureaus, on the other hand, are independent of the community and are under pressure to disseminate news stories.[2]

To determine whether in fact the data might be distorted by reporting processes, I included a new variable in a second regression: whether or not a city had an Associated Press or United Press International wire service bureau office in it. My hypothesis being, if conflict is a reflection of news reporting services, then cities with wire service offices (AP/UPI) would report a greater number of conflict events than other cities.

When this was added to the regression, a striking change in the pattern is seen. AP/UPI now becomes the variable entering the regression first,[3] contributing .261 to R^2 while number non-white falls to third, contributing .083 to R^2.

The simple correlations also indicate this pattern; the correlation between conflict and AP/UPI being .511 while that of non-white and conflict is .479. Furthermore, even where we attempt to eliminate the effects of interdependence between these variables by forcing non-whites and South to enter the regression first, we are still left with AP/UPI contributing .094 to the regression, indicating that it has an effect on the regression quite apart from that of non-white or South. This would certainly indicate that findings utilizing news reports might be distorted by the news gathering processes.

While this seems to hold for my data, does it hold for Spilerman's data[4] as well? I think it does.

Spilerman's (1970:630) data sources are: The *New York Times Index,* the *Congressional Quarterly's Civil Disorder Chronology* (1967), the Lemberg Center's *Riot Data Review* (1968:numbers 1-3) and the *Report of the National Advisory Commission on Civil Disorders* (1969:323-4). Although they appear to be a variety of sources, in reality they are all based on a single source of data, newspaper reports, and several of them are based on the *New York Times.*

To take them in order:

(1) The *New York Times Index,* draws on the *New York Times* alone.

(2) *The Congressional Quarterly Civil Disorder Chronology* draws only on reports published in the *New York Times*, the

better predictors of disorder, namely total populations and police force size, is not a contradiction. Their study was restricted to forty-two cities with 1,000 or more non-whites in the total population range of 50,000 to 750,000.

[2] Whether in fact this is so is still open to question. This is simply one possibility.

[3] In these comparisons, as in the previous ones, a scale of conflict events was used: 0, 1-5, 6-15, 16-50 and 51 or more. This is similar, but not identical with the log of conflict events used below.

[4] A request to Spilerman in August of 1973 for permission to examine his data was refused. He referred me to the sources cited in his article.

Washington Post and the *Washington Star.*[5]

(3) Report No. 1 of the *Riot Data Review* of the Lemberg Center for the Study of Violence at Brandeis University, states that the *Times* and the *Washington Post* were the major sources of data. Additional sources were a newspaper clipping service and the Southern Regional Council. While the first is clearly based on newspapers, S.R.C. relies for almost all of its information on newspapers as well, a fact which many readers may not be as aware of. (The *Review* adds (Lemberg, 1968:1) that if disturbances are not reported in local newspapers it is unlikely that it would know of them.) Nor is *Review* No. 3 substantially improved, as it warned the reader of its tentativeness "owing to its heavy reliance on newspaper accounts"[6] (1969:8).

(4) *The Report of the National Advisory Commission on Civil Disorders* is not a major source of new data for the following reasons: (a) it is restricted to 1967; (b) it relies on the Lemberg Center and the *Congressional Quarterly* for the data. It is thus partially redundant with previous data.

Suffice it to say at this point that all of these various sources rely heavily on the wire services for their information. What appear to be numerous sources, is upon closer examination primarily the same source in different guises.

While the argument to this point has been directed primarily at my own work and that of Spilerman, it applies equally to other work dealing with conflict. The recent reanalysis of the causes of racial disorder by Morgan and Clark (1973) also uses the Lemberg *Riot Data Reports* (1968). Lieberson and Silverman (1965) and Bloombaum (1968) use the *New York Times*. In none of these reports was an attempt made to validate the data.

Coleman's path breaking study of community conflict (1957) and Wanderer's study of riots (1969) have also utilized newspaper sources without assessing the validity of the data. The U.S. Commission on Civil Disorder has also utilized this data despite its demurrer regarding the news (1968: 362-89). Two studies which have become something of classics in their field, Tanter (1966) and Rummel (1966), rely solely on the *New York Times* for data on conflict within and between nations. Here again the problem of validity is ignored, although the *Times* is particularly unreliable on international news because of its own reliance on wire services and for other reasons specified by Lichtheim (1965).

Be that as it may, the analysis of my and Spilerman's studies of racial conflict shows that news reports cannot be accepted uncritically and that techniques for validating large amounts of such data must be developed. What follows, therefore, is a detailed assessment of the validity of news reports for the study of civil rights conflict. The approach here may be applicable to other news data.

CRITIQUES OF THE VALIDITY OF THE NEWS

There is a rich literature which is highly critical of newspaper reports as a source of data. For example, Warren Breed (1958) suggests that many newsworthy stories do not appear in newspapers. News, he argues, is selectively published, and stories not favored by the powerful interests in a community or not consonant with prevailing public opinion are not published. In an earlier article, Breed (1955) describes the mechanism operating in this selection. The experience of having editors "blue pencil" stories, or being called in by the editor or publisher to discuss stories, informs the reporter (informally, but effectively) as to what may or may not be published. Recently, Molotch and Lester (1974) have suggested that the inclusion of an event in the news (rather than its omission) is frequently a product of conscious manipulation of a situation.

Bagdikian (1972) claims to have documented charges of bias in reporting. He states that he reviewed eighty-four systematic studies of news reporting and found that in all but three cases the editorial policies of a newspaper were reflected in its news columns. And Sigelman (1973) has argued that the mechanisms for maintaining the biases are recruitment and socialization.

In a collection of articles on news coverage

[5] Personal communications from Merwin C. Phelps, Chief, Library Service Division, Congressional Research Service, Library of Congress, Dec. 5, 1973, and James W. Lawrence, Research Department Congressional Quarterly Inc., Feb. 5, 1974.

[6] *In Riot Data Review* No. 3, an additional source was used. Police sources were also consulted in those few cases where sniping was alleged.

of the racial crises (Fisher and Lowenstein, 1967), newsmen themselves argue that there is bias in news reporting. The bias appears in the presentation of the image of the Negro, the meaning of conflict and the importance of the issues. Hohenberg (1960), Talese (1969) and Stempel (1964) point out a variety of obstacles to the publication of news. First, some news stories may never be printed because they come into the newsroom after other stories have pre-empted the available space. This is not a random peril but a systematic hazard. For example, Mondays are easy days for getting things published, while Tuesdays are difficult days (interview, Davison, 1968).

Hallow (1972) and Caldwell (1965) have documented some instances where reports of riots have been grossly exaggerated. And Jacobs (1967) has characterized news media estimates of a crowd size as bordering on the fantastic.

Nor has the use of the wire-service materials escaped criticism. White (1950) has concluded that an editor's decision to include or delete material from his newspaper materials coming over the wires appears to be "highly subjective [and] based on the 'gate keepers' own set of experience." Similarly, Gieber's study (1964) of sixteen Wisconsin dailies finds no major differences in news selection and display among wire service editors. He too attributes the selection pattern to subjective selection standards.

An entire body of literature exists which raises the most serious questions concerning the validity of news reports. Researchers on conflict have ignored this.

Yet given the startling similarity between findings based on apparently dissimilar data such as that found between Spilerman and myself and the fact that almost all researchers in conflict utilize newspaper sources in one form or another, the most serious questions regarding the data need to be raised. Are we simply analyzing patterns of news reporting and publishing? If so, what if anything may be considered valid data in the study of conflict?

One could attempt, as an historian might, to find corroborating evidence; but to do this on a story by story basis completely eliminates the possibility of utilizing news reports as data for the quantitative study of conflict. What is needed is an approach which would facilitate validation of large masses of data to indicate their limitations. I propose to do precisely that here.

A review of the literature critical of newspapers as a source of data has led us to categorize the criticism into three types: claims of (a) inaccuracies and distortions of the facts; (b) claims of bias, in terms of slanting the news, or coverage and placement to favor one side; (c) claims of suppression of stories that run counter to the views of the reporter or the policies of the newspaper. We shall deal first with inaccuracies and distortion of fact.

STRUCTURAL ARRANGEMENTS PROVIDING ACCURACY OF FACT

Reconsideration of the critiques of the validity of news leads us to the conclusion that the critiques are themselves flawed in that they ignore the interactive and corrective processes which characterize the news network. For, the fact is overlooked that *every single criticism of the accuracy of newspapers cited above is based on the view of newspapers as a chain of communication—a straight line process. Decidedly, it is not this. It is an interactive network of communication.* And just as more recent studies have shown that outside the laboratory where interactive networks of communication are permitted to work, rumors are not distorted but on the contrary, accuracy may be increased (Buckner, 1965), so the interactive network of news agencies reduces, if it does not eliminate, distortions in the news.

A short digression here to discuss the findings regarding rumors may be useful. Allport and Postman's initial attempts (1945:81, emphasis in original) to assess the distortions in rumor led them to conclude that "rumor will suffer such serious distortion through the embedding process, that *it is never under any circumstances a valid guide for belief or conduct*." These findings were based essentially on experimental evidence, which the authors believed, *underplayed* the degree of distortion that would occur elsewhere.

On the other hand, other researchers have shown that under given circumstances rumors tend to be accurate (Schachter and Burdick, 1955; Bauer and Gliecher, 1953) and that the process of transmission may at times even increase the accuracy of rumors (Caplow,

1947:302). In surveying these researchers, Lang and Lang (1961:57, 58) note that in the laboratory situation, rumors travel in chains. *In the field situation, rumors also travel in circles.* Not only are rumors thereby cross checked, they add that "there is a natural tendency to cultivate accurate sources and to exclude from the rumor network those whose past performances have proved unreliable."

Buckner (1965) notes that two factors affect the accuracy of rumors: the "set" (orientation and knowledge) of the transmitter of the rumor and the pattern of communication, i.e., whether it is a chain or a network. Rumors passed on by a person with a "critical set" "*will benefit from his knowledge: they will be more accurate than those received from any single source*" (1965: 60, emphasis in original). Buckner argues that given a critical set and a communication network, *rumor becomes increasingly accurate as it moves through the network.*

No more than this need be said for news moving through the news network. We turn now to an exploration of news transmission: (a) the wire services; (b) reporter interaction.

The Wire Services

The primary source of domestic news for all newspapers in the United States, including the *Times*, are the wire services—Associated Press and United Press International. Knowledgeable estimates (Hohenberg, 1960:197) are that 75 percent or more of national news published in the American press is provided by the wire services and many newspapers accept at least half their statewide stories from the wire services as well.

Both wire services report more than 7,000 outlets, consisting of newspapers, radio and TV stations, news magazines, government agencies (including military intelligence) and even private concerns (1960:192).

In 1967, UPI had about 10,000 full-time employees, of whom five thousand were located in the United States. In addition, it employed as many as ten thousand additional "stringers." The precise figure is hard to estimate as these people were employed by the story—hence only on occasion. UPI had its staff not only in every state capital, but in principal towns and county seats (interviews, Jesse Bogue and Irving Peck, 1967). The UPI is an independent entity, not an association of news agencies as is the Associated Press.[7]

The Associated Press is a cooperative association of news-gathering agencies, i.e., newspapers, radio and TV stations, etc. Each agency acts as a source of news. AP has fewer people solely devoted to its activities than UPI, but the scope of its activities as a whole is more formidable. In 1967, of the roughly 1,700 newspapers in the United States, about 1,300 belonged to the AP cooperative. In addition, roughly three thousand radio and television offices around the country were members of the association. AP also maintains about one hundred news bureaus in major cities (interviews, Douglass Lovelace and Austin Scott, 1967).

Each state thus had two to five wire service bureaus. Each bureau had a number of full-time men and often a retinue of stringers as well. Major government departments also are sources of news for the agencies. (For example, the Department of Justice was an important source of information for UPI on civil rights.) Leads emanating from Washington are followed up on the local level. Police, stock market teletypes and teletypes from other agencies also feed information directly into the bureaus.

Although AP and UPI are competitive organizations and do not cooperate with each other officially, neither can hide the news from the other for very long once information is on the wires. Hence, in reality, both operate as a single news-gathering network, with competition spurring both on. A signal to this network alerts both agencies and other news-gathering groups, who may be drawn in if the story appears important.

The Reporter Network

Questions may be raised regarding the accuracy of a reporter in covering a story (or in fact the report of any single eyewitness of an event). Eyewitness evidence in a court room often contains omissions and distortions. Allport and Postman (1947; 1945)

[7] This makes UPI a source of information for those local events which might not appear in local papers. Once again, we argue the possibility that UPI may publish a story, may push local papers to publish events even against their own inclinations. We have previously stated, and reiterate here, that whether in fact this is so, is still open to question.

have alerted us to other possibilities for distortion. What would make a reporter's story more accurate? How will he be able to avoid distortions?

An interview with Fred Powledge, formerly of the *New York Times* and assigned to the "civil rights beat," provided some insight. The view expressed by Powledge was generally confirmed by others in the field (Jacobson, Davison interviews 1967; 1968).

Powledge pointed out that reporters from a variety of news organizations found themselves on the same assignments with the same colleagues again and again. Collegial relationships often developed into friendly ties, and the reporters would seek each other's company. They would stay at the same hotels, have their meals together and drink together.

For example, in covering civil rights in the South, Powledge was frequently in the company of Claude Sitton, also of the *Times*, Joseph Cummings of *Newsweek*, Herbert Kaplow of NBC, Hughes Rudd of CBS and a number of others. Inevitably, they would exchange information. If differences of fact arose, they would be reexamined and ascertained. This exchange of information resulted in every reporter having a far more detailed and current view of a wider variety of relevant facts than any could have on his own. In addition, this process of exchange and comparison acts as a corrective of errors of fact by any given reporter.

Such an arrangement might also produce common errors, particularly if the reporters simply accepted the story without checking in on their own (as they must do when there is a pool arrangement). But Powledge argued that while some information was exchanged, reporters covered the stories independently.

Given the large staff of reporters from the variety of news media assigned to cover civil rights, little chance for real distortion exists except in two situations: (1) where access to the situation is limited or dangerous and (2) where the action is so spread out or so unpredictable that it cannot be covered adequately everywhere. This results in reporters being spread so thin that a single reporter's account is all there is to go on.

In the civil rights situation, publicity was sought once conflict broke out. Media were contacted, particularly by integrationist groups, in an attempt to use publication of stories as instruments to achieve their ends. Demonstrations, boycotts, etc. could therefore be adequately covered, as there was advance notice of these events. Where spontaneous events occurred, efforts were made to reconstruct the event and to present discrepant views of the events. Where differences did occur, they were primarily in the interpretation of motives and intentions rather than of the events themselves. Such things as the number of demonstrators, their organizational affiliations, etc. are almost never disputed. In summary, Powledge's description of the interaction of newsmen covering the same beat indicates that it produced a pattern of communication within the reporters' group which probably was of enormous value in *reducing* the possibility of distortion.

The reader should note that part of the corrective process in the news occurs after publication, as it does in the sciences. Corrections may follow the initial publication. A story may be missed one day but pop up again the next day, as with a series of demonstrations. An article may be "knocked out" due to the peculiarities of deadline or the appearance of more important stories on that particular day. If it has continuity, however, it will probably be picked up together with the newer development.[8] Therefore, a researcher utilizing newspaper data, would be well advised to use wherever possible an entire sequence of stories as a unit of data rather than a single story.

An additional caution: riots, particularly those that involve thousands of people, immense property damage and loss of life, are much more difficult to report than demonstrations. In fact, there are numerous accounts which document inaccuracies in riot reporting (Caldwell, 1965; Hallow, 1972; the National Advisory Commission on Civil Disorders, 1968). There is normally no prior notice of riot, hence it is not often that a number of reporters are in a position to observe. Moreover, when violence is intense, movement

[8] It is standard practice for newspapermen to read "the competition." Therefore, even if stories are knocked out by the deadline of one paper, they will appear in another. If one considers that not only newspapers but also radio, TV and magazines provide coverage of events, the "deadline" no longer appears to be a serious obstacle to the publication of news.

is severely restricted so that reporters are not able to observe events first hand. We no longer have a "ball game" situation, a situation open to simultaneous observation by a number of independent observers. The restrictions on the movement of reporters make them dependent on the accounts of others. Communication no longer follows the pattern of a network, but rather that of a chain. Such reports must be approached warily.

Nevertheless, the errors in riot reporting cited by Hallow and Caldwell as well as in the report of the U.S. Commission on Civil Disorders were in fact corrected by the newspapers as soon as new information became available. Whatever the effect of incorrect early news stories may have been (the Commission argued that its effect was harmful) is not the concern of this analysis. My point is that if a researcher used this material he would in all probability eventually be informed of the actual facts of the case. Dealing with a sequence of events minimizes the risks of error. And, in fact, this approach may be more useful in validating data than the most careful scrutiny of a single given report—even by someone trained in internal and external techniques of historical evaluation.

BIAS

A review of the available studies indicates that the editorial policies of newspapers as well as other factors do affect the presentation of the news (de Sola-Pool and Shulman, 1959; Bauer, 1958; Bagdikian, 1972; Fisher and Lowenstein, 1967; Spearman, 1962. With regard to civil rights, for example, one finds (Fisher and Lowenstein, 1967) that there is bias in the presentation of the image of the Negro, the meaning of the conflict, the importance of the issues and, of course, the space devoted to the story, depending on which side is favored.

The advice to a researcher would, of course, be to avoid dealing with bias and to accept only the facts. This may appear to be meaningless advice for the problem would seem to be to distinguish bias from fact.

But comparing observations of conflict to observations of a ball game provides some guidelines as to what elements of the report are most reliable. A ball game's score, the names of the players, the number of hits and so forth are all indisputable data. They are open to observation by any number of people, all of whom will agree on these points. On the other hand, a player's attitude, whether "Jones gave it everything he's got," is open to question. Similarly, in a case of conflict whether an action was a march or a boycott, involved ten or one hundred, the police or the clergy, are all clear facts. The attitudes of rioters—i.e., are their actions part of a "Christmas in August" looters' syndrome or are they trying to get a message through to the power structure; is an attempt to break up a mob intentional harrassment or a reasonable action—all these are open to question. These are the kinds of facts which are easily subverted by bias and difficult to establish beyond question. Newspaper reports may be unreliable data sources here.

Hard facts based on news reports can be developed; and if code categories are sufficiently broad, they may be reliable. One may, for example, utilize crowd counts in a series of conflicts in a city. They probably should not be used as exact descriptions of numbers involved, but a statistical analysis of such data would not be affected by a few miscounts. Moreover, whether or not police or army are involved, whether a case was brought to the courts, whether there were any arrests, whether or not people were killed or injured and property damaged, what the stated reasons for an action were, etc. are all observable facts. Such facts may be relied upon and utilized as data. Tuchman (1973:113) labels this "hard news" in contrast to interpretations which are termed "soft news." There is no evidence that "hard" facts are affected by bias, i.e., are distorted. On the contrary, such existing studies as attempt to assess the reporting of news (e.g., Spearman, 1962; Bowers 1967) agree that bias affects placement and display, but does not produce distortion. The pattern by which news is transmitted tends to make gross distortion of such facts difficult. And if one deals with newspapers of record, such as the *New York Times* purports to be, this is not a serious problem.[9]

[9] Breed (1958), for example, excludes the "quality press" (specifically mentioning the *New York Times* and *Harper's*) from his analysis of distortion.

SUPPRESSION

Warren Breed (1955) has argued that newspapers suppress some stories and there is a widespread belief that this is so. Participants in the civil rights movement, for example, claim that news was frequently suppressed. James Foreman (1972:242) of SNCC writes:

> The wire services had a habit of always checking out a story with their local stringers. The stringers were usually racists who had good relations with the police chief and the sheriff, and who would seldom verify any story of atrocities that we might send them [the wire services].

But Breed does not present hard data to prove suppression. His argument is based on a hypothetical set of possibilities.[10] Nor do others who argue that bias exists in the news (some of these are cited above, such as Fischer and Lowenstein, 1967) provide any hard evidence that suppression occurs. By and large, examples cited indicate only that bias exists, i.e., differences in handling the news and in prominence of stories.

However, Bowers (1967:43-52) does provide data which indicate that suppression does occur. He examined the activity of publishers in directing the use or nonuse, content and display of news for 600 U.S. dailies. Managing editors of the papers rated the publishers' involvement. Results reported included the following: (1) publishers are more active in directing news when they are geographically closer to the subject matter; (2) publisher activity is higher in areas which might conceivably affect the revenue of the newspaper directly or indirectly; (3) the smaller the newspaper's circulation, the greater the publisher's involvement; (4) publisher activity regarding news of local or racial issues is highest in states with a greater proportion of Negroes—upper South, Southeast and Southwest. This last point is of most direct interest in the study of civil rights conflict.

A Test of the Effect of Wire Service Bureaus on News Reporting Patterns

Earlier, we described how news that has been published locally gets to be known nationally and how the pattern of interaction of reporters and of newspapers tends to generate a high degree of accuracy in the reports of events that are published. The weak link in this chain is the possibility that a local newspaper may not publish stories which it wishes to suppress so that the story never gets on the wires. Under these circumstances, those mechanisms of news media interaction which exert pressure for accuracy may never be activated.

We propose a statistical test of possible suppression to determine whether this may have occurred, i.e., whether stories might have been suppressed at the local level. We compare cities which have either AP or UPI offices (not simply outlets or stringers or affiliations with AP, but independent offices) with all other cities. We hypothesize that more stories will be reported in cities with AP or UPI bureaus than in other cities. The logic behind it is that the bureaus, being independent newsgathering groups whose function is to transmit news, are more likely to do so than are local newspapers or stringers. The latter may be subject to local pressure to suppress stories.

If we find no difference in reports of conflict when cities with wire service are compared to those without, this would indicate that news reports are unaffected by a wire service. It would then appear that whether or not a city has a wire service office, the chance of a given story being published in the *Times* is about the same.[11]

[10] Breed (1958) attempts to identify those stories within the range of newsworthy events which, nonetheless, are not found in news reports. He notes a number of themes reported by sociologists engaged in community studies. Under each of these themes, newsworthy stories conceivably could be written, but generally are not. Under "business ethics" one should find stories such as "property interests prevent tax increases, force low physical and educational standards in high school" or "power and light company... remains smoke nuisance"; under "religion," "upper-class resentment at lower-class membership in their church" or "low attendance at services." Under "patriotism," he contrasts the press' reaction to the United States' intervention in the Guatemala Revolution of 1954 with the treatment of the Cuba intervention of 1961. Similar stories might also have been written on the family, the community, health and doctors.

But it is important to note that Breed's criticism refers to "soft news" (Tuchman 1973), those stories describing patterns which require digging, interpretation and analysis.

[11] The argument that news is suppressed may be unpalatable, but it is also most plausible. One cannot

Data

To test the above hypothesis, my procedures were as follows. *The New York Times Index* was scanned for the period 1955 to 1965. All stories under a variety of headings (eg., civil rights, Negroes, education, sit-in, etc.) which related to civil rights conflict between blacks and whites were xeroxed, clipped and pasted onto index cards. Care was taken to preserve the date, page and column on which this story had appeared in the *Times.* These cards were then sorted by state and city and arranged in sequence by date. Well over ten thousand were identified in this manner.

Out of this mass of conflict reports, we selected only those related to cities with populations of twenty-five thousand or more.[12] These reports consisted of descriptions of events such as demonstrations, marches, sit-ins, the posting of guards around schools to facilitate or prevent integration and so forth. Our universe then, consisted of 644 cities in the continential United States, having populations of 25,000 or more by the count of the 1960 U.S. Census.[13]

For each of these cities, we simply counted the number of conflict reports published, without regard to number of participants involved, intensity of conflict and so forth. As the data are extremely skewed, however, we have transformed the simple count of conflict events into the log of conflict events. In the analysis below, "log conflict events" is utilized.

Results

The correlation between AP/UPI and log conflict reports is .524. This would seem by itself to indicate that the reports are an artifact of the location of AP/UPI offices.

It is of course possible that cities with wire service offices report conflict more frequently because they have other characteristics which cause conflict. Hence, the greater incidence of conflict reported in these cities may reflect the reality of these other factors rather than the artifact of reporting patterns. In other words, more conflict may in fact take place in wire bureau cities because such cities also have other characteristics which give rise to conflict. But this does not seem to be the case. Using a multiple regression technique which in effect takes a number of factors and their relation to each other into account simultaneously, it nevertheless appears that the bulk of variance is "explained" by the existence of AP/UPI offices. The thirteen variables included here cover a range of factors which may be thought to be associated with the occurrence of conflict. Yet they account for very little of the variance. This pattern appears to confirm that reports of conflict may be no more than an artifact of the location of wire service offices.

It may be argued that the effect of the other factors is masked by the first factor to enter the regression. Thus, although their effect is real, we simply cannot see it when a regression is used. To test this possibility, we forced into the regression those factors (number non-white, South and city age) which we have argued elsewhere (Danzger, 1968a; 1970) are highly related to conflict reports. Yet AP/UPI still accounts for a

argue that differences in reporting are related to resources and facilities. True, wire service offices have telecommunications facilities; but telephones are also adequate for the transmission of news and are, in fact, often so used. As for personnel, normally they are adequate in number to report the bare essentials of a story so that the wire services may send in teams of reporters for fuller coverage if warranted.

Moreover, the data indicate that some cities without wire service offices report conflict as often as cities with such offices. Now, if the inadequacy of resources and facilities was a sufficient explanation of the difference in reporting between AP/UPI bureau cities and non-bureau cities, then why the differences among non-bureau cities? Certainly within this group one cannot argue that differences in facilities or resources explain differences in reporting of conflict.

Nevertheless, we do not argue that such suppression is conscious. It may well result from a lack of sophistication or self-confidence on the part of reporters, especially young reporters. Breed (1955) has noted that reporters learn what will or will not be published through the experience of having editors "blue pencil" their stories. The reporters may be unaware of their values, once internalized. (See also Sigelman, 1973; Tuchman, 1972:674.)

[12] Keyes (1958) has argued that it is at this population level that communities acquire the characteristics of urban cities. Below this level, their character is not urban.

[13] In 1960 there were in fact 672 cities in continental United States with populations of 25,000 or more. Twenty-eight cities were eliminated from the study because of insufficient data on census characteristics. These data were generously made available by Jeffrey Hadden.

Table 1. City Characteristics and Log Conflict Reports: Step-wise Regression for Cities with Populations of 25,000 or More (N = 644)

	R Square	Zero Order Correlation
1. AP/UPI	.275	.525[b]
2. South non-South	.130	-.396
3. Number non-white	.107	.520[b]
4. City age	.033	.423
5. N unemployed[a]	.018	.414
Total variance explained	.576	

[a]Nine additional factors add only .043 to R Square. These factors are (in the order in which they entered the regression): N employed in public administration; N employed in educational service; payroll of manufacturing establishments; N foreign born and children of foreign born; expenditures of city government; N factory establishments employing 25 or more; total population; N managers, officials and proprietors; state capital. Each contributes less than .003 to the regression.

[b]The F for AP/UPI is 244.1 while that for non-whites is 115.6.

Table 2. City Characteristics and Log Conflict Reports: Step-wise Regression for Cities with Populations of 25,000 or More (N = 644)

Forced Step-wise Regression	
Number of non-whites	1st
South/non-South	2nd
City age	3rd

	R Square
1. Number non-white	.270
2. South/non-South	.139
3. City age	.081
4. AP/UPI[a]	.054
Total variance explained	.572

[a]Ten other factors add .028 to R Square.

substantial proportion of the variance.

It seems clear then, that research attempting to identify factors producing conflict will be utilizing contaminated data if newspaper reports are used. (All studies of civil disorder which attempt such statistical analysis do, in fact, use news reports as their data.) Conclusions based on such data are very likely to be misleading.

WHAT IS VALID IN CONFLICT DATA?

It will be recalled that wire services function as an alarm calling the news media to an event. In response, reporters or even teams are dispatched by other newspapers and by the central offices of the wire services themselves. So that although the wire service may not have an office in a given city, once the word gets out about a newsworthy story, the wire services will not depend on stringers or the local paper for a report, but will send their own teams of reporters to investigate the matter further. We therefore expect that if we compare cities with a "substantial" number of stories but without wire service offices to cities with wire service offices, we will find no difference in the number of conflicts reported. Once the alarm has been tripped, reporting will be equally good regardless of whether or not a city has a wire service office.

A single story, however, is neither sufficient to bring in a substantial number of outside reporters nor to prove the reliability of the stringer or associated agency. As Table 3 indicates, even though the effect of AP/UPI is reduced, it still remains.

When, however, we take only those cities which have reported substantial conflict (here defined as ten or more reports for the eleven-year period under consideration), the effect of AP/UPI is eliminated entirely.

Furthermore, using partials, we find that *now we can eliminate the correlation between AP/UPI and conflict with only two variables,* number non-white and state capitals. What this means is that among those cities which have already reported some conflict, AP/UPI does not affect reports. Once a number of conflicts have been reported, we can rely on

Table 3. City Characteristics and Log Conflict Reports: Step-wise Regression for Cities with Populations of 25,000 or More. Only Cities with One or More Conflict Reports (N = 223)

	R Square
1. Non-white	.247
2. South/non-South	.128
3. AP/UPI	.071
4. Unemployed[a]	.020
Total variance explained	.510

[a]Ten additional factors add .050 to R Square.

the counts of conflict events reported for that city. We find no difference between such cities and AP/UPI cities in conflict reporting.[14]

The statistical comparisons then, indicate that utilizing newspaper reports of conflict as the basis for analysis is likely to be misleading as these data are contaminated by the factor of wire service office location. But they also show if one uses only those cities which have already reported *some* conflict, there seems to be no such contamination.

In sum, what part of newspaper data is valid? It appears that the facts themselves as reported in the news are generally reliable, given the network of news gathering. Reports of the occurrence of an event and many elements of the description, such as the number and type of participants, the actions occurring and so forth may be accepted as fact. Given the structure of corrective processes, such facts are more likely to be valid if gathered for a time sequence rather than for a single event. Interpretation of events and placement of the news is highly likely to be affected by editorial policy, particularly on an issue like civil rights. Care should be taken to distinguish interpretation from fact.

Statistical comparisons of events, particularly those that attempt to deal with why events occur in one place but not another (civil rights conflict, for example) are unreliable. Newspaper reports may be accepted as data that an event did occur, but the lack of such report may not be an indication that events did not occur.

One must begin by ascertaining that news reporting networks are similar in the cities compared. Comparing only cities with wire service offices would be one possibility. Alternatively, as we have shown in the case of civil rights, one may add to such a base other cities for which several reports of conflict have been published even if over an extended time period. Where several conflicts have been reported, one may rely on news networks to be accurate also as to whether many or few conflicts have actually occurred. Statistical analyses which do not examine the news networks and attempt to assess their validity should be viewed with wariness.

Perhaps a word is in order at this point regarding my own and Spilerman's earlier findings. Concretely, what is left of our findings? My own data indicate that number non-white remains the critical factor in the occurrence of conflict. This can clearly be seen in Tables 3 and 4. I would venture the guess that the same would probably be the case for the occurrence of riots. A key point in both our findings is not refuted. But the point at which a "critical mass" effect occurs and the point at which a "ceiling effect" occurs need to be reexamined.

SUMMARY AND CONCLUSION

The striking similarity between my findings and Spilerman's on conflict, despite dif-

[14] It seems quite reasonable that the first two factors in the regression are related to conflict. One could certainly anticipate that civil rights conflicts are more likely to occur where the number of non-whites is larger. The non-white population provides the manpower resources necessary for demonstrations, marches and the like. And the larger the manpower pool, the greater the resources and the greater possibility for conflict.

Similarly, the relation of "state capital" to reports of conflict is equally reasonable. If a civil rights group is unable to influence the local power structure it seems likely that an attempt will be made to outflank them by going over their heads to the state powers which exercise control on these issues. (See Danzger, 1968b for data suggesting this pattern.) One would therefore anticipate a larger number of demonstrations and related activities in state capitals. The larger number of conflicts reporter for state capitals is thus probably not an artifact of reporting, but rather stems from the political reality that state capitals attract such activity.

Table 4. City Characteristics and Log Conflict Reports: Step-wise Regression for Cities with Populations of 25,000 or More. Only Cities with Ten or More Conflict Reports (N = 83)

	R Square
1. Non-white	.244
2. Capital	.153
3. South/non-South	.037
4. Number of manufacturing establishments employing 25 or more	.010
5. Number employed in public administration	.012
6. AP/UPI[a]	.005
Total variance explained	.518

[a]Six other factors add .051 to R Square.

ferences in operational definition, led to an exploration of the possibility that both sets of findings may have been based on similarly tainted data. Closer examination of the data revealed that all reports of conflict cited were based on wire service stories. Others studying conflict also use this type of data, although frequently under different guises. Thus these same problems face many studies of conflict. Findings on conflict may in fact be artifacts of news publication patterns.

But if this is so, is anything left for the study of conflict? An overview of the literature critical of news reports indicated three major criticisms which we have characterized as (1) inaccuracies and distortions of fact, (2) bias, (3) suppression.

Analysis of the first criticism revealed that it was based on the view of news as a straight line process of communication. Yet in fact news transmission involves networks of communication. The networks are the wire services, which compete with each other utilizing and checking each other. Radio and TV, local newspapers and stringers are a part of this network. Thus inaccuracies which might occur in a given report if observed only by a single individual are corrected by others also observing the phenomenon. A given report may be inaccurate, but this is likely to be corrected quickly once the wire services become involved. The wire services act as an alarm signalling the existence of a story and attracting a number of reporters. The reporters often exchange information with each other, and in the process, reports become more accurate. Errors of fact appearing in a given news report will be contradicted by other reports in the network. Such differences are likely to be reexamined so that these errors will be corrected in subsequent reports. Utilizing a sequence of reports, one is likely to have accurate facts.

The process of news transmission is similar to that of rumor under conditions conducive to accuracy, which is to say under conditions where there are a network of careful evaluators of fact. But it appears that critics have viewed news transmission as similar to rumor in the laboratory, i.e., as a straight line process of communication, without awareness of the correctives built into the network.

The charge of bias in the presentation and placement of the news, on the other hand, is well substantiated. But researchers may use specific facts or hard facts in contrast to interpretations of the facts. The two are distinguishable.

Finally, statistical evidence indicates that suppression does occur. A comparison of cities with and without wire service offices indicates that cities with such offices report more conflicts than do cities without such offices. Moreover, the evidence indicates that this difference cannot be attributed to other factors which might be associated with conflict. This finding is consistent with the work of others and with reports from the field.

Further examination of this pattern, however, reveals that once a number of reports of civil rights conflict have been reported in a given city, it makes no difference whether or not a city has a wire service bureau. Conflict is reported as often for both types. One may conclude that in this situation there is no suppression.

With regard specifically to my own findings and those of Spilerman that number non-white is the critical factor in the occurrence of conflict and riots, that fact continues to be supported by the data. But a new assessment of the number required for a "critical mass" or a "ceiling effect" is required in light of the contamination of the data.

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STRUCTURAL CHANGES IN OCCUPATIONAL MOBILITY AMONG MEN IN THE UNITED STATES*

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There has been no change over time in the relative mobility chances of American men whose fathers held different occupations. Changes in the occupational structure are the only source of systematic variation in rates of intergenerational occupational mobility. Based on this assumption, we have constructed detailed tables of the occupational mobility of U. S. men in 1952, 1962 and 1972. Both between and within cohorts, systematic trends appear which include increasing upward mobility, decreasing downward mobility and, by some measures, a weakening of the relationship between father's and son's occupations. At least for men in the U. S., the shifting occupational structure is the driving force and the problematic issue in changing intergenerational mobility patterns. We suggest that these same ideas be applied in cross-societal research. If there be invariances in the mobility process across societies which match those across time within the U.S., then comparative mobility research should be reoriented to investigate the sources and consequences of transformations of the occupational structure.

In this century there has been essentially no trend in the relative mobility chances of American men whose fathers held differing occupations. To put the matter crudely, but correctly, there has been no change in the odds that a man of low status origin will achieve high rather than low occupational standing relative to the odds that a man of high status origin will achieve high rather than low occupational standing. This striking lack of trend has been documented in log-linear analyses of intergenerational occupational mobility tables for the period from about 1910 to 1970 (Hauser et al., 1975). The same result holds throughout men's work careers, and it holds whether occupations are classified in as few as 3 or as many as 12 categories. Findings from the 1962 "Occupational Changes in a Generation" survey (OCG) might be applied to all of the available trend data:

> We have found remarkable homogeneity in the patterns of association between father's occupation and son's first occupation when changes in the occupational structure have been controlled. There appear to be negligible differences among cohorts in the propensity to move vs. stay; in the propensities to move up, move down, or

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stay; in the propensity to move up relative to the propensity to move down; in the propensity to inherit one's father's occupation; in patterns of movement among similar occupation groups; and in patterns of movement among dissimilar occupation groups.

Regression and correlation analyses of status variables have led to parallel conclusions (Duncan, 1966a; 1968). While traditional contingency table analyses suggest a possible increase in occupational mobility across time (Jackson and Crockett, 1964; Blau and Duncan, 1967:109), we shall offer an explanation of this seemingly exceptional finding.

In the same period that the relative mobility chances of American men have been virtually constant, the occupational structure has completed the transformation from that of an agrarian and industrializing society to that of a metropolitan and technological society (Duncan, 1966b; Hauser, 1969). Of middle-aged men who were in the labor force when the last quarter of the twentieth century began, more than one in five were in salaried professional or managerial occupations, and fewer than one in twenty-five were in farm occupations. Yet, when these men were teenagers, one in twelve of their fathers were salaried professionals or managers, and one in four were farmers. Temporal shifts in the occupation distribution have sustained the chances of American men for upward occupational mobility between generations (Duncan, 1965; Hauser and Featherman, 1973). In the detailed intergenerational occupational mobility table of Blau and Duncan (1967:28), upward mobility exceeded downward mobility between the great majority of pairs of occupations.

Together, the constancy of relative mobility chances and the secular shifts in the occupational division of labor imply that the changing occupational distribution is the major factor effecting trends in the intergenerational flow of manpower among occupations. Sociologists have long recognized that changes in the occupational structure affect mobility patterns. This recognition has generally taken the form of injunctions to control variation in the occupational structure before venturing comparisons between mobility tables and of efforts to construct mobility indexes which would make such comparisons possible. However, once trends in the occupational structure are controlled, there are no trends in the occupational mobility of U. S. men.

This suggests an inversion of the traditional problem in comparative mobility analysis. Rather than treating the underlying process of mobility as a variable in comparative research (across time or across societies) and variation in the distribution of occupations as a disturbance, we suggest that shifts in the occupational structure are both the driving force and the problematic issue in comparative mobility studies. That is, we ask what changes in observed mobility chances are induced by transformations of the occupational structure, such as occur in the processes of urbanization and industrialization. Even though the relative mobility chances of men do not change, it is possible for other important properties of a mobility table to vary systematically with changes in the occupational structure. There is no paradox here; as O. D. Duncan (1966a:76-7) has observed, "It is well to bear in mind the fact that invariance with respect to some aspects of the mobility process is compatible with variation in other aspects."

In this paper, we shall document trends in the intergenerational occupational mobility of American men which have been induced by temporal changes in the occupational distribution. That is, given a fixed pattern of association between father's occupation and son's current occupation, we shall ask what the implications are of changes over time in the marginal distributions of father's and of son's occupations. Our procedure follows a logic similar to indirect standardization or regression standardization (Duncan, 1965; Hauser and Featherman, 1973; 1974a; 1974b). We have applied constant patterns of association between father's and son's occupations (analogous to fixed regression coefficients) to populations of men who differ in the distribution of their own and their fathers' occupations. The one conceptual difference between our method and these other methods of standardization is that we vary composition both in respect to the distributions of origin and destination occupations; these other methods are usually applied to assess the implications of changes in occupational origins for the distribution of destination occupations. Applying a well-known technique of

Table 1. A Hypothetical Occupational Mobility Table

Father's Occupation	Son's Occupation 1	2	Total
1	f_{11}	f_{12}	$f_{1.}$
2	f_{21}	f_{22}	$f_{2.}$
Total	$f_{.1}$	$f_{.2}$	f

proportional adjustment (Mosteller, 1968; Deming, 1943), we have taken tables of U. S. men's mobility from father's occupation to current occupation for certain cohorts covered in the 1962 OCG survey, and we have projected them backward and forward in time to fit occupation distributions of fathers and sons in 1952 and 1972, while preserving the underlying patterns of association in the tables. This procedure is often used by economists to adjust tables of input-output coefficients for short term changes in supply and demand (Theil, 1972:131-143).

Since we are about to obtain a new set of occupational mobility data for the United States (Featherman and Hauser, 1975), our effort to predict or project the entries in occupational mobility tables for U. S. men in 1972 might be viewed more as an exercise in quantitative hypothesis construction than one of finding new social facts. While we think our work is justifiable on the former ground alone, our indirect methods may prove useful for measuring mobility in future decades, particularly if resources should diminish for monitoring social change.

The technique of proportional adjustment can be illustrated in a straightforward way. Consider the hypothetical mobility data in Table 1, where f_{ij} is the frequency of men from origin i in destination j. The association between occupational origins and destinations is given by the odds-ratio,

$$\frac{f_{11}/f_{12}}{f_{21}/f_{22}} = \frac{f_{11}/f_{21}}{f_{12}/f_{22}} = \frac{f_{11}f_{22}}{f_{21}f_{12}} \qquad (1)$$

Odds-ratios like that in equation (1) appear to be invariant across time for U. S. men (Hauser et al., 1975). Suppose the frequencies in interior rows 1 and 2 of the table are multiplied by arbitrary non-zero constants, say, a and b, respectively; likewise, the frequencies in the interior columns of the table are multiplied by non-zero constants c and d. The resulting proportionately adjusted frequencies are shown in Table 2. In general, when both row and column frequencies are adjusted, neither the original row nor column percentage distributions will be preserved. However, this transformation does not affect the odds-ratio,

$$\frac{(acf_{11})(bdf_{22})}{(adf_{12})(bcf_{21})} = \frac{f_{11}f_{22}}{f_{12}f_{21}} . \qquad (2)$$

This illustrates the way in which proportional adjustment preserves the association in a frequency table.

As a practical matter, a frequency table may be adjusted to desired row and column marginal distributions by an iterative method. Each row entry is multiplied by the ratio of the desired row sum to the actual row sum,

Table 2. Proportional Adjustment of a Hypothetical Occupational Mobility Table

Father's Occupation	Son's Occupation 1	2	Total
1	acf_{11}	adf_{12}	$a(cf_{11} + df_{12})$
2	bcf_{21}	bdf_{22}	$b(cf_{21} + df_{22})$
Total	$c(af_{11} + bf_{21})$	$d(af_{12} + bf_{22})$	$acf_{11} + adf_{12} + bcf_{21} + bdf_{22}$

and then each column entry is multiplied by the ratio of the desired column sum to the actual column sum. By alternating adjustments of this kind, convergence of the adjusted cell frequencies to both the desired row and column totals is usually obtained within a few iterations.

Whether or not the model of proportional change is correct, we think it is instructive to estimate the 1952 and 1972 mobility tables from the associations in the 1962 tables for the following reason. The process of proportional adjustment preserves the odds-ratios in the table which is taken as the standard. Thus, when we compare a table adjusted to 1972 (or 1952) marginals with the original table, we know that none of the observed differences may be attributed to change in the underlying pattern of association in the table. All must be attributable to changes in the occupational structure. In discussing proportional adjustment, Mosteller notes (1968:10):

> Making the assumption that when a subgroup is formed from the parent population the multiplicative invariance is preserved does not make it true (as a vehicle for getting an estimate in ignorance, it has the same status as the use of linear regression in the absence of knowledge of the shape of the function).

In the present case, the forward and backward projection of 1962 mobility tables is especially compelling because there is a fair amount of evidence that the tables so projected will be substantially correct (Hauser et al., 1975).

The empirical problem in adjusting mobility tables to reflect the occupational structure at earlier or later times is to obtain the marginal distributions of father's and son's occupations. Our solution is to assume that cohorts covered in the 1962 OCG survey are represented equally well in the U. S. Census and in the Current Population Survey (CPS) in earlier and later years. For example, we take the reports of current occupation of men aged 25 to 34 in the 1952 CPS and of men aged 45 to 54 in the March 1972 CPS to pertain to the same cohort of men as reports of father's occupation and of current occupation of men aged 35 to 44 and covered in the March 1962 OCG survey. Hauser and Featherman (1973; 1974a; 1974b) have reviewed elsewhere the threats to the validity of these assumptions, which include differentials in survey coverage, mortality, immigration and entrances into and exits from the experienced civilian labor force.

Assuming temporally invariant association between father's and son's occupations, we have estimated the frequencies in father-son mobility tables in 1952 and 1972 for ten-year age groups of men aged 25 to 64 in 1962 who were not outside that age range ten years earlier or later. Our estimated mobility tables are based on the 12-category major occupation group classification displayed in Figure 1, which was also used by Hauser et al. (1975). Our assignment of off-diagonal cells to "upward" or "downward" mobility is based on the rank positions of the major groups on the Duncan (1961) scale.

Because the marginal occupation distributions in the March 1972 CPS were based on the occupation classification system of the 1970 Census of Population, while the occupation distributions in the 1962 OCG were based on the 1960 classification system, we have adjusted the observed 1972 occupation distributions for men of different ages to a 1960 basis (Priebe et al., 1972). We did not think changes in occupational classifications between 1950 and 1960 were large enough to warrant adjustment of the 1952 data (Priebe, 1968). However, occupation distributions from the 1952 CPS were not tabulated by age, so we adjusted the 1950 Census table of men's occupations by age to the 1952 marginal distributions of age and occupation (U. S. Bureau of the Census, 1953; 1956). This same procedure was used earlier by Duncan (1965:492-3).

The distributions of son's current occupation by age and year which were used in our analysis are shown in Table 3. The columns of the table are arranged to facilitate age-constant intercohort comparisons, e.g., among men aged 35 to 44 in 1952, 1962 and 1972. However, it is also legitimate to follow the experience of a single cohort through the life cycle. For example, men born in 1917 to 1926 were 25 to 34 in 1952, 35 to 44 in 1962 and 45 to 54 in 1972. Both within and between cohorts the occupation distributions in Table 3 reflect the historic pattern of change in the U. S. labor force: net shifts out of occupations at the base of the status hierarchy and net shifts into occupations near the top of the hierarchy. Even in the relatively

Occupation Group	Duncan SEI
Professional, technical and kindred workers, self-employed	84
Professional, technical and kindred workers, salaried and others	73
Managers, officials and proprietors, exc. farm, salaried	68
Sales workers	49
Managers, officials and proprietors, exc. farm, self-employed	47
Clerical and kindred workers	45
Craftsmen, foremen and kindred workers	31
Operatives and kindred workers	18
Service workers, including private household	17
Farmers and farm managers	14
Farm laborers and foremen	9
Laborers, except farm and mine	7

Figure 1. Twelve Category Classification of Major Occupation Groups

recent period represented by the table, the shifts out of farming and laboring occupations and into professional and salaried managerial occupations are noteworthy. (For further evidence and discussion of net occupational trends, see Duncan, 1965; 1966b; Hauser and Featherman, 1973; 1974a.)

Intercohort shifts among the occupation distributions in Table 3 are summarized by indexes of dissimilarity in the right-hand panel of Table 4. The index of dissimilarity gives the percentage of observations in one distribution which would have to be shifted to other categories to render that distribution identical to a second distribution. Obviously, there are substantial net occupational shifts between cohorts. Even over a single decade, the net shift is seven percent or more of the occupation distribution in all of the six possible intercohort comparisons. Table 4 also gives indexes of dissimilarity between the distributions of father's occupation in successive cohorts. Because there is substantial variability in the age at which fathers have sons, the net shifts are in most cases less between the distributions of father's occupation than between those of son's, but they are not negligible. (For further evidence of intercohort shifts in the distributions of father's and son's occupations, see Hauser et al., 1975.)

In Table 5 we display gross measures of mobility by age and year in the observed and adjusted mobility tables. For example, 79.5 percent of men in the observed 12 by 12 table for men aged 35 to 44 in 1962 were mobile, compared to 76.6 percent mobile in the same table adjusted to the 1952 marginal distributions for men aged 35 to 44 and 80.4 percent mobile in the same table adjusted to the 1972 marginal distributions for men aged 35 to 44. Thus, at ages 35 to 44 intercohort shifts in the occupation distributions of fathers and sons induced an increase in the total rate of mobility. At ages 35 to 44 in 1962, 57.4 percent of men were upwardly mobile, but of men at that age in 1952 we estimate 48.5 percent were upwardly mobile, and in 1972 we estimate 59.3 percent were upwardly mobile. Finally, 22.0 percent of men aged 35 to 44 in 1962 were downwardly mobile, but in our adjusted table for men of that age in 1952, 28.1 percent were downwardly mobile, and 21.1 percent were downwardly mobile in 1972. Not only did shifts in the occupational distribution induce a higher rate of mobility among 35 to 44 year old men in more recent years, but they increased the rate of upward mobility and decreased the rate of downward mobility. Moreover, these intercohort trends among 35 to 44 year olds are replicated with only one exception among men at other ages;

Table 3. Occupation by Age: U. S. Men in the Experienced Civilian Labor Force at Selected Ages in 1952, 1962 and 1972*

	Age and Year									
	25 to 34		35 to 44			45 to 54			55 to 64	
Occupation	1952	1962	1952	1962	1972	1952	1962	1972	1962	1972
Prof., tech., & kindred, self-employed	0.9%	1.0%	1.6%	2.0%	2.8%	1.6%	1.5%	2.0%	1.9%	1.6%
Prof., tech., & kindred, salaried & other	7.8	17.2	6.2	11.1	15.4	5.1	7.9	11.2	7.6	9.0
Mgrs., off., & prop., exc. farm, salaried	4.5	7.1	6.4	9.6	9.9	6.9	8.5	9.6	9.7	9.9
Sales workers	6.1	5.9	5.2	5.3	5.8	5.0	5.2	6.0	3.9	6.1
Mgrs., off., & prop., exc. farm, self-emp.	3.8	3.4	6.5	7.6	5.8	7.7	10.1	5.7	10.3	5.8
Clerical & kindred workers	7.8	7.4	6.1	6.4	6.0	6.1	6.9	6.8	5.8	6.7
Craftsmen, foremen & kindred workers	19.2	19.1	20.3	21.5	21.9	20.7	22.2	23.7	19.5	21.8
Operatives & kindred workers	24.8	20.9	21.8	19.0	19.0	18.1	17.8	18.9	15.6	17.7
Service workers, inc. private household	4.6	5.1	5.4	4.3	5.2	6.8	5.7	6.3	7.5	8.6
Farmers & farm mgrs.	6.5	3.3	8.3	5.2	2.5	9.6	6.8	3.5	9.7	5.2
Farm laborers & foremen	5.0	2.5	3.9	1.4	1.0	3.7	1.4	0.8	2.3	1.6
Laborers, exc. farm & mine	9.1	7.2	8.5	6.6	4.9	8.6	6.0	5.6	6.3	6.1
Total	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

*In 1962, men who did not report their father's occupation in the OCG survey are excluded. Rounded percentages may not add to 100 percent. See text for sources of data.

Table 4. Indexes of Dissimilarity between Cohorts in Distributions of Father's Occupation and of Son's Current Occupation: U. S. Men at Selected Ages in 1952, 1962 and 1972

		Father's Occupation		Son's Current Occupation	
Age	Year	1962	1972	1962	1972
25 to 34	1952	8.6	--	12.6	--
35 to 44	1952	5.0	12.1	11.3	16.0
	1962	--	8.6	--	7.0
45 to 54	1952	7.7	12.2	9.2	14.6
	1962	--	5.0	--	8.7
55 to 64	1962	--	7.7	--	10.0

the percentage downwardly mobile at ages 55 to 64 in 1972 is inconsistently large, 22.7 percent compared with the observed 22.3 percent at the same ages in 1962.

With the exception just noted, there are similar intracohort increases in total and upward mobility and decreases in downward mobility. These may be observed by reading the diagonal entries of Table 5 from upper left to lower right. For example, in the cohort of 1917 to 1926 (aged 25 to 34 in 1952), 77.1 percent were mobile in the estimated 1952 table, 79.5 percent were mobile in the observed 1962 table and 82.1 percent were mobile in the estimated 1972 table. In the same cohort, 45.7 percent were upwardly mobile in 1952, 57.4 percent were upwardly mobile in 1962 and 60.4 percent were upwardly mobile in 1972. Further, in that cohort, 31.4 percent were downwardly mobile in 1952, 22.0 percent in 1962 and 21.7 percent in 1972.

While these intracohort comparisons are strictly correct, it should be kept in mind they do not have the same logical status as the intercohort comparisons, relative to the distinction between structure (marginals) and process (association) in the mobility table. That is, in making intercohort comparisons we look at two tables which share the same underlying pattern of association, but have differing origin and destination marginals. In making intracohort comparisons, we look at two tables which share the same origin distribution, but differ both in respect to patterns of association and destination distri-

Table 5. Gross Measures of Occupational Mobility from Father's Occupation to Son's Current Occupation for U. S. Men by Age: from Mobility Tables Observed in 1962 and Adjusted to 1952 and 1972 Marginal Distributions

	Total Mobile			Upwardly Mobile			Downwardly Mobile		
Age	1952	1962	1972	1952	1962	1972	1952	1962	1972
25 to 34	77.1	79.6	--	45.7	53.8	--	31.4	25.8	--
35 to 44	76.6	79.5	80.4	48.5	57.4	59.3	28.1	22.0	21.1
45 to 54	78.2	79.9	82.1	50.6	57.6	60.4	27.5	22.3	21.7
55 to 64	--	78.3	81.0	--	56.0	58.3	--	22.3	22.7

butions. As Hauser et al. (1975) have shown, the differences among age groups in the association of father's occupation and son's occupation are not very great, but any such differences will enter intracohort comparisons. Both intercohort and intracohort comparisons are of substantive interest, but only the former unequivocally represent our interest in measuring the effects of changes in the occupational structure.

The structurally induced changes in mobility rates within and between cohorts are not very large. Only two of the ten-year intracohort shifts in occupational mobility are as large as ten percentage points, and many of the changes between adjacent years or cohorts are only one or two percentage points. At the same time, the pattern and consistency of the estimated changes in mobility rates convince us that we have located a real and substantial trend.

One might easily be misled in attempting to infer trend in Table 5 from cross-sectional age comparisons within any one year. For example, in 1952, the rate of upward mobility varied directly with age, and the rate of downward mobility varied inversely with age. Thus, by identifying the experience of older cohorts with the past and that of younger cohorts with the present, one would reach precisely the wrong conclusions about trend.

In one sense, there is no need for a more refined analysis of our data. The proportional adjustment method guarantees that changes in the occupational structure are the only source of variability between cohorts for men of any given age. Thus, even the gross mobility rates in Table 4 have been purged of extraneous variation in the mobility process. At the same time, we thought there were substantive and methodological reasons for carrying our analysis further. For example, an intergenerational occupational mobility table is not a transformation of the labor force from one period to another, and differences between the marginal distributions of fathers and sons reflect rates and age-variations in nuptuality, fertility, mortality and labor force participation, as well as interperiod changes in the occupational structure. Thus, our results might simply reflect changes in the disjuncture between the occupation distributions of fathers and sons. Also, we thought it would be instructive to see whether some descriptively interesting mobility indexes would display the insensitivity to changes in the occupational structure which have been claimed for them.

The first panel of Table 6 gives percentages of structural (or minimum) mobility for each cohort-year combination. These are indexes of dissimilarity between the occupation distributions of sons and their fathers. They indicate the percentage of sons who could not possibly have entered their fathers' occupations, but in no other way do they describe the actual distribution of long- and short-distance mobility. Of course, the measurement of structural mobility is unrelated to our adjustment procedure, since it only involves the marginal distributions of the mobility tables for each cohort and year. However, the interpretation of the indexes does depend on our assumptions about survey coverage of 1962 OCG men in earlier and later years.

Neither within nor between cohorts is there a consistent temporal pattern of variation in the volume of structural mobility. For some ages or cohorts structural mobility increases with time, for others it decreases, and in still others there is no consistent trend. Indeed, the only reasonably clear pattern is a tendency for structural mobility to vary directly with age within years. Clearly, no systematic trends in structural mobility are responsible for the pattern in the data of Table 5.

The second panel of Table 6 gives percentages of circulation mobility in the observed and estimated tables. Circulation mobility is the excess of total mobility over structural mobility; i.e., it tells what percentage of men moved whose moves were not dictated by the transformation of the marginal distributions between generations. These coefficients come closer than those of structural mobility to displaying the pattern of total and upward mobility rates in Table 5, but there are several exceptions to the temporal increase in circulation mobility. At ages 45 to 54, there was a slight decrease in circulation mobility between 1962 and 1972. Also, in the cohorts of 1907 to 1916 and of 1917 to 1926 there were declines of circulation mobility between 1962 and 1972.

Numerous indexes have been proposed to eliminate the effects of changes in the occupational structure in the measurement of mobility. Given the construction of our data, an index with this desirable property would not vary across years for men of any given

Table 6. Net Measures and Indexes of Occupational Mobility from Father's Occupation to Son's Current Occupation for U. S. Men by Age: from Mobility Tables Observed in 1962 and Adjusted to 1952 and 1972 Marginal Distributions

Age	Structural Mobility			Circulation Mobility			Yasuda Index[a]			Boudon Index[a]		
	1952	1962	1972	1952	1962	1972	1952	1962	1972	1952	1962	1972
25 to 34	24.6	23.1	--	52.5	56.4	--	.821	.857	--	.696	.734	--
35 to 44	24.4	24.2	23.6	52.3	55.3	56.8	.817	.852	.896	.691	.730	.743
45 to 54	29.0	26.0	28.7	49.1	53.9	53.3	.826	.858	.881	.692	.728	.748
55 to 64	--	28.5	29.4	--	49.8	51.6	--	.826	.859	--	.696	.731

[a]See text for explanation of Yasuda's and Boudon's indexes.

age. Two of the proposed mobility indexes are formed as the ratio of circulation mobility to a hypothetical maximum value. Yasuda (1964; also see U. S. Bureau of the Census, 1964; Durbin 1955; Jones, forthcoming) suggests that the difference between mobility under the hypothesis of statistical independence and structural mobility be placed in the denominator of the index. Thus, the index says how close observed circulation mobility would be to circulation mobility if there were no relationship between father's and son's occupations, conditional on the observed marginal distributions.

Boudon (1972; 1973) has proposed a modification of Yasuda's index in which maximum mobility is substituted for mobility under independence in the denominator of the ratio. For mobility tables based on more than two occupational categories and where the marginal distributions are not highly skewed, maximum mobility is always unity. That is, it would be possible for no son to remain in his father's occupation. Jones (forthcoming) has characterized the Yasuda and Boudon indexes as based on "liberal" and "radical" normative criteria, respectively; the former takes chance association between father's occupation and son's occupation as the standard, and the latter takes maximum dissociation as the standard.

The Yasuda and Boudon indexes are based in common on the notion that subtracting structural mobility from observed and from ideal mobility (under independence or dissociation) controls or eliminates the influence of the occupational structure. However, as shown in the last two panels of Table 6, both the Yasuda and Boudon indexes show virtually the same pattern of variation across ages and years as do the gross measures of total mobility and upward mobility in Table 5. As indexed by these two measures, by "liberal" or "radical" criteria of evaluation, the mobility of American men has increased in the past twenty years. While these two indexes obviously do not eliminate the effects of changes in the occupational structure, we would not thereby disregard the results of Table 6. Despite their methodological defects, these indexes do have descriptive value, and we believe their empirical variation reflects substantively important consequences of change in the occupational structure.

Are these trends peculiar in some sense to

the measures of mobility we have thus far reported? In order to be sure this was not the case, we reanalyzed our data using several of the better-known indexes of mobility, including some which pertain to individual cells or occupational categories in a mobility table. Among the measures we looked at were the social distance mobility ratio (Rogoff, 1953; Glass, 1954) and the "percentage determinations" suggested by Hope (1972). These additional analyses were consistent with those we have already reported, but we shall not detail them for reasons of economy. We did carry out three additional analyses which we think are instructive and which can be summarized briefly.

Recently, Klatzky and Hodge (1971) and Hope (1972) have suggested that canonical correlation analysis be used to describe the strength of association in a mobility table. The canonical correlation is determined by assigning that set of scale values to origin and destination categories which yields the largest product-moment correlation between father's and son's occupations. Additional canonical correlations may be extracted if one set of scores is insufficient to represent all of the association in the table, and the evidence suggests that at least two canonical correlations are needed to describe the association in occupational mobility tables. The sum of squares of all possible canonical correlations in a table is equal to the mean square contingency, formed as the ratio of χ^2/N, where χ^2 is the usual Pearson Chi-square and N is the number of observations on which the table is based.

The first and second canonical correlations and the mean square contingencies of our observed and adjusted mobility tables are reported in Table 7. Without exception, these three measures of association decline with the passage of time both within and between cohorts. In some cases these declines are substantial. For example, among 35 to 44 year old men, the first canonical variate in father's occupation accounted for 24.5 percent of the variance in the first canonical variate in son's occupation in 1952. Twenty years later among men of the same age, the first canonical variate in father's occupation accounted for but 19.4 percent of the variance in the first variate of son's occupation. Thus, these correlational measures suggest a tendency for the mobility of U. S. men to increase in recent years simply because of the pattern of change in occupational structure.

McFarland (1969) has advocated an index of uncertainty (information or entropy) to measure the "permeability" of occupational structures, which we take to be the converse of the dependence of son's on father's occupation. Entropy is defined as

$$H = -\Sigma\, p_i \log(p_i) \qquad (3)$$

where p_i is the relative frequency of observations in the ith category of a distribution, so $\Sigma\, p_i = 1$. If we adopt the convention $0 \log 0 = 0$, H is always positive or zero, and it takes on the maximum log K when there is a uniform distribution of observations across K categories. That is, the

Table 7. Canonical Correlations and Mean Square Contingency in Matrices of Mobility from Father's Occupation to Son's Current Occupation for U. S. Men by Age: from Mobility Tables Observed in 1962 and Adjusted to 1952 and 1972 Marginal Distributions

	First Canonical Correlation			Second Canonical Correlation			Mean Square Contingency		
Age	1952	1962	1972	1952	1962	1972	1952	1962	1972
25 to 34	.491	.451	--	.300	.275	--	.446	.382	--
35 to 44	.495	.459	.441	.313	.281	.252	.445	.382	.355
45 to 54	.467	.434	.430	.300	.280	.243	.373	.329	.308
55 to 64	--	.441	.409	--	.259	.232	--	.366	.324

more uniform or heterogeneous a distribution, the more closely H approaches its maximum value.

In Table 8 we show the uncertainty in the marginal distribution of son's occupation for each year and cohort covered in our analysis, and we also show an average measure of the conditional uncertainty in son's occupation. That is, the latter measure is the expected value (weighted by the marginal distribution of father's occupation) of the uncertainty of the distribution of son's occupation for each category of father's occupation. In calculating H, we have taken natural logs, so the maximum of each entry in Table 8 is $\log_e 12 = 2.48$. Between cohorts, but not within them, uncertainty in the distribution of men's occupations has declined. That is, the occupation distribution has become less heterogeneous. There is less consistent evidence of trend in the conditional uncertainty measures. These coefficients vary directly with age in each year, and excepting the estimate for 45 to 54 year old men in 1972, conditional uncertainty increases with age within cohorts. However, there is no consistent pattern of variation in conditional uncertainty in the age-constant intercohort comparisons. In the right-hand panel of Table 8, we give the percentage reduction in uncertainty when father's occupation is known. This is 100 percent minus conditional uncertainty as a percentage of total uncertainty. The larger the percentage reduction in uncertainty, the greater is the dependence of son's on father's occupation. Taken in combination, the declining total uncertainty between cohorts and declining conditional uncertainty within cohorts produce a consistent pattern of decline in the percentage reductions both within and between cohorts. That is, knowledge of father's occupation gives us relatively less information about son's occupational position more recently than in the past.

Suppose we think of occupational mobility as a probability process (Hodge, 1966), and specifically as a Markov process, so the occupation distribution k generations hence may be obtained by multiplying the vector of father's occupations by the kth power of the matrix of row (outflow) probabilities estimated from a mobility table. Of course, an intergenerational mobility table does not literally represent an interperiod population transformation because of differential fertility and the variance in ages at which fathers have sons, and fathers and sons enter and leave the labor force. See Duncan (1966a) for an elaboration of these observations.

The equilibrium vector or fixed point distribution of an outflow matrix is obtained by forming the matrix of relative frequencies of son's occupation within categories of father's occupation and raising that matrix to a high power. The distribution of relative frequencies across any row of the resulting matrix is the equilibrium vector. An equilibrium vector may be thought of as the end-state of a Markov process, an origin vector which would be transformed back into itself by another step in the process. If one chooses not to take the Markovian assumptions seriously, the equilibrium vector may be taken as a convenient summary of tendencies in the mobility table.

Table 8. Marginal and Conditional Uncertainty in the Occupation Distributions of U. S. Men by Age: from Mobility Tables Observed in 1962 and Adjusted to 1952 and 1972 Marginal Distributions

Age	Uncertainty in Destination			Conditional Uncertainty in Destination			Percentage Reduction in Uncertainty		
	1952	1962	1972	1952	1962	1972	1952	1962	1972
25 to 34	2.222	2.208	--	2.030	2.035	--	8.6	7.4	--
35 to 44	2.268	2.248	2.201	2.074	2.080	2.047	8.6	7.5	7.0
45 to 54	2.298	2.259	2.210	2.125	2.109	2.069	7.5	6.7	6.3
55 to 64	--	2.311	2.267	--	2.146	2.124	--	7.2	6.3

In Table 9, we give indexes of dissimilarity between cohorts in the equilibrium vectors of the outflow tables which were observed in 1962 and estimated for 1952 and 1972. Detailed comparisons of the equilibrium vectors within and between cohorts would lead to conclusions similar to those which might be drawn from direct examination of the changing destination vectors of the mobility tables. However, the important conclusion to be drawn from these projections is adequately documented by the indexes of dissimilarity in Table 9. That is, the equilibrium vectors do change substantially between cohorts because the distribution of occupational destinations changes. That result dramatically illustrates the difference between the assumption of constant transition probabilities in the Markov model, and the assumption of constant odds–ratios which is supported by the analysis of Hauser et al. (1975) and has been applied throughout this paper. It is implicit in the changing equilibrium vectors that the observed intergenerational mobility tables could not have been generated by a single Markov process.

Our analysis carries implications for efforts to estimate future occupation distributions with projections based on an observed occupational mobility matrix. Even if the relative mobility chances of men with differing occupational origins were constant over time, the distribution of outflow probabilities would be subject to change so long as the occupational structure is changing. We cannot expect transition probabilities to remain constant over time. However, only by making the assumption of constant outflow probabilities can future occupation distributions be projected from a mobility table. Thus, the invariance of relative mobility chances is insufficient to permit estimation of future occupation distributions by projection of observed mobility tables.

Table 9. Indexes of Dissimilarity between Cohorts in Equilibrium Vectors of the Age Occupation Distribution of U. S. Men by Age: from Mobility Tables Observed in 1962 and Adjusted to 1952 and 1972 Marginal Distributions

Age	Year	1962	1972
25 to 34	1952	19.5	--
35 to 44	1952	16.5	23.2
	1962	--	7.6
45 to 54	1952	11.4	14.4
	1962	--	7.6
55 to 64	1962	--	8.2

One might use other methods to forecast future occupation distributions. For example, one can project future industrial growth and then aggregate industry-specific occupation distributions across industries. Once projections of the occupation distribution were obtained, it would be possible to estimate all of the entries in a future mobility table using the method applied here. Were there published forecasts of the distribution of occupation by age, we could forecast the future of occupational mobility.

Discussion

We began with the finding of Hauser et al. (1975) that there has been no trend whatever in the relative mobility chances of American men whose fathers held different occupations. Taking that finding as an assumption, we used the observed intergenerational occupational mobility tables for men at several ages in 1962 to estimate the frequencies in similar tables which might have been observed in 1952 and 1972. In fact, less detailed mobility tables based on small samples are available for both those years (NORC, 1972; Jackson and Crockett, 1964), and the assumptions underlying our estimation procedure are consistent with comparisons among the observed 1952, 1962 and 1972 tables (Hauser et al., 1975).

Using our observed and constructed mobility tables, we carried out an analysis of mobility trends between and within cohorts of U. S. men which have been induced by changes in the occupational structure. Our analysis pertains to the period since World War II, so the processes of industrialization and urbanization must be regarded as essentially complete throughout our series of observations. Despite the limited temporal scope of our inquiry, there appeared a remarkably consistent set of trends in occupational mobility both within and be-

tween cohorts. In our time series, with few exceptions the total rate of occupational mobility increased, upward mobility increased, downward mobility decreased, and by several measures the relationship between the occupational positions of fathers and sons became less strong. Recall, however, that each of these results presumes a fundamental invariance in the relative mobility chances of American men.

How seriously shall we take these findings? Are they established social facts or reasonably well-grounded hypotheses, or do they merely illustrate a tendency in the occupational structure? Obviously, each reader must judge that from the available evidence. We think the present results are of more than theoretical or analytic interest, and fortunately they may soon be disconfirmed or substantiated by data from the 1973 replicate of the 1962 OCG survey (Featherman and Hauser, 1975).

At the very least, we think our analysis seriously calls into question the thesis of Raymond Boudon's (1974) monograph on mobility in Western societies that the rigidity of occupational structures has led inexorably to constant rates of mobility and to a predominance of downward mobility. On the contrary, at least in the United States the occupational structure has changed continually between and within cohorts, and in consequence of these structural shifts, upward intergenerational mobility appears to have increased and downward mobility to have decreased over time.

It is sobering that the trends toward greater occupational mobility which we have documented presume a fundamental invariance in processes of occupational mobility. Despite the many social changes in the United States in the last two decades, it is a more favorable occupational structure, and only that, which has sustained or improved the mobility opportunities of American men. In light of this finding, it may be discomforting to ponder the social consequences of economic, technological or demographic impediments to further upgrading of the occupational structure.

Whether or not our specific findings are ultimately found valid, we think our analysis carries important implications for the conduct of comparative mobility research. Students of occupational mobility may have placed disproportionate emphasis on differences in mobility processes across time and space. Important and systematic variations in mobility rates may be induced by changes in the occupational structure. These tendencies may be measured even with rather primitive analytic tools, such as those employed herein, and their causes and consequences seem worthy of further study.

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ORGANIZATIONAL DOMAINS*

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It is argued that intangible claims to domain affect the behavior of administrative bureaus. Data from a panel study of agencies with primary financial responsibility for cities, counties and states show the following: (1) bureaus claiming narrow domains and performing essential services remain stable over time; (2) bureaus claiming broad but inconsistent domains contract and lose responsibilities with environmental demand and competition; (3) bureaus claiming broad domains and having the advantages of newness and technological advance expand and augment responsibilities; (4) despite declining resources and responsibilities, claims to domain rarely contract.

The study of organizational domains is an important yet elusive subject. Conventionally, domain is defined as the technology employed, population served and services rendered by an organization. The task environment is the obverse of domain—literally everything else. Domain consensus is a "set of expectations both for members of an organization and for others with whom they interact, about what the organization will and will not do" (Thompson, 1967:26-9). These definitions, while an improvement over simplistic notions of organizational goals, leave considerable ambiguity, and the range of organizations to which they apply is also unclear. The ambiguity occurs because domains and domain consensus are not static. An organization's products and clientele may change over time, and these changes may occur due to organizational action or shifts in the environment. In other words, domains may be either independent or dependent variables. Concern as to whether concept of domain applies to all organizations arises because some, especially administrative bureaus, do not have tangible products. Instead, they make decisions. One can easily speak of a firm's share of the market, but the arena of action reserved to a public bureaucracy is not so easily determined. Indeed, statutes establishing administrative bureaus can be deliberately vague.

In this paper, organizational domains will be investigated by studying patterns of change among some public bureaucracies over a six-year interval. The agencies are departments of finance, comptrollers' offices, departments of administration and the like in some 227 cities, counties and states. They will be described in detail presently. The analysis hinges on some assumptions about the behavior of public bureaus which require elaboration and justification. One assumption is that because the concept of domain is ambiguous in several respects for these agencies, only the effects of claims to domain can be examined. The second is that organizational names often embody their claims. The first assumption can be defended on theoretical grounds, but the second requires some historical and anecdotal information about the organizations studied.

DOMAINS OF PUBLIC BUREAUCRACIES

The distinction between bureaucratic and non-bureaucratic organizations has all but disappeared from contemporary organization

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theory. Weber's (1947) classic essay on bureaucracy, of course, denies fundamental differences between business firms and public agencies. The modern forms of both—the capitalist enterprise, large-scale public administration—are seen as similarly organized, hence similarly bureaucratic. Other theorists, neo-classical economists and political scientists whose work has not been considered carefully by sociologists, disagree strongly. The differences between profit and non-profit organizations, those which enter into voluntary *quid pro quo* transactions and those which do not, are regarded as basic (Von Mises, 1944; Downs, 1967:25). The former can evaluate their performance while the latter cannot, it is claimed; hence, profit organizations will always be more efficient than other types. Attempts to synthesize bureaucratic theory with the theory of the firm have only buried the issue further. Thompson (1967), for example, treats all "instrumental" organizations, bureaucratic and non-bureaucratic alike, as if they were equally constrained to minimize uncertainty and coordination costs. These needs account for behavior aimed at insulating "core technologies" from the environment and for the development of organizational hierarchies.

Recent research on organizations has hardly improved matters. To be sure, there is agreement that the structural features of organizations are not as consistent as a misreading of Weber's ideal-typical model of bureaucracy might suggest they should be (Udy, 1959; Stinchcombe, 1959; Hall, 1963). But few investigators have been concerned with differences between bureaucratic and profit organizations hypothesized by the neo-classical theorists. (An important exception is Niskanen, 1971.) Most research studies treat only single types of organizations whether bureaucracies or firms (see, for example, Blau, 1970; Blau and Schoenherr, 1971; Hall et al., 1967; Hage and Aiken, 1967a; 1967b; Pondy, 1969) or do not discuss systematically differences between the two types (see Pugh et al., 1969; Hickson et al., 1969 and Blau, 1972, for example).

The inattention to possible differences between bureaus and firms is unfortunate for several reasons. First, it leads to overgeneralization. Concepts from one type of organization are extended to other types with wholly different purposes. Second, important nuances of organizational behavior are overlooked because they are peculiar to one type. Just as firms operate in markets, bureaus are embedded in larger governmental structures; one would expect the relevant dimensions and effects of the environment to be dissimilar. Third, some questions bearing on public policy cannot even be asked if differences in type are ignored. Of particular importance is whether certain services are best provided by firms or bureaus. The present research does not speak directly to this question, but it suggests strongly that the behavior of bureaus is affected by intangible factors which would not influence firms.

Differences between firms and public bureaucracies are especially salient when one considers the concept of organizational domains. For firms, domains can be quite precarious. A fickle clientele and uncontrollable market conditions are sources of uncertainty which demand organizational responses. One would want to study firms' strategies for stabilizing domains and establishing domain consensus. For bureaus providing public goods, there is less uncertainty. The clientele is fixed—the citizens of the jurisdiction served (or sixth graders, or the indigent and so forth). Since there is no market for public goods, it is difficult to prove inefficiency, and relatively few bureaus are put out of business for this reason. The absence of a market does not mean that bureaus lack competitors, however. Though the clientele is assured, the right to monopolize certain programs and activities of government is not. Bureaus compete with one another for "turf" or domain through a political rather than through a market process. A key element in this competition is the way programs are articulated or claims to domain asserted. The outcome of competition is allocation of functions and resources among bureaus. The crucial difference between firms and bureaus is this: whereas firms strive to maintain or expand domains by competing successfully in the market, bureaus use claims to domain which cannot be tested in the market to maintain or expand their activities. One would thus want to study the effects of different claims to domain on the distribution of activities and programs among public bureaucracies.

The argument that bureaus use claims to domain to maintain or expand resources

acquires more force if we examine the history of the Municipal Finance Officers Association of the United States and Canada, the professional association to which most of the agencies studied here belong. MFOA was established in 1906 as the National Association of Comptrollers and Accounting Officers. In 1927, the Association's name became the International Association of Comptrollers and Accounting Officers in recognition of Canadian members. A permanent secretariat and headquarters were established in Chicago in 1932, and the name was changed to the Municipal Finance Officers Association of the United States and Canada. The 1906 constitution of the National Association of Comptrollers and Accounting Officers described its purposes as follows:

> The object of this Association shall be the consideration of improved methods of public finance and the extension of the movement for the installation of a uniform system of state and municipal accounting and reporting throughout the United States, and the promotion of legislation toward these ends.

The current bylaws of MFOA list many more purposes:

> Its object shall be to improve methods of public finance, including: to extend the movement for adequate procedures of accounting, budgeting and financial reporting by state, provincial and local government; to encourage the use of common terminology, classifications and principles relating to these subjects; to develop general principles of economy and efficiency in state, provincial, and local government administration; and to bring about the enlistment and training of qualified public officials and employees.

Other changes occurred when the new name was adopted in 1932. Membership classes were added, and the Association's quarterly publication, *The Comptroller,* adopted the title of *Municipal Finance.* It is now *Government Finance.*

Documentary evidence offers little explanation for the switch from the old International Association of Comptrollers and Accounting Officers to the Municipal Finance Officers Association. No doubt, the original objectives of the Association, accuracy and uniformity of accounting, had been achieved to some extent by the early 1930s. The problems of local government administration were perceived as broader, not limited to accounting but also including revenue collection and budgeting. A parallel situation exists today. There is active discussion of rewriting the bylaws of the MFOA and changing the Association's name. The outcome of this process is by no means certain, but it is likely that the objectives of the MFOA will be broadened to encompass improvement of information systems for local governments and implementation of modern budgeting techniques which take account of policy outputs as well as fiscal inputs. Some proposed bylaws which have not yet been considered by the membership would revise the objectives of the Association to include:

1. To identify principles of economy and efficiency in state, provincial and local government.
2. To develop and encourage the use of uniform standards and procedures of public finance management.
3. To provide for the professional development of public finance managers and to assist individuals in seeking a public finance career.
4. To resolve issues of public fiscal policy and to inform those persons interested in such issues.
5. To develop, improve and publish a body of knowledge in public finance management.
6. To extend cooperation and assistance to other associations and professional organizations concerned with public finance management.

The last three are new. No new name has been settled upon yet, but if there is a change, the likely candidate is Government Finance Management Association.

The pending changes in MFOA's bylaws and names are by no means accidental. They are intended as a response to the removal of resources and responsibilities from finance departments which is widely felt by the membership and was documented in an earlier article (Meyer, 1975). By changing the name and bylaws, the leadership of the association hopes to redefine the functions of local government finance officers and prevent further contraction of the finance officer's role. The hope is not without foundation. In

the past, changes in the MFOA have anticipated changes in finance agencies. Indeed, there is a striking parallel between the various names of the Association since 1906 and the evolution of forms of finance administration. Just as the Association has moved from one of comptrollers and accounting officers to finance officers and may soon become one of finance managers, the dominant organizational forms for the finance function have been comptrollers' offices, finance departments and departments of administration. The simplest form is the comptroller's, (or auditor's, or accountant's) office which used to be universal; the most common pattern is the finance department which nowadays is also the most precarious; and a relatively new but rapidly spreading model of organization is the department of administration. All three deserve detailed descriptions which are drawn from both formal interviews and informal conversations with finance officers throughout the United States.

The Comptroller's Office

One city comptroller suggested that the organization of his office reflected a "Depression mentality," and most of his colleagues agreed. By this he meant that the comptroller's job is regulation of expenditures—limiting budget requests, limiting disbursements to amounts and for purposes budgeted, debiting accounts when obligations are incurred (as opposed to when paid) and minimizing long-term indebtedness. Some of the more aggressive comptrollers conduct detailed audits of local agencies which generate some savings and, occasionally, publicity. The comptroller's main activity, then, is bookkeeping. He is involved in policy only when fiscal safeguards and regulations are needed. Not surprisingly, a comptroller's outlook is like that of a traditional accountant. Financial data are examined for internal consistency and are not linked directly, if at all, to the level of government services. Thus the domains claimed by comptrollers are not large, but what they do is essential and cannot be reduced much. All of this would suggest little change in comptrollers' offices over time save for a growth trend due to accounting requirements of new programs, particularly Federal categorical grants and now, revenue sharing.

The Finance Department

If the comptroller's office is a creature of the Depression, then the finance department reflects the perspective of the 1940s and 1950s. The finance director, like the comptroller, is responsible for accounting and auditing of government funds, but these functions are subsumed under an organizational structure which includes a number of other activities such as revenue collection, purchasing, treasury management, investments and the like. While the finance director examines all expenditures to make sure that they conform to budget requirements, he is often more concerned with finding the resources needed for local government services. The finance director issues all bonds and other obligations. Part of his job is to develop close ties with the banking and investment communities to ensure that funds will always be available when needed.[1] Needless to say, the finance director is more involved in policy than the typical comptroller, for he must determine whether funds can be raised for proposed projects and then find the money. This is especially the case where he has charge of budgeting.

The finance department was the logical outgrowth of the comptroller's office, and its domain was initially well defined. Almost all fiscal activities were consolidated in a single office, and questions concerning fiscal policy were meant to be referred to the finance director. By the 1960s, however, the distinction between financial administration and the non-financial aspects of local government began to be questioned and it has all but evaporated now. This is due in part to the replacement of line-item budgets which take no account of policy outputs with program or performance budgets which link outputs to expenditures. The introduction of large-scale computing has also blurred the line separating fiscal management from operations. Data once used only for financial purposes now have

[1] Local government obligations are, of course, exempt from Federal taxation and yield somewhat lower interest (currently around 7 per cent) than other bonds. There is always the temptation to borrow excessively and invest the surplus at a high interest rate; this is called arbitrage. Treasury regulations prohibit it, and the successful finance director approaches but never crosses the fine line between shrewd money management and arbitrage.

other applications. For example, assessment records showing dwelling type and construction (as well as value) are helpful to fire departments in dispatching equipment. Records of traffic citations are used to measure patrolmen's productivity as well as to dun violators. As local governments move toward integrated data bases and financial data are merged with other kinds of information, the separation of finance from other administrative activities becomes even more tenuous.

Modern budgeting and computing techniques have also created difficulties within finance departments. Both involve guesswork and uncertainties which are anathema to the traditional accountant. What was a set of complementary functions in the 1950s may now be perceived as incompatible, especially to finance officials who believe their main job is to keep track of every cent of public funds. The declining viability of the idea of finance administration as opposed to administration in general and the strains between traditional accounting and innovative computer techniques suggest that the actual domains of finance departments may be contracting whatever their claims.

The Department of Administration

Departments of administration are relatively new. Almost all have been formed since 1960, and many comptrollers' offices now reorganizing switch to the administration rather than the finance model. The department of administration completes the subordination of accounting and auditing activities to other housekeeping functions, especially data-processing. Unlike finance, the concept of administration leaves no ambiguity about the location of the computer and budgeting. Both fit squarely within its domain, as administration connotes linkage of financial with non-financial data – of cash inputs with policy outputs. The personnel function is usually placed in administration along with property management and other activities which do not fit clearly in one of the operating departments of government. Indeed, his scope of responsibilities can make the director of administration a second city or county manager.[2] The domains of administration departments are broad and expanding at the present time because of their newness, incorporation of computer technology and modern budgeting concepts. Whether administration departments will develop the same internal strains as departments of finance remains to be seen. Most likely they will, but not for some time.

The domains claimed by comptrollers' offices, finance departments and departments of administration, then, are quite different though they share primary financial responsibility for their cities, counties and states. Diverse beliefs about the role of the chief fiscal officer underlie these differences in organization. The comptroller's function is primarily regulatory and his domain is narrow but well defined. The finance director is meant to manage all fiscal activities; his objectives are broader, less well defined and his domain is subject to erosion as the separation of financial from non-financial administration crumbles. The director of administration links financial with other administrative activities. What he does is not much different from what many finance directors would like to do, but he has established a claim to do much more; hence, administration departments are now expanding.

Perhaps several more observations about the link between organizations names and claims to domain are in order. First, finance agencies do not change their names casually. A legal action, usually enactment of an ordinance or statute, is required to name a new agency or change the name of an existing one. This means that organizational purposes will be scrutinized and some consistency between names and the scope of responsibilities claimed will be expected. Second, organizational names may affect the range of decisions in which a department head participates. A comptroller is asked to certify whether a proposed expenditure is allowed by the budget; a finance director's responsibility is to raise the money if it is not budgeted; and a director of administration may be consulted to determine whether the project is the most cost-effective means of attaining long-run objectives. In other words, people outside an agency will associate a name with a domain. Third, certain names and labels also reflect commitments to modern administration which is presumed to be able to do things

[2] Several states also have departments of administration. They are similar to the Office of Management and the Budget at the Federal level.

old-fashioned organizations cannot accomplish. "Administration" is the most contemporary label. What is modern at one point in time may be eclipsed later on, however. A final observation is that direct measurement of claims to domain would require intensive case studies of individual organizations which would exhaust most researchers and research budgets. Names are only imperfect indicators of claims to domain, but they are accessible and unambiguous.

Some broad hypotheses to be explored in this article include the following:

1. Because the outputs of bureaucratic organizations, especially administrative agencies, are ill defined, intangible claims to domain may have definite consequences. It is especially important to examine the relationship of domains claimed by bureaus to their success in holding or expanding resources.
2. Whether a bureau's domain is held, claims to domain may have definite claim, technological advancement and newness:
   a. Bureaus claiming narrow domains and performing essential services remain stable over time.
   b. Bureaus making broad but inconsistent claims contract over time.
   c. Bureaus making broad claims, using advanced technologies and having the temporary advantage of newness expand their domains over time.[3]
3. Despite erosion of resources, bureaus rarely contract their claims to domain. The inflation of claims may lead to increased conflict among bureaus over time.

THE STUDY OF FINANCE AGENCIES

Data describing some 227 finance agencies of city, county and state governments are analyzed in this article. Each of the agencies is headed by the chief financial officer of the jurisdiction. The agencies are located in cities which had populations greater than 50,000 in 1960, counties of more than 100,000 and most of the fifty states. In 1966, some 254 agencies were surveyed and we were able to complete interviews in 240 of them in 1972. The department head and all of the division heads were interviewed in each agency. In addition, heads of budgeting and data-processing units located outside the focal finance agencies were contacted in each jurisdiction.[4] Almost all of the data reported in this article are from the interviews with department heads. Of the 240 agencies in the 1972 survey, 215 were the same organizations surveyed in 1966. Eleven cases were questionable and excluded from the analysis. Fourteen were clearly different because they had reorganized totally and changed their names since 1966. The fourteen are described in the section on Changing Domains. Two of the 215 departments which did not change had unusual names and are excluded from the tabulations in this article. Of the remaining 213, 71 were comptrollers', auditors' or accountants' offices, 135 were finance departments and seven were departments of administration in 1966.

It should be emphasized that the data reported in this article describe finance agencies in 1966 and 1972 only. Clearly, it is desirable to replicate the analysis over different intervals. A 1975 survey will allow examination of causal patterns using three- and nine-year lags in addition to the six-year lag we now have.

Domain and Organizational Structure

The surface characteristics of comptrollers' offices, finance departments and departments of administration should be compared at this point. Table 1 displays some of the major structural attributes of these bureaus in 1966. Finance departments, on the average, were somewhat smaller than comptrollers'

[3] Stinchcombe (1965) discusses liabilities of newness, but my research suggests that new bureaus, unlike business enterprises, grow more rapidly than others. Perhaps this is because the liabilities facing most new organizations—Stinchcombe lists the need to learn new roles, development of new organizational routines, reliance on social relations among strangers and the acquisition of a clientele—do not plague bureaus formed out of old ones or do not affect them as much as others. Clearly, some research is required in this area.

[4] Data for the 1966 and 1972 surveys were collected by the Field Department of the National Opinion Research Center. For both surveys, a 97 percent completion rate was achieved which is a tribute to NORC's persistence. The Data-Processing Department of NORC coded and cleaned the 1972 data and contributed materially to their quality.

Table 1. 1966 Structural Characteristics of Departments by Name

	Comptrollers' Offices[a]	Finance Departments	Administration Departments
Mean size	118.2	85.1	294.0
Mean proportion expected to have college degree	.230	.184	.256
Percent using computer	63%	53%	86%
Of those using computer, percent operating own computer	69%	74%	100%
Mean index of automation[b]	.527	.403	.869
Mean number of financial responsibilities	4.41	7.37	6.29
(N)	(71)	(135)	(7)

[a]Includes Auditors' and Accounting Offices.

[b]Of those accounting activities for which a department is responsible, the proportion done on a computer.

offices—the mean numbers of full-time employees are 85.1 and 118.2, respectively—and departments of administration were considerably larger. A relatively small proportion of employees of finance departments—less than one-fifth—were expected to have college degrees compared to comptrollers' offices and departments of administration. About half the finance departments, three-fifths of comptrollers' offices, and all but one of the seven administration departments used computers in 1966. About 70 percent of finance departments and comptrollers' offices which used computers in 1966 had their own data-processing equipment; all six of the automated administration departments did. An index of automation reflects the same pattern as the percentages of agencies using computers; finance departments were least likely and departments of administration were most likely to have computerized their major accounting activities. Finally, and not surprisingly, comptrollers' offices had considerably fewer responsibilities than either finance departments or departments of administration in 1966. Comptrollers' offices had a mean of 4.41 major fiscal functions; finance and administration departments had 7.37 and 6.29, respectively. These data describing 1966 structure describe quantitatively some of the differences in organizational patterns sketched above. More important, however, they allow examination of the impact of claims to domain. If, on the one hand, causal processes among comptrollers' offices, finance departments and departments of administration are similar when variables describing organizational structure are controlled, then names which reflect claims to domain are unimportant. If, on the other hand, differences remain or are accentuated after surface characteristics are taken into account, then claims to domain must be understood as central to the behavior of bureaus. My data show the latter to be the case. Indeed, it will be argued that some of the differences revealed in Table 1, especially differences in size and scope of responsibilities, are effects of claims to domain rather than the other way around.

ENVIRONMENTAL DEMAND AND DOMAIN

One way of testing the importance of a bureau's claim to domain is how to ask how it mediates external pressures influencing organizations. A narrow claim is easily protected because it identifies a bureau's outputs and services which would have to be reduced should resources decline. The obverse does not necessarily hold, however; a narrow claim can be enlarged. A broader claim to domain is

Table 2. Regressions of 1972 Size on 1966 Size and 1966 Demand Variables by Name

	Comptrollers' Offices		Finance Departments		Administration Departments	
Variables	Zero-Order	B*	Zero-Order	B*	Zero-Order	B*
1966 Size	.9879	.9908	.8142	.9085	.9717	.8174
1966 General Fund	.6816	(-.0042)[a]	.1766	-.2174	.7376	.2692
1966 Size	.9879	1.0038	.8142	.9464	.9717	.9231
1966 Total Funds Administered	.4943	(-.0305)	.0397	-.2392	.4381	(.1631)
1966 Size	.9879	.9816	.8142	.9291	.9717	.8526
1966 Government Employees	.8320	(.0074)	.2676	-.2192	.6628	.2403
1966 Size	.9879	.9935	.8142	.9864	.9717	.8482
1965 Population	.7244	(.0077)	.2742	-.2972	.6675	.2524

[a]Coefficients in parentheses are less than twice standard errors.

more volatile. The broader the claim, the greater the diversity of clientele, hence the greater the likelihood of dissatisfaction. Bureaus which are new and technologically sophisticated can use broad claims to augment their resources, but over time broad claims may become inconsistent and clients disenchanted so that resources are shifted elsewhere. All three patterns—stability, growth and contraction—are evident among the agencies administering local government finances we have studied.

One element of the environment to which administrative agencies may respond is demand for their services. For bureaus in charge of the finance function, the size of the budget, the size of the government and the size of the population served can create demand but need not do so. The limited domains claimed by comptrollers' offices may make them insensitive to such pressures because budgets and disbursements can increase without affecting the number of accounts maintained or checks written. The broader domains claimed by finance departments and departments of administration render them more vulnerable, however. The management of investments, purchasing and especially data-processing are sensitive to scale factors. One would expect finance departments to contract somewhat with environmental demand as their functions are shifted to other agencies, and administration departments to grow with demand as they accumulate responsibilities. My data are consistent with these expectations.

Table 2 shows regressions of 1972 size as measured by the number of full-time employees on 1966 size and measures of environmental demand for agencies administering local government finance. Twelve separate regressions are displayed in the table. Each panel includes one of the four measures of demand—the four are the size of the general fund which covers common services such as police, fire protection, health and highways; the total funds administered by a department; the number of the government employees; and the population served. The effects of environmental demand on 1972 size are estimated by standard regression coefficients (B*) net of 1966 size.[5] Briefly, for comptrollers' offices, there are no effects of environmental demand on 1972 size when the earlier measure of size is controlled. None of the four coefficients is significant. For finance departments, the impact of environmental

[5]This is a variant of the cross-lagged path model in which a dependent variable at a later time is regressed on lagged dependent and independent variables. See Heise (1970) for a full description of the model.

Table 3. Finance Departments Only: Regressions of 1972 Size on 1966 Size and 1966 Demand Variables Controlling for Use of Computer in 1966

	Used Computer		Did Not Use Computer	
Variables	Zero-Order	B*	Zero-Order	B*
1966 Size	.8212	.8236	.8340	1.1227
1966 General Fund	.2124	(.0089)	.6162	-.3393
1966 Size	.8212	.8135	.8340	1.4418
1966 Total Funds Administered	.4299	(.0151)	.6628	-.6616
1966 Size	.8212	.8101	.8340	1.1969
1966 Government Employees	.4170	(.0272)	.6587	-.4074
1966 Size	.8212	.9273	.8340	1.4808
1965 Population	.4987	(-.1513)	.6719	-.6988

demand is consistently negative,[6] and for departments of administration three out of four coefficients of the demand variables are significantly positive despite the small number of cases. (The number of cases does suggest caution in interpreting this finding, however.)

It is important to note that the same data can be presented descriptively rather than as causal statements. Comptrollers' offices in 1966 had a mean of 2.3 percent of local government employees and 2.1 percent of employees in 1972, an insignificant change. Finance departments dropped from 6.4 to 4.0 percent of employees, while departments of administration grew from 3.3 to 4.4 percent. The causal statements about effects of environmental demand are preferred to the descriptive ones because, as will be shown later, demand is the exogenous variable driving a set of relationships among environmental and organizational variables.

The domains claimed by bureaus administering local government finances, then, influence patterns of growth. Environmental demand appears not to affect comptrollers' offices and similar agencies claiming narrow domains, contracts finance departments and spurs growth of departments of administration. An important question, however, is whether the apparent effects of domain are due to identifiable structural attributes of these bureaus.

An Effect of Automation

To check the possibility that the effects of environmental demand are spurious, each of the variables listed in Table 1 was controlled in additional regressions.[7] Neither size, the proportion of college-trained employees nor operation of data-processing facilities made any difference. The scope of activities of a department yielded an anomolous result: finance departments least like comptrollers' offices, those charged with most responsibilities, were least likely to be influenced by environmental demand. But even this effect disappeared when departments were grouped by whether or not they used computers in 1966. Computerization had no impact on comptrollers' offices, but finance departments which were automated in 1966 did not contract with environmental demand while those not computerized were strontly affected. The regressions are shown in Table 3. As before, each of the four panels includes 1966 size and one of the measures of environmental demand; the left columns display regressions for departments which were computerized in 1966, and non-automated departments appear at the right.

[6]The effects of leadership reported in an earlier article (Meyer, 1975) hold for finance departments but not comptrollers' offices. Detailed tabulations will be sent to the reader upon request.

[7]Needless to say, the small number of departments of administration makes it impossible to group cases to control for additional variables.

Among finance departments which did their accounting work on computers, no significant effects of the demand variables appear. Finance agencies which were not computerized are affected by demand. The coefficients of all four demand variables are significantly negative. Again, these data can be presented descriptively. Using a computer had no effect on the relative size of finance departments in 1966—6.4 percent of government employees—but those which were computerized in 1966 dropped only to 4.9 percent of employees in 1972 while those not computerized dropped to 3.1 percent.

Several explanations for this pattern can be imagined. One is that technological advance of any kind has the element of novelty which helps a bureau expand or at least maintain its resources. No doubt there is some truth to this, but the explanation cannot be tested easily. And there is no evidence that computerization has augmented the size of comptrollers' offices. Another possibility is that resources lost from finance functions are balanced by growth in data-processing whether or not a department actually operates a computer. Some of the data support this interpretation. Automated finance departments are losing fiscal functions as rapidly as others; I shall deal with this presently. Finally, automation may redefine a department's tasks somewhat so that its claim becomes less precarious. The availability of a computer may allow a shift of emphasis away from disparate, even antagonistic responsibilities (for example, budgeting and purchasing) and toward machine processing of fiscal data regardless of their purpose. Centralization of control of finance functions may give way to centralization of fiscal data together with decentralization or even loss of specific functions, minimizing the contradictions in the finance department model of organization.

The data show some evidence of a trend toward decentralization of decisions in automated finance departments which is not present in either non-automated finance agencies or comptrollers' offices. Table 4 displays percentages of departments where decisions of various kinds are delegated to division heads or below. Several patterns are evident in the table even though most of the percentage differences are not statistically significant. First, regardless of name, agencies which were automated in 1966 tend to be less centralized than those which did not use computers. This is partly, but not entirely, a function of size and is not of major concern here. Second, there is little overall change in decision-making practices among comptrollers' offices. If anything, the trend is toward centralization of authority. Among automated comptrollers' offices, 54 percent delegated budgeting and accounting decisions in 1966 but 42 percent did in 1972; 47 percent of department heads had a policy of delegating other than routine decisions in 1966 but only 40 percent did this in 1972. Third, there is a trend toward decentralization of automated finance departments. Thirty-eight percent allowed division chiefs to make official promotion and dismissal recommendations in 1966; the 1972 proportion was 49 percent. Only 36 percent delegated budgeting and accounting decisions at the time of the earlier study, but 56 percent did in 1972. Finally, 41 percent of finance department heads had a policy of decentralization in 1966; 58 percent did in 1972. The trend toward delegation of authority in automated finance departments makes them now less centralized than automated comptrollers' offices even though they were more centralized in 1966. The interaction of domain and automation may have shifted subtly but importantly the actual functions of finance departments.

In sum, the domains claimed by bureaus administering finance mediate the effects of environmental demand on size. Comptrollers' offices which claim the narrowest domains are least affected; finance departments whose domains are broad but uncertain contract with demand if they were not automated or remain stable but decentralized if they were; and departments of administration claiming broad domains and having the at least temporary advantage of newness increase their resources with demand.

COMPETITION IN THE IMMEDIATE ENVIRONMENT

Another way to determine if the domains claimed by public organizations affect their behavior is to examine the impact of the competition in their environments. Competition is the obverse of a bureau's scope of responsibilities: the former is the *number of other agencies in the same unit of government*

Table 4. Delegation of Decision-Making Authority by Name, 1966 Use of Computer and Year

	Comptrollers' Offices		Finance Departments	
	Used Computer	Did Not Use Computer	Used Computer	Did Not Use Computer
Percent of departments where head delegates authority to recommend promotions				
1966	55%	23%	38%	29%
1972	52%	31%	49%	31%
(N)	(44)	(26)	(72)	(62)
Percent of departments where head delegates authority to set budgets or accounting procedures				
1966	54%	39%	36%	44%
1972	42%	39%	56%	49%
(N)	(43)	(26)	(72)	(63)
Percent of departments where head's policy is to delegate decisions				
1966	47%	39%	41%	27%
1972	40%	31%	58%	39%
(N)	(43)	(26)	(71)	(62)

which either share or have fully some of the responsibilities one might expect to find in a finance department; the latter is the number of major finance activities for which a department has full responsibility. My hypothesis is that just as environmental demand shifts resources away from bureaus with precarious claim to domain, competition leads to a loss of functions. Contrariwise, bureaus which are new, have expansive claims to domain and grow with demand for their services should be able to reduce the competition in their environments as their functions enlarge. In other words, the direction of causality between the number of competitors and the scope of responsibilities depends on the nature of the domain claimed. The weaker the claim, the greater the likelihood that the environment contracts organizations; the more expansive and compelling the claim, the more easily organizations reduce competition from other agencies.

The data generally support these hypotheses, but with some qualifications. Table 5 displays regressions indicating the direction of causality between the number of competitors in the environment and the scope of responsibilites of the agencies studied. The number of responsibilities a department had in 1972 is regressed on 1966 responsibilities and 1966 competitors in the upper panel of the table; 1972 competitors is regressed on 1966 competitors and 1966 responsibilities in the lower panel. From the regressions in the upper panel, it can be seen that the number of competing agencies in the immediate environment reduces responsibilities for all types of finance agencies. The coefficients of 1966 competitors are significantly negative for comptrollers' offices, finance departments and departments of administration. However, for comptrollers' offices and finance departments, scope of responsibilities has no impact on the number of competitors. But this is not the

Table 5. Relationship of Number of Competitors to Scope of Responsibilities by Name

	Comptrollers' Offices		Finance Departments		Administration Departments	
	Zero-Order	B*	Zero-Order	B*	Zero-Order	B*
Regression of 1972 Responsibilities on 1966 Responsibilities and 1966 Competitors						
Variables						
1966 Responsibilities	.5470	.3229	.4179	.4213	.3489	(-.2436)
1966 Competitors	-.5463	-.3205	-.2057	-.1767	-.8185	-.9677
Regression of 1972 Competitors on 1966 Competitors and 1966 Responsibilities						
Variables						
1966 Competitors	.6562	.6526	.4844	.4505	.5269	(-.5230)
1966 Responsibilities	-.2792	(-.0089)	-.3415	(-.0524)	-.8545	-1.2826

case for the seven departments of administration. As the righthand columns in the lower panel of Table 5 show, the number of responsibilities departments of administration had in 1966 is negatively related to competitors later on. The more functions they had, the fewer competitors they now have. Departments of administration, unlike the others, are managing their environments to some extent. (Again, given the small number of cases, the results for administration departments should be interpreted cautiously.)

That competition contracts the responsibilities of finance departments is not surprising. It is consistent with the overall pattern of decline of these agencies. The same effect was not anticipated for comptrollers' offices or administration departments. Indeed, the negative effect of competitors on scope of responsibilities persisted when other variables such as size and use of computers were controlled. And, save for the seven departments of administration, no variable other than the number of competitors directly affected an agency's scope of responsibilities. These results suggest that the antecedents of competition in the environment should be traced in order to understand how these bureaus gain or lose functions.

Sources of Competion

If competitors in the immediate environment reduce an agency's scope of responsibilities regardless of other conditions, then some of the conditions creating competition should be identified. In particular, possible effects of environmental demand as well as of organizational size should be examined. My expectation is that for finance departments, environmental demand increases competition which in turn limits scope of responsibilities. Size may have the same effect; the domains of finance departments may be so precarious that past a point growth ceases and functions are dispersed to other agencies. For comptrollers' offices, a different pattern is anticipated. It may be that their environments were essentially stable between 1966 and 1972 so that neither size nor demand increased the number of competing agencies. My data indicate that this was the case.

Table 6 shows regressions of 1972 competitors on 1966 competitors, responsibilities, size and the four measures of environmental demand. For comptrollers' offices, no significant predictors of 1972 competitors other than the level of competition in 1966 appear; their environments are stable. The regressions for finance departments are quite different. The amount of competition in 1972 is a positive function of 1966 competitors, 1966 size and the demand variables. Only 1966 responsibilities have no effect. Environmental demand and size contribute separately to the number of competitors, and their independent effects are significant. For finance departments, then, demand and size lead to proliferation of other agencies with fiscal

Table 6. Regressions of 1972 Number of Competitors on 1966 Competitors, 1966 Responsibilities, 1966 Size and 1966 Demand Variables by Name

Variables	Comptrollers' Offices Zero-Order	Comptrollers' Offices B*	Finance Departments Zero-Order	Finance Departments B*
1966 Competitors	.6561	.7513	.4844	.4211
1966 Responsibilities	-.2692	(-.0022)	-.3145	(.0504)
1966 Size	.0085	(.2298)	.4156	.2576
1966 General Fund	.1492	(-.2478)	.3669	.1736
1966 Competitors	.6561	.6392	.4844	.4311
1966 Responsibilities	-.2692	(.0965)	-.3145	(.0748)
1966 Size	.0085	(.0125)	.4156	.2008
1966 Total Funds Administered	.2666	(.0732)	.4301	.2413
1966 Competitors	.6561	.6719	.4844	.4135
1966 Responsibilities	.2692	(-.0174)	-.3145	(.0492)
1966 Size	.0085	(.1505)	.4156	.2345
1966 Government Employees	.0749	(-.1188)	.4102	.1905
1966 Competitors	.6561	.6591	.4844	.4247
1966 Responsibilities	-.2692	(-.0006)	-.3145	(.0694)
1966 Size	.0085	(.0477)	.4156	.1971
1965 Population	.1143	(.0049)	.4383	.2377

responsibilities; as the number of competing agencies increases, the range of responsibilities of finance departments declines. Again, the broad but inconsistent domains of finance departments are buffeted by the environment.

Because of the small number of cases, regressions for departments of administration were limited to a single independent variable and the lagged dependent variable. This limitation means that estimates of regression coefficients may be quite biased and should be taken as only suggestive. With this understanding, it appears that size has no impact on the number of competitors for administration departments—the regression is shown in the upper panel of Table 7—and the effects of environmental demand are inconsistent. One interesting effect of size emerged. For departments of administration, size increases scope of responsibilities as is shown in the lower panel of Table 7. This was not the case for comptrollers' offices and finance departments. The link completes the causal path between demand and competitors for departments of administration. Demand increases size, size increases responsibilities; and

Table 7. Administration Departments Only: Effects of Size on Number of Competitors and Scope of Responsibilities

Regression of 1972 Competitors on 1966 Competitors and 1966 Size

Variables	Zero-Order	B*
1966 Competitors	.5269	.5268
1966 Size	.0153	(.0067)

Regression of 1972 Responsibilities on 1966 Responsibilities and 1966 Size

Variables	Zero-Order	B*
1966 Responsibilities	.3489	.5854
1966 Size	.1964	.4831

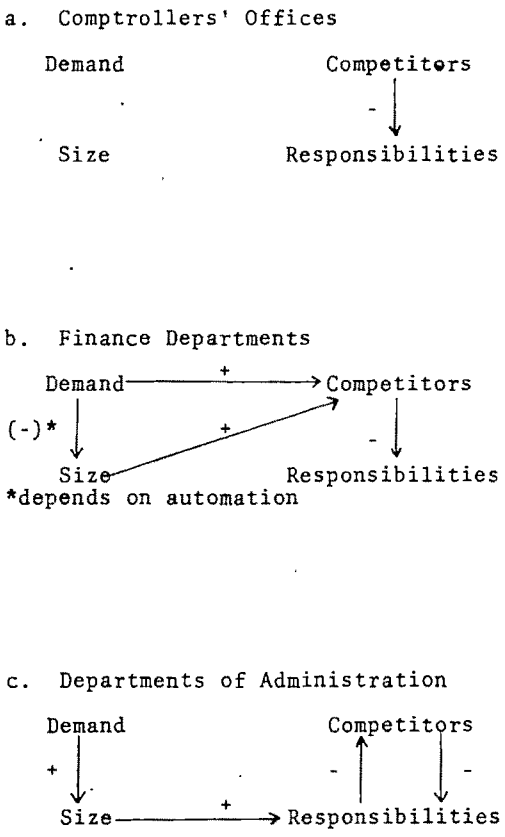

Figure 1. Causal Relationships among Organizational Variables

as responsibilities increase, competitors decrease. For finance departments, demand had a direct and positive effect on competitors; for comptrollers' offices the link was nonexistent.

Summary

At this point, the findings presented so far should be reviewed. Taken together, they indicate that the causal structures of variables describing comptrollers' offices, finance departments and departments of administration are quite different. Figure 1a shows the causal structure for comptrollers' offices; Figures 1b and 1c describe finance departments and departments of administration, respectively. Among comptrollers' offices, practically no causal links can be found. As Figure 1a indicates, competition tends to reduce the scope of functions of comptrollers' offices, but there are no other connections among the four variables. Finance departments are replete with causal paths. Environmental demand decreases size; both size and demand increase competition; and competition contracts the scope of responsibilities of finance departments. The effect of demand on size holds only for finance departments which did not use computers in 1966. (This is indicated in Figure 1b, but not shown is the trend toward decentralization among automated finance departments.) Departments of administration are also replete with causal paths, but they are quite different from finance departments. Environmental demand increases size; size increases the scope of responsibilities; increased responsibilities reduce competition. (At the same time, as Figure 1c shows, competition in environment reduces responsibilities.) In a sentence, comptrollers' offices are essentially stable over time, finance departments are being contracted by their environments while departments of administration are growing, adding functions and, to some extent, dominating their environments.

CHANGING DOMAINS

Here I wish to test the proposition that when bureaus reorganize and change their names, they usually expand their claims to domain. The data are straightforward and nearly overwhelming. Some fourteen departments interviewed in 1966 did not exist in 1972. They were replaced by or subsumed under new agencies with new names whose heads became the chief financial officers for their jurisdictions. Table 8 shows the cross-tabulation of 1972 name by 1966 name for the fourteen departments. Eleven of the fourteen were comptrollers' offices and the like in 1966; of the eleven, four became departments of administration, six changed to finance departments and one which had combined the county clerk's and auditor's functions became a separate accounting office. Two finance departments changed names. One became a department of administration while the other was split into several offices leaving the comptroller as the chief fiscal officer. One administration department became a department of finance, but the new name in this case was inappropriate because the new finance department incorporated all of the functions of the former department of

Table 8. Name Changers Only: 1972 Department Name by 1966 Name

	1966 Name		
	Comptroller's Office	Finance Department	Administration Department
1972 Name			
Comptroller's Office	1[a]	1	0
Finance Department	6	0	1
Administration Department	4	1	0

[a]Clerk-Auditor to Accounting Office.

administration and added to them. In short, in only two instances out of fourteen did bureaus split and domains claimed by the agency with primary financial responsibility contract. In one case, a name changed was patently misleading, and in eleven others claims to domain expanded.

That a number of comptrollers' offices changed to finance and administration is not surprising, though it is striking that there were about as many new administration as finance departments. More important is the near absence of change among finance departments. One might have expected declining resources and responsibilities to have forced more of them to revert to the comptroller's model of organization. No doubt, this occurred in fact if not in name for a number of departments of finance, especially those not computerized in 1966. But the name "finance" and the claims accompanying it persisted for most. Evidently, there is reluctance to contract claims to domain despite loss of responsibilities. Perhaps the title of finance director is needed to justify the department head's salary; perhaps jobs can be preserved if obsolete claims can be maintained. Whichever is the case, fundamental change among the agencies studied here is a one-way process. Claims to domain expand, but they rarely contract. If this is true for most bureaus, then one would predict increasingly overlapping jurisdictions, hence greater conflict and proliferation of coordinating bodies over time.

Antecedents of Changes

A brief comment on the antecedents of change among the fourteen name changers is required. Their surface characteristics in 1966 were unremarkable. Indeed, the eleven comptrollers' offices replaced by other agencies between 1966 and 1972 were of the same size and had on average the same structural features as the seventy-one comptrollers' offices which did not change. The lack of consistency with environmental demand for their services among the fourteen name changers in 1966 does draw attention, however. As the rightmost columns in Table 9 show, the correlations between the four measures of environmental demand and size were little different from zero in 1966. After reorganization and change of name, the correspondence between demand and size increased markedly. The correlations of the demand variables with size range from .66 to .84 in 1972. These data suggest that, among other things, a source of fundamental change in organization is inconsistency with environmental demand.

Whether inconsistency with the environment is a sufficient condition for change is another matter. Departments of finance were less likely than others to undergo reorganization; yet, as is evident in Table 9, their consistency with environmental demand dropped substantially between 1966 and 1972. Comptrollers' offices remained highly consistent with demand during this interval, and the correspondence between demand and size actually increased for departments of administration. My best guess is that change will take place eventually among finance departments, but the process will be delayed because of resistance to contracting claims to domain.

CONCLUSION: A CYCLE OF CHANGE IN PUBLIC BUREAUCRACIES?

Some effects of organizational claims to

Table 9. Correlations of Size with Demand Variables by Name and Year

	Comptrollers' Offices		Finance Departments		Administration Departments		Name Changers	
	1966	1972	1966	1972	1966	1972	1966	1972
General Fund	.6922	.5723	.4337	.2528	.5730	.6981	.0477	.6599
Total Funds Administered	.5228	.6041	.5526	.2423	.2978	.6707	.1433	.5989
Government Employees	.8400	.8256	.5239	.3133	.4956	.6198	-.0074	.8423
Population	.7368	.8498	.5793	.2547	.4894	.6627	-.0370	.7713

domain embodied in names were explored in this article. The conventional view of domain—technology, population served and services rendered—was not applied to public bureaucracies because the population served is fixed and the services rendered are intangible for administrative agencies. In its place, the notion of claim to domain was developed, and it was argued that the kind of claims public agencies make strongly affect their behavior. Differing claims to domain were illustrated by the recent experience of agencies administering finances of city, county and state governments. Comptrollers', auditors' and accountants' offices make the narrowest claims to domain; the claims made by departments of finance are broad but inconsistent; and the claims of departments of administration are broad and expanding at the present time. It was hypothesized that bureaucracies claiming narrow domains but performing essential services remain stable over time; bureaus claiming broad but inconsistent domains decline over time; and bureaus claiming broad domains which are consistent, have the advantage of newness and utilize advanced technologies, tend to grow and expand their responsibilities.

The data show these hypotheses essentially correct. Causal links were sparse among comptrollers' offices. Environmental demand reduced size and competition reduced responsibilities among finance departments. For departments of administration, demand increased size which increased responsibilities which in turn reduced competition.

It was also hypothesized that the domains claimed by bureaus may expand but tend not to contract over time. Data describing fourteen agencies which were reorganized and changed their names between 1966 and 1972 support this notion. They also suggest that inconsistency with environmental demand is one source of fundamental change.

The findings underscore the importance of unprovable claims made by administrative bureaus. Unlike firms operating in competitive markets whose clientele is not limited by boundaries, bureaus can justify expansion only by increasing their functions. And as the neo-classical critics note, the outputs of bureaus are often ill-defined and not subject to quantitative measurement. Under these circumstances, what a bureau claims to be doing may be more important than anything else in augmenting its size and responsibilities. The broader the claim, the greater the growth, but claims rendered inconsistent by either technological change or changing beliefs about administrative organization render a bureau vulnerable to the environment as new agencies take over its functions. The findings also underscore the need for research comparing bureaus and firms (or segments of firms) performing similar functions. If the results reported here are not unique to agencies administering local government finances, then the distinction between bureaus and firms may be important. One must ask not only whether differences exist between bureaus and firms, but also whether the differences can be explained in terms of generic organizational variables. My own inclination is that the distinction drawn by the neo-classical economists and political scientists is probably right, but for the wrong reason. Rather than the absence of a market as such, the structure of whole local governments which links

bureaus with one another and removes them, organizationally if not physically, from their constituents may account for the patterns observed here. Finance agencies not embedded in larger bureaucratic structures (for example, accounting firms) would perhaps respond differently to their environments.

An overriding question is whether there is a predictable cycle of change among public bureaucracies, and if so, whether the pattern contributes to the inefficiencies for which bureaus are often criticized. Among agencies administering the finances of city, county and state governments, there appears to be a pattern. Claims to domain expand and increases in size and scope of responsibilities follow. After some time broad claims are rendered either inconsistent or obsolete by technological change and new management concepts, and either stability or decline sets in. Some agencies expand their claims further so as to encompass new technologies and beliefs, and their growth resumes. Claims to domain are rarely revised downward, however, even when key functions are lost. It could be that the constant inflation of claims gives rise to jurisdictional disputes and inter-agency wrangling which consume much time and manpower. If so, this is an important dysfunction of bureaucratic organization. Another question is whether the findings will hold for the finance agencies studied over a lengthy interval. The logic of the argument implies that the erosion of finance departments and growth of departments of administration will continue for a while, but neither can continue indefinitely. The results of the 1975 survey of finance agencies now under way will help answer this question.

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ORGANIZATIONAL STRUCTURE AND VOLUNTARY PARTICIPATION IN COLLECTIVE-GOOD DECISIONS*

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Voluntary participation in collective-good decisions is formulated as an exercise of individual choice within a context determined by the authority structure and the agreement structure of an organization. A choice model developed by Camilleri and Berger (1967) is used to explicate the nature of a collective-good decision. Organizational variables are interpreted as determinants of the parameters of the individual's choice to participate or not. Three sets of experiments comprising 15 conditions were conducted to investigate the substantive implications of the formulation. Each condition involved multiple replicates of organizations each with eight simulated members and a subject. Participation rates calculated from the model, using estimates of utility ratios derived from the data, closely matched the rank-order of observed rates within sets of experiments and less closely matched the rank-order of the differences between those observed rates. The results were supportive of the overall argument. Implications with respect to considerations of size, efficacy and structural complexity are discussed.

Within every organization providing collective goods there are structural conditions that differentially affect the way in which its members will be able and will want to become actively involved in the decision making of that organization. The defining characteristic of a collective-good decision is that its outcome accrues to everyone in the organization by virtue of the simple fact of membership. For any member to receive the rewards associated with the outcome of such a decision, it is necessary only that a decision is made, *not* that he participate in making it. (In a labor union, for example, a negotiated wage is paid to all members in good standing; whether or not they participated in the decision process which resulted in that wage.[1]) As Mancur Olson (1965) points out most effectively, motivation for participation in such decisions is inherently problematic.

This paper presents a partially formalized argument to account for the effects of some organizational factors on voluntary participation in collective-good decisions and a series of laboratory experiments aimed at evaluating the empirical adequacy of the formulation we propose. Our formulation has two parts. The first is a specification of a general choice situation for an actor presented with a collective-good decision. Like Olson, we explicate the process of voluntary participation as a problem in choice behavior; our formulation deals with the incentives and risks

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[1] Our use of the term, collective-good decision, is an extremely broad one; we mean to include such decisions as elections and the passage of a law. That is, we are talking about all decisions resulting in the selection of a collective outcome. Collective outcomes are binding upon, or available to, *all* members irrespective of their preferences among the original alternatives and without respect to their participation in the decision-making process.

an individual evaluates to arrive at a decision to participate or not participate.

The second part of our formulation is a specification of the effects of different organizational variables upon individual choice. Our reasoning here rests heavily on our understanding as to what the empirical findings in *Union Democracy* (Lipset et al., 1956) imply for Michels' argument in *Political Parties* (1915) about the inevitability of centralized authority systems in organizations. Michels argued that the reward structure of a collective-good organization makes active participation by non-officers unlikely. His reasoning is not fundamentally different from Olson's: such organizations reward officers directly for their participation but provide no rewards over and above collective goods for rank-and-file exercise of decision-making rights. This differential reward system inevitably leads to low rank-and-file participation, which is the main dynamic behind the "Iron Law of Oligarchy." Lipset, Trow and Coleman argue that if a persistent pattern of disagreement about the value of alternative collective goods is created at a point before the rank-and-file have lost (irretrievably delegated) their decision-making rights, direct motivation for the active exercise of those rights exists. That is, it is to the members' advantage to do what they can to insure that the collective-good alternative most valuable to them gets chosen. Rank-and-file participation is motivated, then, by a desire to improve the chances that a preferred outcome will be selected; patterned dissensus among the rank-and-file ensures members that their preferred outcome is at risk.

Taken together, the arguments in these two books suggest that rank-and-file inactivity in an organization with a decentralized authority system is a sufficient condition for producing centralization, that high rates of rank-and-file participation are sufficient to abort the centralization dynamic, and that some pattern of dissensus among the membership is sufficient to maintain high rates of rank-and-file participation in decentralized organizations. More abstractly and generally, the combined argument is that structural features of the organization itself determine participation rates and that participation rates in turn can modify or maintain organizational structures.

We are concerned primarily with the first half of that argument—with the effect of organizational structure upon participation. The term, "organizational structure," refers throughout to any property of an organization, including the *distribution* of a property of its members, which is stable over a non-trivial set of activities (including, of course, decision-making). We will deal specifically with two structures which are suggested by Michels and Lipset et al., the authority structure and the agreement structure. The authority structure of an organization is defined by the set of rules which distributes the formal decision rights over the entire membership. The agreement structure refers to any regular pattern among the members' preference-rankings of the collective-good alternatives. We investigate the effects of several states of each of these two variables. (We examine also the effects upon participation of the leader's influenceability [or lack of it] by participation, but we do not consider this to be a structural property.)

We mean, of course, for our analysis to be useful in the investigation of organizational structure in general, not just to an examination of the specifics in *Union Democracy* and *Political Parties*. We are concerned with the effects upon voluntary participation of dissensus within centralized organizations as well as within decentralized ones. Much of our reasoning with regard to the effects of authority structure and various patterns of dissensus is influenced by Gamson's discussions in *Power and Discontent* (1968), in particular by his explications of "efficacy" and of "trust" in terms of expected probability of obtaining satisfying decisional outcomes. Our conclusions, however, are not always compatible with his. We will not consider in any formal way the effects of participation rates upon structural stability, although we believe our specification of the effects of structure upon participation will prove informative in an investigation of that issue.

Our formalization of voluntary participation as individual decision-making is based upon a model developed by Camilleri and Berger (1967; Camilleri et al., 1972); the organizational variables are used to specify the parameters of that model. In an earlier study (McMahon and Camilleri, 1975), we subjected our application of their model to a simple experimental investigation using simulated

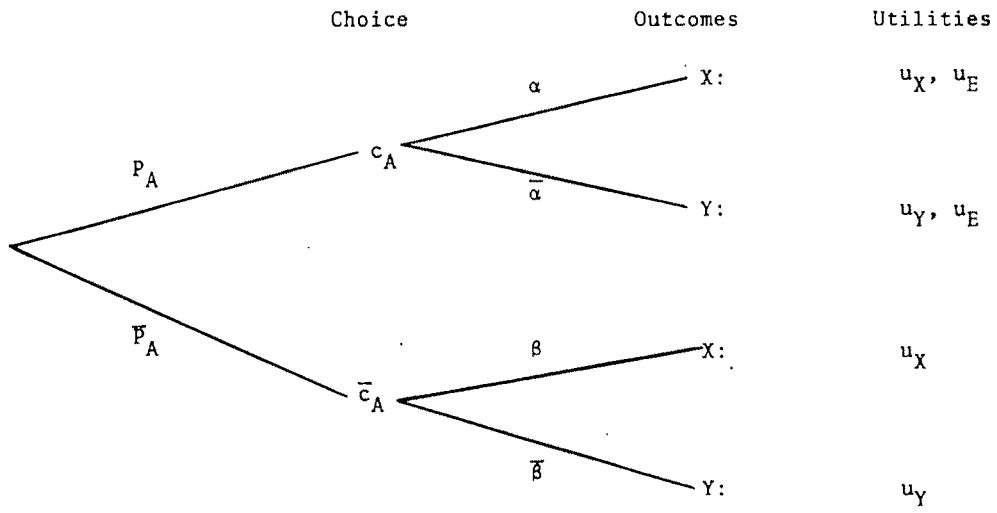

Figure 1: General Choice Utility Structure for Actor in a Collective-good Decision-making Opportunity. α and β are subjective probabilities, c_A indicates Actor comes to the meeting, $\alpha \geq \beta$ and $u_E < 0$.

three-person groups. That study was intended primarily to help us determine whether further research with more complex experimental procedures was warranted and feasible. This previous experience persuaded us that in further investigation we needed to construct experimental situations with greater real life properties (especially those associated with size) and with variations which permitted us to focus upon those implications especially characteristic of the way we formalize the problem.

In the next section of this paper, we will explicate the technical details of the argument which forms the basis of the experiments. This is followed by a rather lengthy description of the general properties of the new experimental procedures and of the reasoning which led to them. Finally, we report the details of the experiments conducted to date and discuss their results.

THE MODEL

In order to explain our conception of how organizational variables affect the individual's choice to participate, we first describe a collective-good decision-making opportunity as a choice-utility structure for an organization member. Imagine a hypothetical organizational decision-making situation which has two collective-good alternatives, X and Y. Alternative X has positive utility for a given actor (A); Y has no utility for him (i.e., the individual expects to gain nothing and to lose nothing if Y is selected as the organizational outcome.[2]) If A is given an opportunity to participate in the selection of one of these as the group outcome, his decision to accept or decline that opportunity will be a function of:

(a) the utility to him of the two alternatives (u_X and u_Y);
(b) whatever he must expend in order to participate, (u_E);
(c) his subjective assessment of the likelihood that each alternative will be selected with and without his participation.

If we let α designate his expectation that X (his preference) will be chosen under the condition that he participates and β symbolize his expectation that X will be selected even if he does not, then his choice utility structure can be represented as follows in Figure 1.

What the diagram indicates is that if A does participate, he certainly forfeits whatever he has to expend in order to do so (u_E), and with probability α he expects to gain the rewards he associates with alternative X. He is aware that even if he does participate, with some

[2] Of course, u_Y can take any value less than u_X; it is set equal to zero here to simplify presentation.

probability ($\bar{\alpha}$) Y will be chosen as the group outcome. In the latter case, he expends u_E, but gains nothing since $u_Y = 0$. If A decides not to participate, he expends nothing and may yet with probability β gain the rewards he associates with X. We assume here α is never less than β. (We avoid the situation in which A expects his own participation to reduce the likelihood of his preference being selected.)

In order to predict the probability that an individual will choose to participate in a collective-good, decision-making opportunity, it is necessary to:

(a) specify how to combine the elements in the tree to arrive at a prediction;
(b) specify the probabilities and utilities for particular structural contexts of interest to us.

The Choice Function

The Camilleri and Berger function asserts that the ratio of the choice probabilities of two, mutually exclusive alternatives (e.g., participation and non-participation) is equal to the ratio of their "expected gains."[3] The expected gain (G) of a particular alternative is defined as the sum of the positive utilities associated with that alternative, each weighted by its subjective probability, *minus* the sum of the negative utilities associated with the rejected alternative, each weighted by its subjective probability. The approach is p-centric; that is, we are dealing with the actor's evaluation of events and his subjective expectations about their likelihood of happening. Because the sum of the choice probabilities of two exhaustive, mutually exclusive alternatives must be unity, the probability of one alternative's being selected is simply the ratio of its expected gain to the sum of the expected gains for each. In this context the function is:

$$P_A = \frac{G_c}{G_c + G_{\bar{c}}} \qquad (1)$$

where G_c and $G_{\bar{c}}$ are the expected gains the focal decision-maker (A) associates with participating and not participating, and P_A is the probability that A participates. That is, the probability that A will participate equals the ratio of the expected gain in participating to the combined gain in both courses of action.

Applying this function to the utility structure sketched in Figure 1, the expected gain in participating is simply αu_X.[4] The expected gain in not participating is $\beta u_X + u_E$ (i.e., the only positive utility associated with not participating is βu_X and in choosing that course of action, the individual avoids the "expense" (u_E) participation would have entailed for him). So:

$$G_c = \alpha u_X, \text{ and } G_{\bar{c}} = \beta u_X + u_E.$$

The probability that A decides to participate is, then:

$$P_A = \frac{(\alpha u_X)}{(\alpha u_X) + (\beta u_X + u_E)} \qquad (2a)$$

and the probability that A decides not to participate is:

$$\bar{P}_A = \frac{(\beta u_X + u_E)}{(\alpha u_X) + (\beta u_X + u_E)} \qquad (2b)$$

In similar manner we can write a prediction equation for any particular tree of interest.

This formulation differs from conventional ones (e.g., Coleman, 1964; Riker and Ordeshook, 1973; Luce, 1959) in several ways: (1) it defines utilities on a ratio scale; (2) it

[3] The reader is referred to Camilleri and Berger (1967) for the *substantive* reasoning which originally led to this function. We will discuss only those problematic properties of the function which are not discussed there and which are important to our purposes here.

[4] In this example, $u_Y = 0$, and u_E is a negative element to be treated as a gain associated with not coming. Strictly speaking, we should define u_E as a number less than zero and enter it as a subtracted element associated with not coming as follows: $G_{\bar{c}} = \beta u_X - u_E$. For convenience we shall instead define u_E as a positive number and *add* it to the elements associated with not coming. The results are algebraically equivalent. Up to now the context made the meaning of u_E clear. We will follow this practice with regard to u_E throughout the paper.

defines negative as well as positive utilities; (3) it defines a function on the utilities which transforms them into a pure number between zero and one. This number is interpreted as the probability that an identified alternative will be chosen from the set of alternatives presented. The quantities G_j (expected gain associated with the jth alternative) that we define on the alternatives are combined by a functional form similar to Luce's Theorem (1959): $PS(x) = \frac{v(x)}{\Sigma v(x)}$. This theorem requires $v(x)$ to be a positive, real-valued function, unique only up to multiplication by a constant. The quantity G_j defined by Camilleri et al. is equivalent to the expected value when all of the utilities are positive; but if there are any negative utilities involved, it is not. G_j is defined by subtracting negative utilities associated with a particular alternative from the positive utilities associated with any alternative which, if selected, avoids the negative utilities. This procedure ensures that G_j is always a positive, real-valued number. However, if any alternative entails a negative utility, G_j is relative to the subset of alternatives presented. That is, the formulation in effect violates the "independence from irrelevant alternatives" criterion. Many of the unconventional properties of this formulation can be tested with appropriately designed experiments, however the research we report here is not particularly suitable for this purpose. Our interest here is strictly in the way in which organizational variables affect participation by virtue of their effects upon parameters of the choice structures, i.e., the probabilities of organizational outcomes.

Organizational Variables and α, β

The parameters α and β represent a member's subjective expectations about the effects of participation (his own and others') upon the determination of organizational outcomes. α represents A's expectation that the organization will select his preference when he participates, and β represents his expectation that it will select his preference when he does *not* participate. These entities must be distinguished from the objective realities of the situation. We denote the objective counterparts of α and β as a and b, respectively. In the experiments we report below, a and b (i.e., the objective probabilities) are determined by the structure of the organizations we create and by the particular pattern of participation we specify for the other members of the organization. It is our working assumption that the conditions of the experiments dominate the actor's field so that α and β are closely determined by a and b. Accordingly, we use a and b in our computations as estimates of α and β.

Any difference between α and β can be interpreted as the degree of *efficacy* a member attributes to his own participation. We wish to distinguish between efficacy and a belief on the part of a member that participation has some causal relevance to the selection of organizational outcomes. In the formal literature on voluntary participation, causal relevance is expressed statistically as some function of the probability that A's vote will affect the results–i.e., as efficacy. (For a review of some of this literature, we recommend Good and Mayer, 1975.) It seems to us that a statistical representation of causal relevance does not fully attend to the essential features of voluntary participation in collective-good decisions, particularly under the special circumstance that a = b. Consequently, we represent efficacy as any difference between α and β (as estimated by a and b). A belief in causal relevance has no statistical representation in our calculations, but it is treated as a necessary scope condition for voluntary participation. That is, if a member believes that participation is causally relevant to organizational outcomes, then there is some probability that he will participate, but we do not represent a statistical effect due to causal relevance. If a member does not hold a belief in causal relevance, we would not consider his behavior an instance of voluntary participation governed by the argument we present here.[5]

We believe that causal relevance will be present whenever $\alpha > \beta$; that is, whenver a

[5] Olson's position is that since participation entails some cost and since the collective goods are available even without participation (as a "free rider"), there is no incentive to participate when a = b. Thus, the alternative, "participate," would be dominated by the alternative, "not participate," and would not "rationally" be chosen over that alternative. If these are the only alternatives available, then actor can be expected to adopt the pure strategy of never participating. See Luce and Raiffa (1957) for a theoretical treatment of this issue and Camilleri et al. (1972) for its relevance to the model used here.

member attributes efficacy to his own participation, he also believes that participation is causally relevant to outcome selection. In our experiments, we believe that $a > b$ insures $\alpha > \beta$, and we attempt to evaluate this assumption in the post-experimental interview. We also believe that if $a = b$ and $\alpha = \beta$, a member may yet conclude that participation is causally relevant to the outcome of the organizational decision-making process. We report two experimental conditions for which $a = b$; these conditions were constructed to provide some diagnostic information about our reasoning on this distinction.

The Utilities

In the following discussion, we consider the effects of structurally different situations upon participation for *fixed* utilities of the organizational outcomes and for fixed values of u_E. In doing this we postulate that while structure affects the probability of outcomes it does not affect the subjective worth of the outcomes to the members.[6]

Although we do not investigate variations in utility values, it should be noted that the choice function does permit such variations to affect the probability that a member will participate in a way which seems intuitively reasonable. The more important the alternative (i.e., the greater the value of u_X), the more likely A is to participate; and as the cost of participation increases (i.e., as u_E gets larger), participation rates decline.

Our formulation deals with only those incentives for participation associated with the intrinsic worth of the organizational outcomes for the member. There may be, of course, other incentives for participation and the additional utilities associated with these could be added to the trees, entered into the equations and evaluated for their effects on participation.[7]

Specifying α, β

α and β are determined by a series of events which A expects to happen because of his experience with the structure of the organization. The illustrative tree in Figure 1, together with its function, reflects only our ideas about individual choice behavior and the nature of the choice which collective-good decision-making opportunities present to a member. A *full* description of A's decision structure requires that this tree be elaborated to detail the branches which present our hypotheses about how structure determines the events A expects to happen.

Our task in the experiments is to specify the complete set of possible events for each situation of interest; i.e., we identify an "event tree" which details how the agreement structure constrains A's expectations about who will participate and how they will vote and which shows how the rules that define the authority distribution specify which outcome is associated with each set of events. Having written the event tree, we symbolically represent the utility of each outcome to A and write the equation for the probability that A will participate.

We have placed a number of severe constraints upon the experimental organizations we created. In each organization there are just two alternative collective goods, each with positive utility for Actor. A believes that everyone in the organization has a preference between the alternative collective goods and he knows what it is. We postulate that the order of the utilities determines the order of the preferences; i.e., A prefers X to Y because the utility of X for A is greater than the utility of Y.[8] Stalemates and No Quorums are

[6] It is possible, of course, that we are incorrect about this. For example, Lipset et al. may have meant to suggest that political parties not only affect the perceived probability that rewarding outcomes will be selected by the organization but also the additional idea that they serve to alter the perceived *worth* of collective-good alternatives. Our specification assumes that patterned disagreement does not affect utility values.

[7] Similarly, our assumption about the independence of utility values from organizational structure is not a denial of Michels' argument about the ability of officers in centralized authority systems to control agenda-setting and thus produce a situation in which highly-valued collective-good alternatives are not produced for rank-and-file valuation. Such agenda-setting would by our formulation imply a lowering of participation rates.

[8] In the purely descriptive theory of utility and choice, the causality is reversed; the utility of an outcome is higher because it is preferred. Our version is regarded as a "fallacy," yet it is difficult to see how a predictive, non-normative theory of choice could be based upon utilities unless the order of causality is as we say. This is another of the ways in which our formulation differs from conventional utility theory (Luce and Raiffa, 1957:12-38).

assumed to have zero utility for everyone. Finally, whenever it is reasonable to do so, parameters are fixed in the extreme. For example, when investigating consensus, we consider only the circumstance in which A expects the other members to agree with him *every* time. Similarly, when we say the president is perceived as not influenceable, we mean that A expects that the president will *never* be influenced.

These constraints could be modified or relaxed individually: they are imposed here only to make the experimentation and analysis manageable. There are some additional constraints which also simplify our task, but the pragmatic effects are not the main motivation for them. They are introduced as substantive assertions. Chief among these are our assumptions that participation is not itself directly rewarding and that consensus is neither valued nor rewarded directly.[9] Both of these are assumed to be rewarding only through their effects upon the probability that a preferred collective good is selected by the organization. In the experiments reported below, we take these assumptions as the actual state of affairs. We do nothing to evaluate them empirically and we do not knowingly introduce aspects into the experiments which might produce circumstances that violate them.

All the organizations we consider here are characterized by a specific combination of the possible states of the following three variables:

The agreement structure. We postulate that α and β are affected by any pattern among the members' preference ranking of the collective goods. We use the term, "agreement," generally to stand for any such pattern, not just consensus. Two agreement structures will be considered: consensus and one particular pattern of dissensus. In a consensus structure, the members consistently agree with each other and with their officer on the preference ranking of the alternatives. The dissensus structure we investigate is one which cross-cuts the general membership, dividing it into two groups in opposition to each other. Each member has a clear preference; there are no "undecided" or "indifferent" members.[10]

The authority structure. We have said that the authority structure of an organization is defined by the set of rules which distribute the formal decision-making rights over the entire membership.[11] Two extremely different distributions of these rights will be considered: a centralized distribution and a decentralized one. In the centralized organization, the president makes binding decisions; the other members' participation constitutes legitimate influence aimed at affecting the president's choice. In the decentralized organization, decisions are made by a one-person-one-vote, simple majority rule; the organization has a quorum rule which must be met for a decision to be binding upon the membership, but the president must be present. No decision so made is subject to reversal or veto by the president; but under less constraining conditions than we entertain, he would undoubtedly exert influence upon the other members.

Influenceability of officers. We restrict our attention to the effects of one particular influence process upon participation—i.e., the degree to which the officer will change his decisions when faced with disagreement from the rank-and-file. We do not regard this influence process as a structural feature, and

[9] We will not digress to argue with literature which suggests that participation is itself rewarding for the individual. Our position on this issue is in keeping with the statement found in Gamson (1968:139): "To increase the participation of a group. . .may mean to increase its influence over decisions. If there is increased satisfaction in such situations, it is because the modified outcomes are closer to what the. . .group desires. It may have very little or nothing to do with the fact of participation itself." As is the contention in *Union Democracy* that social rewards ought to be taken into account, the question can be empirically evaluated and the model obviously permits the addition of other utilities.

[10] The inclusion of such members implies processes such as bargaining, negotiation, influence and coalition formation. We do everything we can to prohibit these processes from occurring in our experiments because they represent behavior alternatives beyond and distinct from participating and not participating. From our point of view, they have to do with what a member does after he has decided to participate and with what alternative finally gets chosen by the organization.

[11] Our discussions of authority and binding decisions are based heavily upon Chapter 2 in Gamson's *Power and Discontent* (1968). We refer the reader to that chapter for a more detailed elaboration of the arguments we make here.

our approach to it will not generate information about when or why officers might be susceptible to influence. We are concerned here only with the effects on the participation of the rank-and-file of an officer who is perceived as influenceable. We believe that if an officer has a voting pattern which is variable (i.e., sometimes he votes for one alternative and sometimes for another) and that if this variability can be attributed to intra-organizational events (e.g., members' participation), then he will be regarded as influenceable. The *fact* of influenceability is all that we are concerned with and *not* the way in which the influence is accomplished.[12]

Any particular combination of the organizational variables specifies an event tree.[13] The rules that define the authority structure affect the tree in two ways. First, they tell us which branches to include in the tree (e.g., a set which represents all legal behavioral options of each legitimate voter); and second, they tell us how to associate outcomes with each branch of the tree. On occasion the rules specify the probability of events; for example, a rule which requires that the president be present in order for a decision to be binding upon the organization implies that the probability of the president's attending is equal to 1.00 for most circumstances. The agreement structure tells us how to set the probability that other members will vote for A's preference if they attend. To express our idea of an influenceable president requires adding a set of branches and one more parameter representing the likelihood that the president will change his vote when faced with disagreement from one or more of the other members.

These variables, then, tell us how to write the choice tree; they also set values for the probabilities of each event in the tree, with one important exception. The structure does not provide us *directly* with information about the non-officers' probabilities of attendance. We assume that these probabilities are inferred from a history of experience with the organization and we provide a history for each subject in our experiments.

By representing the utility value to A of each outcome and his utility for the effort entailed in participation, we indicate a particular utility structure for a member of a particular organization. We can then determine the expected gain in participation and in non-participation and write the appropriate prediction equation.

THE EXPERIMENTAL PROCEDURES

The procedures we developed for these experiments provided that:

(1) each subject have membership in a group with a designated leader and an explicit decision-making rule system;
(2) each subject be assigned preferences appropriate to his experimental condition;
(3) each subject have appropriate expectations for the preferences and participation patterns of the other group members;
(4) collective goods be provided for each group decision;
(5) participation entail some expense for the subject.

In addition to these five properties of the experiments, there are two others which we judged necessary, but our reasoning for them is not explicitly represented in the formulation as we have presented it here. The first concerns our judgment that the subject should believe the other members of his organization are real people, and the second is our decision to set the size of each organization at nine members. With regard to the first, it is reasonable to assume that the subject's participation might be affected by the knowledge that his behavior influences not only his own rewards but those of the other members as well (by virtue of its impact upon determining which alternative becomes the collective good). We suspect that either (a) such knowledge has no effect upon participation or (b) if it does, that the effect is constant for all structurally different condi-

[12] We will write as if the form of the influence is unimportant; we refer the reader who is interested in a complex idea of influence which is compatible with the approach we are taking here to Gamson's *Power and Discontent* (1968:59-92, 145-62).

[13] It would be technically impossible to display here an event tree for an organization of the size used in the experiments about to be reported; the task is one for the computer. We refer the reader who wishes a more detailed understanding of the strategy we used to the appendix which displays a very simplified example.

tions. In order not to prejudge the issue, however, we induced the subjects to believe that the other members were real people.

With regard to the problem of organizational size, we attempted to construct experimental procedures which in principle can accomodate any number of members. However, the consequences of constructing very large organizations impose requirements upon the experimental procedures which we wanted to avoid for the following reasons. Our formulation implies that increasing size will have a dampening effect upon participation rates by decreasing efficacy (i.e., $\alpha - \beta$).[14] If this is so, then our experimental procedures must be sensitive enough to detect relatively small differences between participation rates. Another consequence of large size concerns the way subjects react to information about the other members of the organization. If the organization is small, subjects can be given information about the behavior and preferences of each other member; this allows the subjects to aggregate the information as they will. If the number of other members is very large, however, we are forced to provide this information in some already aggregated form. This exposes us to the risk that the particular way in which we aggregate this information has an effect upon the participation rates. Thus, we sought an organizational size that would press the experimental procedures in terms of their sensitivity and still allow us to present the subjects with unaggregated information. By a combination of algebraic analysis and speculation, we determined that nine-member organizations would serve our purposes.

Each organization was composed, then, of nine members—the subject and eight simulated members. The subject was led to believe that the other eight "persons" in his organization were students like himself and he knew not only how the organizational decision affected his own financial outcomes, but also how it affected each of the other members. Each subject was paid $2.00 for an initial briefing session during which his organization was explained to him; this was followed by the opportunity to participate in a series of meetings of that organization, two a week for the remaining weeks in the quarter. At the end of his participation in the study, the subject was interviewed and debriefed.

Recruiting the Sample

Subjects were recruited in lower-division classes at Stanford University. They were told that we were interested in volunteers for a series of sociology studies in which they could earn from a minimum of $2.00 up to as much as $30 depending upon which study contacted them. Interested students filled out a schedule of class and work commitments and indicated their addresses and phone numbers. Volunteers who satisfied our sample conditions were then phoned and scheduled for a briefing session. At that session they learned that the study involved the opportunity for repeated participation.[15]

All subjects were males between 18 and 21 years of age. In order to minimize variation in u_E, we chose subjects who lived on campus, and all were scheduled for meeting times at least one hour after their last regular academic or work commitment of the day. To reduce the likelihood of suspicion and of communication which might result in a subject's having information about the study not given in his briefing, no two subjects resided in the same wing of the same floor in the same dormitory; subjects living in the same residence unit were given meeting times as different from each other as possible. Subjects were given no choice as to the day or hour of their organization's meetings.

To avoid systematic variation between conditions due to meeting times (e.g., early afternoon versus late afternoon) or meeting days (e.g., Tuesday and Thursday versus Tuesday and Friday), the number of organizations in each condition was evenly distributed over the entire schedule of available meeting times (1 pm to 5 pm, M W, M Th, T Th, T F,

[14] That is, increased size tends to restrict the distance between α and β; that in turn produces calculated participation rates which are similarly restricted in range. The implication is that increased size tends to lower participation rates in general, and thus rates for structurally different situations are closer together than they might be in smaller organizations. (See Olson (1965) for a similar argument.)

[15] Names of volunteers who did not meet our sample needs were made available to other studies as promised in the recruiting situation.

W F). Within each hourly unit, eight meetings were scheduled (two in each of four conditions); assignment of conditions to meeting times within each hourly unit was random.

The Initial Briefing Session

At the beginning of the quarter, each subject was paid $2.00 for an individual briefing session. He was told that in volunteering for the study (a) he had become a member of a very rudimentary organization consisting of himself and eight other members; (b) the organization had one officer, a president; (c) it had been in existence for several months. The subject was given to understand that he was replacing someone who had withdrawn at the end of the previous quarter due to graduation, a lack of schedule fit or transfer from the university.

We explained that the organization would soon resume meeting twice a week. At each meeting, the organization would make a decision involving a choice between two alternatives. One of those alternatives would be his preference and each time the organization selected it, he would receive $2.00. Each time it chose the other alternative, he would receive $.75. He correctly understood that he would receive the money *whether or not he participated.* He would receive no pay only if the organization was unable to arrive at a decision due to a stalemate or lack of a quorum. He and all the other non-officers were free to attend or not attend the meetings at will; only the president was required to be present. A report describing who had attended and which alternative had been chosen would be sent to all members who did not attend a particular meeting; any money due would be enclosed with that notice. Subjects understood also that the president would attend every meeting and would always vote. To make that appear equitable to the subject, he was told that the president was paid differently from the other members; he would receive $2.00 for every meeting at which a decision was reached. However, the president also had a preference and when it was chosen, he would receive an additional $1.00. All other members were paid as the subject was: each received $2.00 when his own preference was chosen, $.75 when the other alternative was chosen. No one, not even the president, received any money if the organization failed to arrive at a decision due to a stalemate or quorum failure.

The Authority Structure

The authority structure in the subject's organization was not labeled for him; he was simply told how decisions were made in his organization. In Decentralized Organizations, the subject was told that his organization had a one-person-one-vote, majority rule procedure and a specific quorum rule. In Centralized Organizations, the subject was told that his president's decision would determine the organizational choice, but that the president could vote only after having looked at how the members who attended that meeting had voted. The president, of course, was free to vote as he wished.

The Agreement Structure

Subjects were told that we were studying organizations which had particular agreement patterns. We also explained that we were going to use money to insure that the organization made decisions they cared about and to insure that we would be able to study the particular agreement pattern in which we were interested.

In the case of Consensus, subjects were told that we were interested in studying organizations in which members find they regularly agree with one another and their officers about the best decision. Therefore every member of the organization preferred the same alternative (labeled No. 1); i.e., each member would receive $2.00 every time that alternative was chosen.

In the case of Dissensus, each subject was told that we were interested in organizations whose members did not always agree about what was the best alternative. Consequently, he and three of the other non-officers preferred alternative No. 1 and each would receive $2.00 every time it was chosen; the remaining four members preferred alternative No. 2 and each of them would receive $2.00 every time No. 2 was chosen. In addition, we were interested in organizations whose officers were unbiased, so he could expect that the president would sometimes prefer No. 1 and sometimes prefer No. 2. However, the subject would not know in advance of any

meeting which alternative the president preferred.

Influenceability of Officers

In some Dissensus Organizations, the president was explicitly uninfluenceable by the vote of the other members; he was required to cast his vote before looking at the votes of the other members. In other Dissensus conditions, we introduced the possibility of the president's being influenced by having him cast his vote at the end of the meeting after having looked at everyone else's vote. To create the impression of a clearly influenced president, we simulated his voting behavior so that he always voted with the majority if there was one and voted against the subject's preference if there was not. We simulated a president who was not being influenced in any obvious way by having him vote randomly, half the time for alternative No. 1 and half the time for alternative No. 2.

Participation History

We acknowledged that the subject was at a disadvantage as a new member. Unlike the others, he had no idea about how things had gone in the past. Consequently, we had compiled a sheet based on ten meetings of his organization in the past; he understood that these ten meetings were not in the order of actual occurrence, nor were they all the meetings his organization had held so far. We explained that we had selected these ten because they seemed to us to be "usual" for his organization. For each meeting, the history showed each "member's" first name, whether he had attended and how he had voted if he did attend. This procedure allowed us to establish for each subject a participation expectation for each member in his organization. The member whom the subject was replacing was deleted "because we did not want him to be influenced by that person's behavior." The subject was then asked to imagine that he was that person so that he could see what would have happened at each of the meetings if he *had* attended and if he had *not* attended. In short, the history of the organization was presented by using a "member votes last" strategy (Good and Mayer, 1975). (The meetings shown on the history sheet were produced by the same simulation procedures used to produce the actual meetings which the subjects were about to experience, and the marginals of the meetings in the history were the same as those for the actual meetings.)

Consent

Finally, the subject was asked if he would agree to be a member of the organization we had just described; no one declined. He addressed envelopes to himself in which he would be sent his money and meeting reports. He was given a notice of his meeting days and times and reminded that his decision to join the organization was not a commitment to attend the meetings; he was free to attend or not attend each meeting.

The Actual Meetings

We explained during the briefing session that we were not interested in how interpersonal processes such as friendship and influence affect participation. Consequently, the subject would never actually meet the other eight members of his organization. The subject was told that the other members, like himself, would go to a room (in a different place) at the time of the meeting and register their votes on computer terminals which were all linked to the president's terminal.

When a subject attended a meeting, the terminal was used to register his vote and to monitor the other "members' " participation as the meeting progressed. At the end of the meeting (ten minutes), the final voting results were printed out on the terminal. Each member's name was listed and next to it appeared his vote. The results were copied on to a meeting report form which was given to the subject, along with any money he had due as a result of the organization's decision at that meeting. When a subject did not attend a meeting, his absence was recorded by typing "NO VOTE" on the terminal; the simulated members' action and the meeting's outcome were then copied on to the meeting report form which was mailed to the subject along with any money due him. The subjects did not know how many meetings there would be nor did they know in advance which was the final one of the quarter.

Each subject was given the same ten history meetings (differing only by condition manipulations) and the same set of actual meetings (differing only by condition manipu-

lations). The *order* of the *actual* meetings, however, was varied randomly over subjects because pretest information suggested that: (1) the sequence in which the meetings occurred had an effect on the subject's expectations (i.e., a rare event occurring on the 12th meeting would be interpreted differently than if it occurred on the 3rd meeting) and (2) when two subjects compared the forms on which their meeting results for that day were copied, identical sequences created suspicion about the reality of the other members.

The Simulation

An interactive computer program was used to simulate both the actual meetings and those used in the history. The participation of the eight simulated members did not differ by condition; only the assignment of preferences and the rules for deciding what constituted a binding decision were varied.

It was necessary that the particular distribution of participation rates for the eight simulated members seem subjectively plausible to all subjects in *all* conditions. In addition, the distribution should allow the subject to have "company" regardless of how often he chose to come (so that he would not feel "odd"); and finally, it should present no clear "norm" to follow. After pretesting, the set of rates presented in Figure 2 was settled upon: one member attended every meeting—the president; one came 70% of the time; a third, 60%; two more attended 50% each; and the remaining three came 40%, 20% and 10% respectively. To ensure that the Dissensus conditions differed as little as possible from the Consensus conditions in expected financial pay-off, preferences were distributed across the simulated members in a manner which maximally favored the subject's financial gain without producing widespread suspicion.

The simulated members were identified for the subject by first name rather than by number as in Figure 2. Each subject received a unique set of first names randomly selected from the set of names in the volunteer pool.

We conducted three sets of experiments using these procedures. The first set of experiments was designed to evaluate the effects of changes in rules which define the authority structure in two different agreement structures. A second set of experiments was designed to resolve a problem raised by the first set about the effects of rule changes in organizations with a consensus agreement structure. The third and final set of experiments was designed to evaluate the model's implications about the effects of different agreement structures in different authority structures and to replicate some of the findings in Sets I and II.

EXPERIMENT SET I: AGREEMENT STRUCTURE AND QUORUM RULES

Our formulation implies that the variables affecting participation rates act as a system, their separate effects being conditional upon

Member	Participation Probability	Alternative Preference	
		Consensus	Dissensus
(Pres.) 1	1.00	1	.5 Alt. 1, .5 Alt. 2
Member 2	.70	1	1
Member 3	.60	1	2
Member 4	.50	1	1
Member 5	.50	1	1
Member 6	.40	1	2
Member 7	.20	1	2
Member 8	.10	1	2
Subject	P_A	1	1

Figure 2. Simulation Summary

the values of the other variables. Thus we cannot easily isolate "the effect" of any single variable. In these experiments we set up two kinds of organizations, each described by a different combination of variables; we then vary one particular variable in both situations to evaluate the model's implications for these changes under the stipulated circumstances. We have elected to investigate the effects of changes in the quorum rule under different agreement structures, but within the same decentralized distribution of decision-making rights. We regard a change in the quorum rule as a small change in the authority structure of the organizations.[16]

We chose to investigate changes in the quorum rule under these conditions primarily because that rule is unambiguous, easy to operationalize and has considerable substantive relevance. Participation rates have a direct bearing on whether quorate meetings will occur with sufficient regularity to allow an organization to do business. Michels argued that there is a tendency in organizations to adapt to quorum difficulties by making binding decisions in their absence. In *Union Democracy* (Lipset et al., 1956), it is suggested that rank-and-file dissensus increases the likelihood of meetings being quorate. It is suggested there too that such dissensus also produces active resistance to illegally made decisions.

[16] In our discussion, we have spoken of centralized and decentralized authority as polar contrasts. This may suggest that authority structure can be treated as a unidimensional variable. This is not our intention. Authority structures are defined by the complex of rules which distribute decision-making rights. In this sense, then, each different set of rules defines a different authority structure. To be sure, some will closely resemble others, their differences seeming small or trivial; and some will clearly appear quite different from each other. This suggests that there might be some value in searching for a schema that groups authority structures into classes that are "theoretically equivalent." That phrase, though much used, does not have an unambiguous meaning. In any case, we were not able to devise a satisfactory classification of that sort even for our own purposes. Thus our language remains imprecise, and we will talk about "large" changes or "small" changes in the authority structure according to our impressions and intuitions about their importance either to the composition of an authority structure or to the effects upon participation rates of such changes.

We cannot now deal with these hypotheses about illegitmate decisions, but our experiments do allow us to investigate the effects upon participation of dissensus and quorate meetings. The rates predicted by the model are very sensitive to structural changes in consensus situations, but its predictions for dissensus situations are not. These rates are more affected by the relative worth of the competing alternatives and by the relative turnout of the contending parties. Changes in quorum rules affect participation rates more strongly in consensus situations than in dissensus situations. Both sets of rates are most affected by quorum rule changes within the range from just below the expected number of attenders to just above that number. The first set of experiments we report addresses the empirical accuracy of the model's ability to rank participation rates in two agreement structures under a comparable change in the quorum rule.

Specification of the First Set of Experiments

There are four experimental conditions in this set. Two conditions are composed of organizations in Consensus: one of these with a quorum rule of three out of nine members (one less than the expected number of attending simulated members); the other with a quorum rule of five (one more than the expected number). The other two conditions are composed of organizations in Dissensus, with the same difference in quorum rule between them. The organizations in all four conditions employed a one-person-one-vote, simple majority rule for arriving at a decision. No decision was binding unless the president attended and unless the quorum rule was met. The president was required to vote before he saw anyone else's vote, which means he was uninfluenceable and could not break stalemates nor be held accountable by the subjects for producing stalemates. These rules, together with the simulation outlined in Table 1, permit us to write the utility matrices for subjects in each of the four conditions. Table 1 summarizes this information.

The Predictions

The utilities listed in Table 1 must be estimated from the data. The probabilities are determined by experimental manipulation, specifically by the simulation which produces

Table 1. Utility Matrices: Experiment Set I

	Dissensus: Quorum = 3 Meeting Outcomes			Dissensus: Quorum = 5 Meeting Outcomes		
	Alt. 1	Alt. 2	No Decision	Alt. 1	Alt. 2	No Decision
c_A	$u_{\$2.00}$, u_E (.70)*	$u_{\$0.75}$, u_E (.10)	$u_{\$0.00}$, u_E (.20)	$u_{\$2.00}$, u_E (.50)	$u_{\$0.75}$, u_E (.00)	$u_{\$0.00}$, u_E (.50)
$c_{\bar{A}}$	$u_{\$2.00}$ (.40)	$u_{\$0.75}$ (.30)	$u_{\$0.00}$ (.30)	$u_{\$2.00}$ (.20)	$u_{\$0.75}$ (.20)	$u_{\$0.00}$ (.60)

	Consensus: Quorum = 3 Meeting Outcomes			Consensus: Quorum = 5 Meeting Outcomes		
	Alt. 1	Alt. 2	No Decision	Alt. 1	Alt. 2	No Decision
c_A	$u_{\$2.00}$, u_E (1.00)	**	$u_{\$0.00}$, u_E (.00)	$u_{\$2.00}$, u_E (.70)	**	$u_{\$0.00}$, u_E (.30)
$c_{\bar{A}}$	$u_{\$2.00}$ (.90)	**	$u_{\$0.00}$ (.10)	$u_{\$2.00}$ (.30)	**	$u_{\$0.00}$ (.70)

c_A signifies Actor participates: $c_{\bar{A}}$ signifies Actor does not participate.

*The quantities in parentheses are the objective probabilities of the outcomes and are used as estimates of the subject's subjective likelihoods.

**In Consensus conditions, Alternative 2 is never chosen.

both the meetings used for the organizational histories and the set of meetings which the subject experiences. The numbers listed in Table 1 are the objective probabilities that the specified outcomes will occur if the subject always attends or if he never attends. Thus, for example, in the Consensus-5 case, if the subject attends all of ten meetings, alternative No. 1 would be chosen seven times and three meetings would fail to achieve a quorum. If he never attends a meeting, alternative No. 1 would be chosen three times and seven meetings would fail to attain a quorum. We use these same probabilities on the history sheet in the briefing session. Although we use these values as estimates of the subject's corresponding subjective probabilities, we cannot know how close they are to the subject's actual values. In the post experimental interview, we try to learn whether the subjects' subjective appraisals approximate the objective numbers.

The predicting equations, then, for each of the four conditions in this set are as follows:

Consensus, Quorum = 3:

$$P_A = \frac{r_1}{1.9r_1 + 1}$$

Consensus, Quorum = 5:

$$P_A = \frac{.7r_1}{r_1 + 1}$$

Dissensus, Quorum = 3:

$$P_A = \frac{.7r_1 + .1r_2}{1.1r_1 + .4r_2 + 1}$$

Dissensus, Quorum = 5:

$$P_A = \frac{.5r_1}{.7r_1 + .2r_2 + 1}$$

where P_A is the probability that A participates (i.e., the participation rate), r_1 and r_2 are the ratios of u($2.00) and u($.75) to u(effort). These ratios appear in the equations because we cannot, with the information available in the experiments, determine the values of the utilities except up to an arbitrary, non-zero multiplier. The ratios are computable under the assumption that they are constant across experimental conditions and individuals. We always assume $r_1 > 1.00$, $r_2 \geqslant 0.00$, and $r_1 \geqslant r_2$.

We cannot determine a complete ordering of the participation rates over the four conditions on analytic grounds, but we can derive the following: (1) the participation rate for Consensus-5 is greater than that for any of the other conditions; (2) the rates for Consensus-3, Dissensus-5 and Dissensus-3 can take any order, depending upon the value of r_1 and r_2; and (3) the absolute difference between the rates of Consensus-5 and Consensus-3 is greater than that between those of Dissensus-5 and Dissensus-3.

Criteria of Fit

Our general procedure for evaluating the model is to calculate rates from the appropriate equations using estimates of r_1 and r_2 derived from the data and to apply the following criteria: (1) whatever rank order implications can be derived *a priori* from the model must obtain in the observed rates; (2) the complete rank-order of the calculated rates must match that of the observed rates. We will not evaluate the closeness of the calculated rates themselves to the observed ones principally because we do not believe our observational procedures are precise enough or the model complete enough to warrant a severe test. We know, however, from an analysis of the equations that if the rank-order implications are met, the calculated rates must be similar in magnitude to the observed ones.

Results

The observed and calculated rates are summarized in Table 2. Two partitions of the data are displayed, one labeled "good" and the other "total." The experimental procedures involved deception and a lengthy communication of detailed information about the organization's history in the initial briefing session. To detect suspicion and to determine whether the history had communicated appropriate expectations which were still unchanged some seven weeks later, each subject was scheduled for an interview during the week following his organization's final meeting of the quarter. A subject's data

Table 2. Observed and Calculated Participation Rates: Experiments Set I

Condition	Good Subjects		Total Subjects	
	Observed	Calculated**	Observed	Calculated**
Consensus Quorum = 5	.59 (9)*	.58	.56 (18)	.55
Dissensus Quorum = 3	.47 (13)	.47	.45 (19)	.45
Dissensus Quorum = 5	.43 (10)	.45	.40 (18)	.43
Consensus Quorum = 3	.59 (16)	.47	.59 (18)	.46

*Number of subjects in condition. Twenty subjects were briefed for each condition; each subject participated in 12 trials.

**The calculated rates of good subjects data were solved using $r_1 = 4.83$ and $r_2 = 4.70$; the calculated rates of total subjects data were solved using $r_1 = 3.70$ and $r_2 = 3.63$. r_1 was estimated in both cases from the Consensus-5 condition and r_2 was estimated in both cases from the Dissensus-3 condition.

were classified in the "good" set if, in reply to open-ended questions:

(1) he could identify the purpose of the study and replied in the negative when asked if he ever believed we were studying anything else;
(2) he indicated that he believed the other members of his organization were real people and that the computer connection allowed him to witness their actual attendance and voting behavior;
(3) he expressed no other suspicions either about the study's purpose or about details we might be controlling or concealing;
(4) he could accurately detail the composition and decision-making rules of his organization without prompting;
(5) he gave *accurate* subjective likelihood estimates about what might happen at the meetings—i.e., his reported subjective expectations matched the objective probabilities used in the equations.

These are stronger criteria than are strictly required by our argument. We have not argued that it is necessary that the subjects believe the other members of their organizations are real people, nor have we stipulated that the subjects must be able to articulate their expectations for the model to be applicable. Consequently, the main implications of the model ought to apply even to subjects who fail to meet the above conditions. We therefore examine the total data as well as that of the subset of subjects defined as "good" by the stated criteria. A few subjects were excluded from the analysis completely, i.e., subjects whose behavior appeared to be governed by factors external to the experimental situation (such as some who were in the hospital during most of the experimental period). We expected that subjects who were suspicious about the reality of the other members in their organization might conclude that participation was not relevant to the meeting outcomes and thus violate our scope condition requiring a belief in causal relevance. Had this happened, we would have dropped their data from analysis completely; however, the interviews indicate that subjects did believe that their behavior was causally taken into account as described in the briefing even if they suspected that the organization was a simulation. As a result, suspicious subjects' data were kept in the analysis but not included in the subset labeled "good."

The rank-order of the participation rates is our primary concern. The rank-orders of the observed rates in Table 2 were stable when partitioned by day and time of meeting and by experimenter.

In order to calculate rates, we need estimates of r_1 and r_2. In the following sets of experiments, our general procedure for estimating these quantities is as follows. An estimate of r_1 can always be obtained by substituting the observed rate for a Consensus condition into its equation and solving for r_1. This estimate in turn can be substituted into the equation for one of the Dissensus conditions (along with its observed rate) to yield an estimate of r_2. These values can then be entered into the equations for the remaining conditions to produce calculated rates for them. Each single estimate of r_1 (or r_2) is actually the mean value of a *range* of estimates; and each value in that range produces the same calculated rate (rounded off to two places). That is, although solving any single equation produces a single value for r_1 (or r_2), that value is only one of a set of numbers in a range which will produce a calculated rate equal to the observed one. What we actually do, then, is compute the *full set* of values of r_1 (and then r_2) which produce a calculated rate equal to the observed one *or* which is within .01 of the observed one. Restricting ourselves to that range, we choose a pair of values (r_1, r_2) so that $r_1 \geqslant r_2 \geqslant 0$, $r_1 > 1.00$, and so that they give calculated rates which best fit the observed ones for all conditions. This procedure does not affect the rank-order of the calculated rates or of their differences, but it does improve the closeness of fit between observed and calculated rates.

For Table 2 we estimated r_1 and r_2 for each partition of the data separately. The calculated rates for the good data in Table 2 are based upon $r_1 = 4.83$ and $r_2 = 4.70$; r_1 was estimated from the good data in the Consensus-5 condition, and r_2 was estimated from the good data in the Dissensus-3 condition. The calculated rates for the total data are based upon $r_1 \doteq 3.70$ and $r_2 = 3.63$; as with the good data, Consensus-5 and Dissensus-3 were the conditions used to estimate those values.

We used the data of the good subjects in

Consensus-5 to estimate r_1 on the following grounds: the observed rate for Consensus-3 was greater than that for Consensus-5, which is contrary to the *a priori* implications of the model. Whereas we could obtain an estimated value of r_1 from the Consensus-5 data that satisfied our requirements, all estimates of r_1 from the Consensus-3 data obtained from using participation rates within the neighborhood of the observed one were negative in value, a consequence of the fact that the observed rate exceeded the analytic maximum permitted by its equation. We were inclined by these facts to consider the results observed for Consensus-3 as subject to some kind of error. We could strengthen the confirmatory value of the close fit of the other three conditions if we had independent evidence that the high rate for Consensus-3 was due to factors external to the model.

If there is an error in Consensus-3, it does not appear to us to be due to sampling variation. The sample size is greatest in this condition and the participation rates of the individual subjects has a very small variance. It occurred to us that we may have introduced a procedural artifact which affected only this condition. Subjects in this condition were "almost assured" (to use their phrase) that they would receive $2.00 if they attended. This assured payoff was not true of the other condition. We may have introduced into this condition, then, the "paycheck phenomenon" familiar to the work setting of the everyday world, i.e., absentee rates are lower than average on pay-days. We suspect that subjects in this condition were not there to participate but were merely stopping by to pick up their money. If this caused the participation rate to be inflated, it should be correctable in a replication of the experiment in which all money due is mailed regardless of attendance.

There is, however, an alternative explanation for the observed rank-order. The observed participation rates are in the same order as the total expected payoff for each condition. We are skeptical of this explanation. If amount of payoff were the single important issue, we would expect to see an intertrial dependency such that the probability of participation on trial n + 1 depended upon payoff on trial n. For the data in each condition reported in Table 2, participation on trial n + 1 was independent of payoff on trial n. The explanation cannot, however, be discounted entirely on the available evidence. The next set of experiments was designed to help us decide between these alternative explanations of the results of the first set.

EXPERIMENT SET II

We ran ten new subjects in each of three conditions, Consensus-3, -4 and -5, employing the new payment-by-mail procedure. The replications of Consensus-3 and -5 were to provide a direct check on the effects of that procedure and so, hopefully, to clarify the status of the earlier Consensus-3 experiments. Taken together, these experiments should according to our formulation, have participation rates in inverse rank-order to their total expected payoffs. We added Consensus-4 to increase the power of the test. If the rank-order predicted by the model obtains, this can be construed as evidence that the subjects are being affected by the structure of the circumstances we created and not purely by the economic gain possible.

In addition to the new subjects, in each of these conditions we ran some good subjects who had taken part in our earlier experiments. Nine subjects who had taken part in the earlier Consensus-3 experiment were continued in the new Consensus-3 condition, and nine who had taken part in the earlier Consensus-5 experiment were continued in the new Consensus-5 experiment. Ten subjects who had taken part in either Dissensus experiment were put in the Consensus-4 experiment. The purpose of continuing subjects who had already taken part in the earlier experiments was to provide a comparison for possible changes in utilities and to give us some indication of the effects of a longer series of trials.

The last condition involved Consensus organizations with a quorum rule of two. This condition was designed to investigate a circumstance in which the subject was assured of winning $2.00 every meeting whether or not he attended. That is, we wanted to construct a circumstance in which $\alpha - \beta = 0$. Causal relevance was assured because the subject knew that if he attended, his own vote and the president's vote were sufficient to produce a decision. However, the subject learned through the history used in briefing that he could expect to receive $2.00 every time, even if he never attended. We included

Table 3. Utility Matrices: Experiment Set II

	Consensus: Quorum = 5 Meeting Outcomes		Consensus: Quorum = 4 Meeting Outcomes	
	Alt. 1	No Decision	Alt. 1	No Decision
c_A	$u_{\$2.00}$, u_E (.70)*	$u_{\$0.00}$, u_E (.30)	$u_{\$2.00}$, u_E (.90)	$u_{\$0.00}$, u_E (.10)
$c_{\bar{A}}$	$u_{\$2.00}$ (.30)	$u_{\$0.00}$ (.70)	$u_{\$2.00}$ (.70)	$u_{\$0.00}$ (.30)

	Consensus: Quorum = 3 Meeting Outcomes		Consensus: Quorum = 2 Meeting Outcomes	
	Alt. 1	No Decision	Alt. 1	No Decision
c_A	$u_{\$2.00}$, u_E (1.00)	$u_{\$0.00}$, u_E (.00)	$u_{\$2.00}$, u_E (1.00)	$u_{\$0.00}$, u_E (.00)
$c_{\bar{A}}$	$u_{\$2.00}$ (.90)	$u_{\$0.00}$ (.10)	$u_{\$2.00}$ (1.00)	$u_{\$0.00}$ (.00)

c_A signifies Actor participates: $c_{\bar{A}}$ signifies Actor does not participate.

*The quantities in parentheses are the objective probabilities of the outcomes and are used as estimates of the subject's subjective likelihoods.

this condition primarily to evaluate our reasoning about the likelihood that subjects would believe in causal relevance under the condition that $\alpha - \beta = 0$. We ran only eight subjects in this condition, all of them good subjects from the two Dissensus experiments previously conducted.

Procedural Changes

Subjects continuing in the same organization in which they previously held membership were scheduled for a briefing session at the beginning of the academic quarter during the same two-day period in which the new subjects were briefed. All subjects were told that every member has an opportunity to withdraw from the experiment at the end of the quarter and that if more than two withdraw, we disband the organization. We told the continuing subjects who were changing conditions that we had had to disband their previous organization and we were giving its remaining members an opportunity to fill in for withdrawing members of other organizations. In every case the subject indicated a wish to continue and the briefing proceeded as for new subjects, except for an explanation about the change in payment procedure. To explain that change to the continuing subjects, we confided to them we had been victim of two thefts and felt it unwise to keep such large amounts of cash around openly.[17]

Except for these matters, the briefing was essentially the same for old and new subjects. A "history sheet" of his organization for the previous quarter was included with the subject's participation record deleted "so he could review how things had been going on in the past."

Apart from the differences in briefing and payment procedure, the experimental procedures were identical to those used in the earlier set of experiments.

[17] The thefts were real, not deception. We also indicated that we had reason to believe that the thefts could not be attributed to one of the subjects in the study (also a fact, not deception).

The Predictions

The utility matrices for these four conditions are presented in Table 3.

The equations for the conditions are as follows:

$$\text{Consensus, Quorum} = 5: \quad P_A = \frac{.7r_1}{r_1 + 1}$$

$$\text{Consensus, Quorum} = 4: \quad P_A = \frac{.9r_1}{1.6r_1 + 1}$$

$$\text{Consensus, Quorum} = 3: \quad P_A = \frac{r_1}{1.9r_1 + 1}$$

$$\text{Consensus, Quorum} = 2: \quad P_A = \frac{r_1}{2r_1 + 1}$$

If the error in fit for the earlier Consensus-3 condition was due to the payment procedure, the participation rates should be ranked (from greatest to least) in the following order: Consensus-5, -4, -3, -2. According to calculations from the model, the difference between the rates for Consensus-5 and Consensus-4 should be greater than that between Consensus-4 and Consensus-3.

Results

Table 4a, 4b reports the main results for new subjects and continued subjects.

Two sets of calculated rates appear in Table 4a. The first set is based upon estimates of $r_1 = 4.29$ for the good subjects' data and $r_1 = 2.46$ for the total subjects' data. Each r_1 is a weighted average of the individual estimates derived form solving each condition separately, excluding the data of the good subjects in Consensus-5 because the observed rate for that condition is beyond the maximum consistent with its equation. The second set of calculated rates in Table 4a is based upon the estimates of r_1 used in Set I. Table 4b presents the data for the continued subjects. The estimate of r_1 (1.17) computed from these data is a weighted average of the individual estimates derived from the separate conditions, excluding the data from the Consensus-2 condition because the observed rate for that condition was beyond the maximum consistent with that equation. (We used a weighted average here because all conditions are equivalent estimators of the same quantity, r_1.)

The hypothesis that total expected payoff is the principal determinant of participation implies that the participation rates be ranked in the following descending order: Consensus-2, -3, -4 and -5. The observed rates, however, are in the exact opposite order for every partition of the data reported in Table 4a, 4b. Moreover, the rank-orders of the observed rates are stable when partitioned by trial and by experimenter. On these grounds, we are inclined to discount the payoff hypothesis as an explanation of the results in Set I.

Our model, however, implies exactly the observed rank-order and only that rank-order. Further, the differences between the calculated rates are in the same rank-order as in the observed rates for the new subjects' data. The difference between the rates for Consensus-3 and Consensus-4 and the difference between Consensus-5 and Consensus-4 for the continuing subjects are in the wrong rank-order. It should be noted that the rank-order implications of these four equations are the same for every value of $r_1 > 1.00$. Variation in the value of r_1 affect only closeness of fit of the calculated rates to the observed ones.

The value of r_1 for the new subjects is essentially the same as that of the continued subjects when they were in Set I. However, r_1 for the continued subjects in this set is strikingly lower. In addition, the shapes of the graphs of the participation rates over trials for the new subjects are very similar to those for the continued subjects in Experiment Set I: there is rather high variability in the rates of the first few trials, a decline in the middle ones and a slight increase in the final trials. The graphs for the continued subjects indicate that they began participating in the Set II trials at approximately the same rate they ended the trials of the previous one; but the rate for those in Consensus-5 then decline gradually over the last nine trials of the current experiment, while those in Consensus-3 decline sharply on the second trial. It is our opinion that this sharp initial drop is due to the change in the payment procedure. After the third trial, the shapes of the graphs for both conditions are quite similar. Comparisons with Consensus-4 are difficult because these subjects changed conditions; however, their graphs appear very similar in shape to that for Consensus-5. It is clear that the earlier experience of the continued subjects had definite effects upon their

Table 4. Observed and Calculated Participation Rates: Experiment Set II.

4a. New Subjects

Condition	Good			Total		
	Observed	Calculated		Observed	Calculated	
		$r_{1(II)}$	$r_{1(I)}$		$r_{1(II)}$	$r_{1(I)}$
Consensus-5	.78 (5)*	.57	.58	.54 (9)	.50	.55
Consensus-4	.49 (9)	.49	.50	.45 (10)	.45	.48
Consensus-3	.47 (6)	.47	.47	.39 (9)	.43	.46

$r_{1(II)}$ = 4.29 for the Good data and $r_{1(II)}$ = 2.46 for the Total data; both estimates are weighted averages of the estimates from each condition in the Set II data. $r_{1(I)}$ = 4.83 for the Good data and $r_{1(I)}$ = 3.70 for the Total data; these estimates are based on data from Set I.

*Final number of subjects in condition. Ten subjects were briefed for each condition; each subject participated in 10 trials.

4b. Continued Subjects

Condition	Total	
	Observed	Calculated
		$r_{1(II)}$
Consensus-5	.41 (8)*	.38
Consensus-4	.39 (10)	.37
Consensus-3	.30 (9)	.36
Consensus-2	.18 (8)	.35

$r_{1(II)}$ = 1.17 for the Total data: this estimate is a weighted average of the estimates from each condition in the Set II data.

*Final number of subjects in condition. One subject (in Consensus-5) was dropped from analysis because he was in the hospital much of the quarter.

participation patterns and utility ratios, yet in a general way they participated as the model indicates. Their behavior seems to reflect the influences of both their previous history and the structure of the current situation.

There is one final matter we wish to discuss—the case of Consensus-2. The data for that condition clearly indicate that the subjects did not adopt an obvious pure strategy. The subjects sometimes came to the meetings and the instances of participation were not heavily loaded on the early trials. One interpretation of their attendance is that despite our efforts to prevent it they felt guilty toward the experimenter and came to alleviate their guilt. In the post-experiment interview, the subjects were queried on this and replied in the negative. When asked how likely they thought they were to receive $2.00 when they did not attend, they replied "every time." When asked about the likelihood of receiving $2.00 when they did decide to come to a meeting, they answered, "I was sure to win if I came." As we have already indicated, the rank-order of the calculated rates is correct; but since the model grossly overestimates this rate, the difference between the calculated rate and that of Consensus-3 is in the wrong order with respect to the other between-rates differences of the continued subjects.

On the foregoing considerations, we conclude that the rank-order error in Experiment Set I was due to the payment procedure and that both sets of experiments support the general implications of the model.

EXPERIMENT SET III: CENTRALIZED ORGANIZATIONS

In this set of experiments, we sought to represent a structure which was both theoretically and phenomenologically different from those in Experiment Sets I and II: a completely centralized authority structure in which the president's choice was binding on the organization regardless of the preferences of the rank-and-file and of their attendance. We designed two conditions with this authority structure. Both conditions were composed of organizations with Dissensus structures exactly as created in Experiment Set I.

In these experiments, subjects were told that the president would look at the votes of the other members before he voted. Thus, contrary to the situation in the earlier experiments, there seemed to be the possibility of influencing the president's vote. In the first Centralized condition, the president's history of voting indicated that he was influenced but biased. The "history" showed that the president voted with a majority when there was one, but always voted for alternative 2, when there was not. We denote this experiment Centralized, Dissensus, Influenced. In the other Centralized condition, the influenceability of the president was left ambiguous. His history showed a variable voting pattern, such that he voted for alternative 1 (the subject's preference) half the time and for 2 half the time; but his voting was not apparently (or in fact) related to the attendance of the rank-and-file: sometimes he voted with the majority, sometimes against it. We denote this condition Centralized, Dissensus, Not-Influenced.

The Centralized, Dissensus, Not-Influenced condition is another for which $\alpha - \beta = 0$. In briefing subjects for this condition, we made no special effort to establish a belief in causal relevance. If the subjects interpret the president's behavior to mean that their attendance and voting preferences are irrelevant to his choice, then by the argument presented earlier, we would not expect them to participate. In the interviews, these subjects indicated to us that since the president had to look at their votes before he voted, it was *possible* to influence him. They interpreted his voting pattern to mean that he was trying to "be fair to both sides." When asked about how likely they were to receive $2.00 if they attended a meeting, they replied, "about 50/50." When asked how likely it was that they would receive $2.00 if they did *not* attend a meeting, they said, "about the same, 50/50."

To provide sufficient data to estimate r_1 and r_2 and to increase the severity of the rank-order criteria of fit, we included two additional experiments in the set: Decentralized, Dissensus-3 and Decentralized, Consensus-3. The two Decentralized experiments also provide some evidence on the reproducibility of the results of the complex experimentation procedures we had employed in the entire collection of experiments. Finally, the Decentralized, Dissensus-3 condition provides a special contrast for the Centralized, Dissensus, Not-Influenced con-

Table 5. Utility Matrices: Experiment Set III

	Centralized, Dissensus Not Influenced Meeting Outcomes			Centralized, Dissensus Influenced Meeting Outcomes		
	Alt. 1	Alt. 2	No Decision	Alt. 1	Alt. 2	No Decision
c_A	$u_{\$2.00}$, u_E (.50)*	$u_{\$0.75}$, u_E (.50)	**	$u_{\$2.00}$, u_E (.80)	$u_{\$0.75}$, u_E (.20)	**
$c_{\bar{A}}$	$u_{\$2.00}$ (.50)	$u_{\$0.75}$ (.50)	**	$u_{\$2.00}$ (.40)	$u_{\$0.75}$ (.60)	**

	Decentralized, Dissensus: Quorum = 3 Meeting Outcomes			Decentralized, Consensus: Quorum = 3 Meeting Outcomes		
	Alt. 1	Alt. 2	No Decision	Alt. 1	Alt. 2	No Decision
c_A	$u_{\$2.00}$, u_E (.70)	$u_{\$0.75}$, u_E (.20)	$u_{\$0.00}$, u_E (.10)	$u_{\$2.00}$, u_E (1.00)	***	$u_{\$0.00}$, u_E (.00)
$c_{\bar{A}}$	$u_{\$2.00}$ (.40)	$u_{\$0.75}$ (.40)	$u_{\$0.00}$ (.20)	$u_{\$2.00}$ (.90)	***	$u_{\$0.00}$ (.10)

c_A signifies Actor participates: $c_{\bar{A}}$ signifies Actor does not participate.

*The quantities in parentheses are the objective probabilities of the outcomes and are used as estimates of the subject's subjective likelihoods.

**In Centralized conditions, a decision is always made.

***In Consensus conditions, Alternative 2 is never chosen.

dition, which it resembled in most ways except for the decision rule.[18]

Procedures

As in the second set of experiments, all subjects received their money by mail, whether or not they had attended a meeting. We have already noted that, in these experiments, the presidents voted after seeing the votes of the rank-and-file. In presenting the history of an organization, we summarized our comments about the president's voting pattern by saying to subjects in conditions where the president was to be pictured as definitely influenced, "it seems as though your president is voting with the majority." Where the president's influenceability was to be inferred by the subjects, we said, "it is not clear to us exactly what your president is doing." As we noted above, subjects in this condition indicated in the interviews that the manipulation succeeded in inducing a picture of a potentially influenceable and fair president. All other procedures were as followed in the previous experiments.

The Predictions

The utility matrices for these four conditions are presented in Table 5.

The equations for the conditions are:

[18] The two Decentralized conditions are not strict replications. As we noted, in this Experiment Set the president votes at the end of the meeting after having viewed the other members' votes. In the Decentralized, Dissensus-3 condition, this means that the president can now be held accountable for stalemates. To avoid judgmental attributions based on this accountability, we selected meetings with fewer stalemates than in Experiment Set I. This results in an equation with probabilities of u_Y which are slightly different from those in the equation for this condition in Experiment Set I.

Centralized, Dissensus, Influenced:

$$P_A = \frac{.8r_1 + .2r_2}{1.2r_1 + .8r_2 + 1}$$

Centralized, Dissensus, Not-Influenced:

$$P_A = \frac{.5r_1 + .5r_2}{r_1 + r_2 + 1}$$

Decentralized, Dissensus, Not-Influenced-3:

$$P_A = \frac{.7r_1 + .2r_2}{1.1r_1 + .6r_2 + 1}$$

Decentralized, Consensus, Not-Influenced-3:

$$P_A = \frac{r_1}{1.9r_1 + 1}$$

The Results

Table 6 summarizes the observed and calculated rates by condition for the total subjects; we do not display the data for the good subjects because it is not different from the total data. The calculated rates are based on $r_1 = 2.35$ (estimated from the data in the Consensus-3 condition) and $r_2 = .03$ (estimated on the data from the Centralized, Dissensus, Influenced condition). The rank-order correspondences are accurate, and the actual rates are in all cases quite close to the observed ones.

The Centralized, Dissensus, Not-Influenced condition is especially interesting. The subjects' participation rate is very close to that calculated rate even though $a = b$. Although we took no special pains in the procedures to produce a belief in casual relevance, all 20 subjects implied such a belief when they reported that the president was susceptible to influence because he saw the votes. The major difference between this condition and the Consensus-2 condition in the Set II data is that the latter situation is not obviously probabilistic, since $a = b = 1.00$ for that condition. It is possible, then, that the model will not closely approximate participation rates when the structure presents a member with a "sure thing" decision even when there is a belief in causal relevance.

Table 6. Observed and Calculated Participation Rates: Experiment Set III

Condition	Total Subjects	
	Observed	Calculated**
Centralized, Dissensus Influence	.50 (19)*	.49
Decentralized, Dissensus Quorum = 3	.48 (20)	.46
Decentralized, Consensus Quorum = 3	.43 (28)	.43
Centralized, Dissensus No Influence	.37 (20)	.35

*Number of subjects in condition. Twenty subjects were briefed for each condition except Decentralized, Consensus-3 for which thirty subjects were briefed; each subject participated in 12 trials.

**For the calculated rates, $r_1 = 2.35$ and $r_2 = .03$; r_1 was estimated using the data from the Decentralized, Consensus-3 and r_2 was estimated from the data for the Centralized, Dissensus, Influence condition.

Discussion of Over-all Results

We have been able to produce good correspondence between the rank-orders of the calculated and observed rates within three sets of experiments carried out over a period of 15 months. We take this as substantial evidence that the model traces the main experimental results.

Additional information about the adequacy of our formulation can be gained from an examination of the utility ratios used to produce the calculated rates. What is at issue here is whether the estimated ratios, r_1 and r_2, make sense according to our ideas about utilities. These ideas are extraneous to the choice function itself, which tells us only how to operate on the estimated ratios to produce a calculated rate. Our stipulation that the ratios be constant over subjects and experimental conditions can be considered only a crude simplification of the actual state of affairs. Since both r_1 and r_2 are based on the utility of effort, even if the utility of money were constant, these ratios must be expected to vary over subjects and time because of the variability of competing opportunities (to participation) offered by the environment. We have no idea what the distribution of these opportunities might be; hence we have no idea of the distribution of the ratios. We have operated on the assumption that within an experimental set, the parameters of that unknown distribution would be fairly stable. We could not expect them to be stable over academic quarters or climatic seasons. On the other hand, the ratio of r_1 to r_2 (which is to say, the ratio of \$2.00 to \$0.75) ought to be fairly stable over the total period of our experimentation, given that no catastrophic changes in economic circumstances occurred within that time. The ratio of r_1 to r_2 was indeed about the same for all conditions, except the centralized ones of Set III. We will return to that exception presently.

The estimated values of r_1 and r_2 used to produce our calculated rates are collected for all experiments in Table 7. It can be seen there that r_1 for the good subjects' data is higher than the r_1 for the total data. Included in the total category are subjects who reported subjective probabilities which did not accurately match the histories or who were mistaken about details of the rules of their organization or who suspected that the other members were not real people. Nevertheless, the observed rates for both good and total data are fairly similar and the model approximated both well. These facts encourage us to believe that neither a precisely accurate perception of the probabilities and structural elements of the situation nor an explicitly conscious consideration of them are necessary for structural factors to affect participation rates in the way we describe.

There is a sharp difference in r_1 between the new and continued subjects of Set II. Since the good data for the new subjects produces an r_1 which is about the same as that for the good subjects of Set I, we suspect that the lower value for the continued subjects is an effect of their previous experience.

There is a difference between the values of r_1 and r_2 for Set III and those for Set I. Set

Table 7. Summary of Estimated Utility Ratios

	Good Subjects			Total Subjects		
	r_1	r_2	N	r_1	r_2	N
Set I	4.83	4.70	48	3.70	3.63	73
Set II						
New	4.29	--	20	2.46	--	28
Continued	--	--	--	1.17	--	35
Set III	2.35	.03 2.00*	65	2.35	.03 2.00*	87

*Maximum value of r_2 based on Decentralized Dissensus-3 condition.

III experiments were conducted a full year after the Set I experiments, so this difference possibly may reflect some amount of change in the utility of money. It seems more likely to be an effect of the marked difference in organizational structure—decentralized versus centralized. Another possible indication of such an effect is the exceptionally low value of r_2 in the Centralized conditions. An r_2 close to zero is the only value that gives a reasonable match between the calculated and observed rates for these conditions, but a value of r_2 as high as 2.00 gives a good match for the Decentralized condition in that set.

A tentative explanation for the difference between the ratios for the Centralized and Decentralized conditions may be developed from the following considerations. Whereas in Centralized conditions subjects always receive *some* money at each meeting, in Decentralized ones they may receive *nothing* for a given meeting (because of stalemates and quorum failures). Subjects in Centralized conditions may, therefore, make a sharper distinction between amounts of money available, while subjects in Decentralized Dissensus conditions may distinguish more sharply between some money and no money. This, if true, would explain the pattern of ratio values and contradict our assumption that the structure of an organization does not affect the worth of collective goods.

DISCUSSION AND IMPLICATIONS

Although we were heavily influenced by many of the basic findings and controversies in the literature, our research strategy was not directly addressed to these issues. Instead, we designed the experiments to evaluate some implications with systematic import for the details of our argument so that the evidence would have disconfirmation value for the formulation as a whole. Our findings encourage us to believe we have made some significant progress in developing and supporting a formulation which warrants further theoretical and empirical investigation. However, a research strategy more closely linked to the literature would have led to quite different experimental conditons focused on the relevance of our argument to other discussions of voluntary participation; and so it seems appropriate now for us to identify those aspects of our work which we think have important and general implications.

First, our formulation permits an articulation of the joint effects of structural variables in different combination; this was the focus of Experiment Sets I and II concerning the differential impact of quorum rule changes in consensus and dissensus organizations. The reasoning underlying those experiments is not exactly consistent with the implication in *Union Democracy* (Lipset et al., 1956) that patterned dissensus uniformly increases participation rates. Our algebraic analysis suggests that dissensus does not produce rates which are always higher than those which consensus produces, but rather that dissensus produces rates which are relatively insensitive to the effects of all changes in structure except those which affect the relative strength of the opposed parties.

Further, as we have formulated the process, there appears to be no simple empirical relationship between "degree of centralization" of an authority system and participation rates. For example, our argument implies that in polar cases of centralization the officers must be influenceable if members are to participate at all; but if they are, participation is not necessarily less than in decentralized organizations. This implies that participation rates are not likely to be an accurate index of decentralization. Because changes in the various rules that define an authority structure reflect themselves differentially in participation rates, our formulation indicates a conceptualization of authority structure which is configurational rather than unidimensional. There are different ways to alter centralization (e.g., adding or removing rights of referendum, giving or revoking from the officers the right of veto, and so on); it is not apparent how such changes can be made commensurable so as to permit a unidimensional ordering of authority structures *in general.* Thus, although we cannot classify the various events Michels (1915) describes in terms of their "degree of centralization" in a theoretically general way, our formulation allows us to examine the effects of those configurations upon participation.

A third set of implications has to do with the way that structure and the participation patterns of others determine efficacy. To the extent that α and β are based upon the objective likelihoods, a and b, organizational variables are sources of efficacy which does not depend upon interpersonal influence. By

implication, any organizational variable which produces $a > b$ should be a factor which mitigates against the dynamics described by Michels as leading to the "apathy of the masses." Our interpretation of the *Union Democracy* argument is that dissensus produces $a > b$; and it produces rates which are insensitive to rule changes which, in consensus organizations, might lower participation rates by reducing the difference between a and b (e.g., by a reduction in the quorum rule). However, dissensus is not, by our argument, the *only* organizational source of a difference between a and b; for example, a representational voting system for elections should create greater efficacy than a "winner takes all" system.

In addition, our specification of α and β asserts that $a = b$ does not necessarily imply an absence of participation, although we do not wish to argue that participation under these conditions is sufficient to inhibit Michels' dynamic. (If $\alpha = \beta$, P_A has an upper limit less than .50.) We cannot, of course, draw strong conclusions from the two such cases in the experiments reported here; nevertheless, we are encouraged to continue investigating the empirical conditions under which members will apparently hold a belief in the causal relevance of participation when $a = b$ and to describe participation under this condition for various levels of a and b. It seems to us that this issue is particularly important to Olson's (1965) stress on the importance of size on the likelihood that individuals will participate in providing collective goods.

As for the effects of efficacy (regardless of its source), our formulation agrees with Gamson (1968) and Olson that as efficacy increases, so does the probability of participation. However, our specification of efficacy in terms of α and β is not exactly the same as either of theirs. Our formulation implies that for *fixed* degrees of efficacy (i.e., $\alpha - \beta = k$), participation increases as the level of α increases if r_1 (i.e., u_1/u_E) is less than $1/k$, but it decreases as α increases if r_1 is greater than $1/k$, and it does not change as a function of α if $r_1 = 1/k$. That is, the effect of efficacy depends not just upon the difference between α and β, but also upon the relation of that distance to the relative worth of the outcomes and upon the value of α itself. If we apply this reasoning to the situations Olson discusses, the likelihood that an individual will contribute to provide a collective-good depends not solely upon efficacy (i.e., $\alpha - \beta = k$) but also on the *level* of α and β; and whether the level will increase or decrease the probability of an individual's contributing depends upon the relationship between k and the ratio of the utility of the collective good to the utility of the necessary contribution.

The relationship between k and r_1 has practical implications which may not be apparent. Imagine the situation of a two-party national election. The members of both parties have some fixed efficacy (probably very small) and some value of α and β. For most voters, it is reasonable to assume that u_1 is considerably greater than u_E (and therefore that r_1 is less than $1/k$). If it should become evident to all that one party is almost certain to win, we would interpret this to mean that members of the soon-to-be-victorious party experience a marked increase in α and β and that members of the other party undergo a marked decrease in α and β. This directly changes the activity rates for both groups, resulting in an increased difference in relative turnout between the two parties. If this increased participation is thought of as a "bandwagon" effect, then it is accompanied by a sort of reverse bandwagon effect (a "sinking ship" reaction). The bandwagon effect implied by our formulation is not dependent upon vote switching; but this is not to deny the possibility that vote switching may also be a function of the sureness of defeat. Moreover, the activation-deactivation effect occurs even if there is no change in the efficacy of the voters (although both effects are limited since all our equations are bounded above and below). By implication, then, campaigns can increase participation not only by convincing people that the issues are important (i.e., by increasing utility values) and by indicating that the issue to be decided is likely to be close (thereby implying that a single vote may be efficacious), but also by simply convincing its proponents that they are almost certain to win and its opponents that they are virtually certain to lose.

Finally, the way we specify the determinants of α and β permits a separation of sheer size effects from a consideration of structural complexity. The complexity of the rules defining the authority structure or the number of structurally different positions in

the organization, for example, can be increased considerably in the organizations we created without increases (beyond position occupancy) of the membership. Conversely, the number of members can be increased without changes in the rules and without substantially increasing structural differentiation. Similarly, multiple contending parties and issues can be given explicit representation in the event trees without entailing major increases in size. In principle, at least, this should enable us to pose more precisely the questions raised by Olson and Michels regarding the effects of organizational size on participation.

APPENDIX

It is not possible for us to display a detailed utility tree for the subjects in our experiments; detailing such a tree for even nine-person organizations is a task which requires a computer. On page 623 we characterized the strategy we use to arrive at the probabilities which are entered into the equations. This appendix reports a detailed example of our strategy for a very simple, three-person organization. In Figure 3, we have drawn a general event tree for a focal member (A) of a three-person organization consisting of A, a president (L) and another non-officer (0).

Figure 3 is drawn for an actor who cannot be influenced, because our focus is upon *when,* not *how,* A will vote. A's preference (again denoted X) does not change. p_O is a participation parameter; it stands for A's expectations about how likely the other member is to participate; v_O denotes A's expectations about how likely O is to vote for X if O participates. One officer, L, is represented, and A expects L to participate always (i.e., $p_L = 1$); v_L designates A's expectations about how likely L is to vote for X. The branches of the tree are drawn in a way which asserts that unexpressed preferences have no effect; for example, if O does not attend, his preference is ignored by A and we delete the v_O branch.

Representing different organizational contexts entails fixing each of the parameters in Figure 3 according to the organizational variable under consideration. Having done that, the rules that specify what constitutes a binding decision for the organization are applied to determine what outcome results from each branch of the tree.

The agreement structure is reflected in the values of v_O and v_L. Any regular pattern of preferences can be represented. As an example, consensus is represented by setting both of those parameters equal to unity and their complements equal to zero. This produces a simple four-branch tree composed of the first, fifth, seventh and eleventh branches of Figure 3.

As we said in the text, the rules which define the authority structure affect the tree in two ways. First, they tell us which branches to include; for example, since L must be present, $p_L = 1$ and since A, O and L are all legitimate voters, branches must be written for the participation and preferences of each. The rules also tell us how to associate an outcome with each branch of the tree. In Figure 3, we have entered the complete outcome distribution for an A who is a member of the decentralized authority structure described earlier, and for an A who is a member of the centralized system just described. In the case of the decentralized organization, we have specified a quorum rule of 2; the centralized organization has no quorum rule. Figure 3 is written as if no one can be influenced. (To express our idea of an influenceable president requires adding one more set of branches and one more parameter representing the likelihood that the president will change his vote when faced with disagreement from one or both of the other members.)

Specifying a particular set of organizational variables, then, indicates a subset of the branches in the general event tree, identifies the values of the parameters (except p_O) and indicates how to write the outcome structure. p_O is fixed by the simulation for each member (see Figure 2 in the text); that is, the history used to brief each subject shows each other member attending according to the percentages in Figure 2.

For the nine-person organizations reported in the text, a computer program is used to calculate the probability of all outcomes under the stipulation that the subject does not come and under the stipulation that he does. We use these calculated numbers as estimates of the subjective likelihood beliefs of the subjects.

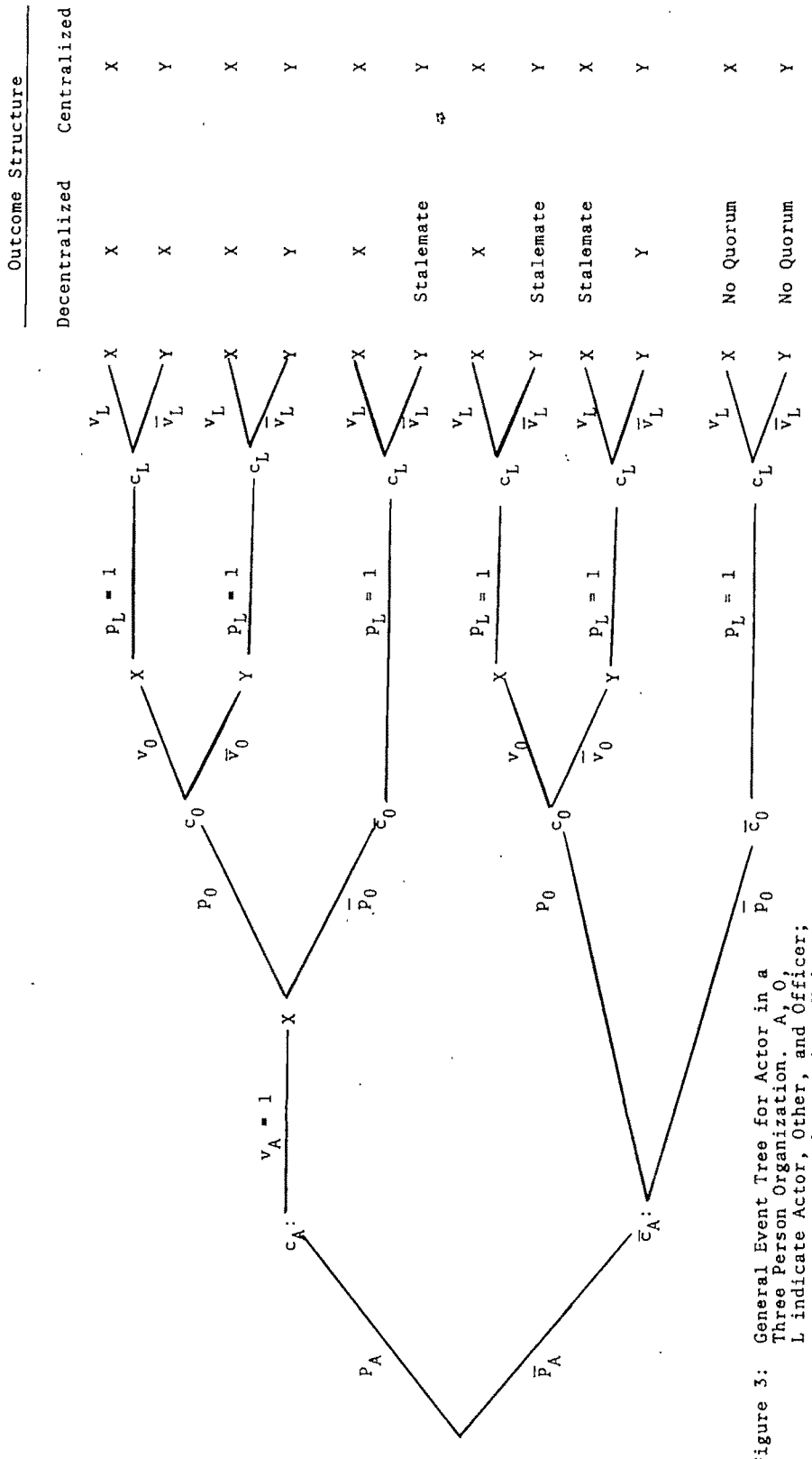

Figure 3: General Event Tree for Actor in a Three Person Organization. A, O, L indicate Actor, Other, and Officer; c means comes to the meeting; X indicates A's preference; p indicates the probability of coming; and v indicates the probability of voting for X.

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OCCUPATIONS, LABOR MARKETS AND THE PROCESS OF WAGE ATTAINMENT*

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A key problem in sociology, as in economics, is explaining why some workers earn more money than others. Sociological models of earnings have stressed the role of a worker's occupation and have tended to ignore the conditions of the labor market in which he finds work. Economic models have stressed labor market functioning at the expense of considering the role of a worker's occupation in determining his wages. In this paper, I attempt to combine sociological models of earnings with (a) economic models of earnings and (b) concepts and findings from the sociology of occupations and professions. I argue theoretically and empirically that some similar conclusions about the processes governing individual earnings attainment can be drawn by examining occupations in terms of labor markets and by analysis of labor markets from the standpoint of occupations. These conclusions are: (a) that labor markets tend to be fragmented along occupational lines, (b) that the processes governing wage attainment vary from one occupation to another and (c) that occupational differences in these processes can be predicted from and explained in terms of the forces which lead to occupational segmentation of labor markets. I discuss some useful implications of my analyses for the study of the relationship between worker age and worker earnings, and I perform some empirical and theoretical analyses of occupational differences in the age-wage relationship. Data are drawn from the U.S. Censuses of 1960 and 1970 and from publications of the U.S. Bureau of Labor Statistics.

A key problem in sociology, as in economics, is explaining why some workers earn more money than others (for recent notable empirical sociological examples, see Duncan, 1969; Jencks, 1972; Hauser and Featherman, 1974; for purely theoretical examples, see Davis and Moore, 1945; Parsons, 1964). If one can generalize at all about economic and sociological approaches to individual earnings, it seems safe to draw two conclusions: (1) sociologists have focused their attention on the role of a worker's occupation in determining his wages, and sociological models of wage attainment have stressed the ways in which an individual's occupation intervenes between his earnings and his education, intelligence, etc.; (2) economists have focused their attention on the workings of the labor market and the interplay between supply and demand for labor in determining individual earnings.[1]

To the sociological mind, the economists' labor market models of earnings are deficient because they usually treat occupations as uninteresting, and therefore ignored, intervening variables. From the sociological perspective, occupations seem to be the key to understanding earnings, because one cannot have earnings unless one has an occupation and, since wage levels vary from one

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[1] In recent years, many economists have focused on an investment model of labor earnings, the Human Capital model, but this trend does not detract from my point that marginal productivity theory (i.e., supply and demand analysis) remains a major economic theory of labor earnings (see Butler, 1961; Friedman, 1962; Mincer, 1970; Rees and Shultz, 1970).

occupation to another, because the forces that determine an individual's occupational attainment also affect his earnings. Furthermore, it is a basic, empirically supported tenet of the sociology of occupations and professions that the conditions of work, abilities, skills and characteristics of persons which determine the success or failure at work and the ideologies of persons regarding work vary from one occupation to another (for some reviews of this literature, see Caplow, 1954; Slocum, 1974; Hall, 1969). Accordingly, ignoring occupations in studying wage dispersion amounts to incorrectly assuming that the determinants of earnings are the same for the incumbents of all occupations.

However, to the economist's mind, the sociological approach to earnings attainment ignores the obvious facts that one can have earnings only if one has a job and individuals find their jobs in the labor market. Neglecting the labor market and the processes which adjust supply and demand for labor amounts to neglecting the very mechanisms by which jobs are secured and wages are determined.

It is, of course, clear that the prevailing viewpoints of economsts and sociologists are both correct: one cannot ignore labor market supply and demand or occupations and the occupational structure if one hopes to build a reasonable theory of why some workers earn more money than others.[2] However, the obviousness of this point seems to have done little to facilitate the marriage of economic and sociological theory and research on individual earnings attainment. Sociological models of earnings generally continue to treat the labor market as either too dull to mention or worse, as nonexistent. And acceptance of economic models of earnings generally continues to make questions about occupations theoretically unimportant and, therefore, too dull to ask.

In this paper, I argue theoretically and empirically that some similar conclusions about the processes governing individual earnings attainment can be drawn by examining occupations in terms of labor markets and by analysis of labor markets from the standpoint of occupations. These conclusions are: (a) that labor markets tend to be fragmented along occupational lines; (b) that the *processes* governing wage attainment vary from one occupation to another; (c) that occupational differences in these processes can be predicted from, and explained in terms of, the forces which lead to occupational segmentation of labor markets. These conclusions are useful for two reasons: first, they are a modest step toward linking two separate theoretic approaches to explaining individual earnings which have substantial communalities and complementarities, but which have developed independently (even in spite) of each other; and second, these conclusions are directly applicable to the problem of extending economic and sociological models of wage attainment to understanding wage differentials between incumbents of the same occupation. As I will point out later, considerable attention recently has been focused on the inadequacy of theories of occupational attainment to account for patterns of individual earnings (Jencks et al., 1972). These recent arguments have made much of the fact that there is tremendous variation in earnings between incumbents of the same occupation. The conclusions which I reach in this paper provide the modest beginnings of a systematic theory of within-occupation earnings differentials.

The plan of this paper is as follows. First, I argue that labor markets tend to be segmented along occupational lines. My argument here is simply that the supply of labor, the demand for labor and social factors such as judgments about the worth of work and patterns of social organization among workers and employers all tend to vary across occupations, and that this variation causes labor markets to be fragmented along occupational lines. Next, I argue that the forces which I identify as leading to occupational segmentation of labor markets also lead to occupational differences in the processes governing wage-setting and earnings. These forces are observable characteristics of occupations, and I give two short examples which demonstrate that empirically testable propositions about occupational differences in wage determination can be systematically derived from consideration of these factors.

[2] This point is neither new nor original, inasmuch as it dates at least to Adam Smith's (1776) *Wealth of Nations*. The curiosity is that this point has been ignored almost since the day that Smith advanced it.

THE OCCUPATIONAL SEGMENTATION OF LABOR MARKETS

Occupational segmentation of the supply of labor

Common experience indicates that labor market competition is segmented along occupational lines to a certain degree: plumbers do not compete for the jobs that electricians get, and neither competes for the jobs taken by stenographers, at least in the short run. The so-called human capital perspective in economics suggests reasons why a rational, utility-maximizing worker would seek work in only one occupation. The human capital theorists argue that workers often make tremendous investments in acquiring skills which they can sell in the labor market.[3] For the moment, assume that occupations requiring larger training investments by their incumbents pay better than occupations requiring smaller training investments. Then, if other things are equal, the higher the personal, occupation-specific training investments which are prerequisites for incumbency in an occupation, the less likely it is that men who have made those investments would seek jobs in any other occupation in which they could find work; any other occupation would offer them lower wages than the one for which they had prepared.[4] Further, if occupations requiring larger amounts of occupation-specific training are more prestigious than occupations requiring less of this training, one would expect that a worker would lose prestige as well as income by taking work in an occupation other than the one for which he had prepared. Thus, one would expect that the financial and social benefits paid to a worker for investing in training specific to an occupation produces an incentive to seek work only in the field for which he has trained, and, as a result, a strain toward segmentation of labor market competition along occupational lines.

The above argument is based on normative expectations of worker behavior. We can bring evidence to bear on the questions of whether or not there are in fact prestige and earnings benefits paid for training investments made by workers in order to gain incumbency in specific occupations. The Department of Labor (1968) has estimated the Specific Vocational Preparation (SVP) in years of training necessary for a worker to attain an "average" level of performance in each of 4,000 jobs.[5] Subsequent work allows conversion of traits of 4,000 jobs into characteristics of Census occupation categories.[6] I note that, in examining the effect of SVP on prestige and earnings, it is desirable to hold constant the effects of the general level of schooling of an occupation's incumbents. Years of schooling and years of specific vocational preparation are positively related to earnings. By examining the effect of SVP on earnings net of the effect of years of schooling, I avoid confounding the direct impact of SVP on earnings with the correlation between the two

[3] These costs are both direct, as in the case of tuition, and indirect, as in the case of earnings foregone while training. These costs are borne by the individual even when his training is obtained on the job, according to Becker (1964). In theory, to the extent that on-the-job-training prepares the worker for increased earnings in firms other than the one giving the training, the individual bears the cost of instruction in the form of reduced wages. The firm bears the cost to the extent that knowledge imparted is useful only in one particular firm and thus does not make the employee who learns it more valuable to other employers who will pay higher wages to that worker.

[4] Gitelman (1968:328) has made the unreasonably severe argument that only under the duress of widespread unemployment would a worker who had invested in his productivity take a job which required less than his investment equipped him to offer, for doing so would lower his income.

[5] These ratings were made by a panel of experts.

[6] The actual merger of job traits with Census occupation codes was performed by the staff of the Institute of Labor and Industrial Realtions of Wayne State University and the University of Michigan. Unweighted averages of SVP scores of the 4,000 jobs were assigned to the corresponding Census occupations.

While the DOL estimates are expert judgments rather than measurements, they are at least indicative of gross differences in specific vocational preparation necessary for a person to become a full-fledged incumbent (i.e., not a trainee) in an occupation. What is more troublesome, though, is the possibility that the raters' judgments of SVP times were contaminated by the prestiges of the occupations they rated. Duncan et al., (1968) present evidence that such contamination occurs in the rating of intelligence required to perform work. However, estimates of training time are probably less subject to such bias because "training time" is more directly observable than "intelligence" and it is unidimensional (which intelligence most certainly is not).

Table 1. Zero-Order Correlations, Means and Standard Deviations for 283 Census Occupational Categories

	M.Ed	Pres	Mean	Std Dev
Median Years of School Completed[a] (M.Ed)	1	.8685	12.04	2.92
Siegel's Prestige Score[b] (Pres)	.8685	1	41.8	16.94
Specific Vocational Preparation (SVP)	.7147	.8141	2.31	2.24

[a]U. S. Bureau of the Census (1963a, Table 1).

[b]Siegel (1971:Ch. 2).

variables which results from the fact that SVP is related to both education and earnings. The same argument applies to examining the relationship between SVP and prestige.

I first examine the relationship between SVP and prestige. Performing the analysis on the 283 detailed Census occupations for which SVP scores, median schooling levels of male incumbents and Siegel's prestige scores are available (sources of these data are given in the notes to Table 1), I obtain the correlations presented in Table 1 and regression equation (1). Symbols are identified in Table 1. Standard errors of coefficients are given in parentheses.

$$\text{Pres} = -8.01 + \underset{(.217)}{3.66}\ \text{M.Ed} + \underset{(.281)}{2.46}\ \text{SVP} \qquad (1)$$
$$R^2 = .81$$

The coefficient of 2.46 for SVP in equation (1) is consistent with my reasoning: the longer the time which workers must invest acquiring SVP to enter an occupation, the higher the prestige of the occupation. So I have some support for the notion that investments in specific vocational preparation offer non-pecuniary returns in the form of prestige and that a worker's time investment in SVP provides an inducement for him to remain in the occupation for which he has prepared.[7]

I now examine the monetary returns to specific vocational preparation by regressing the median earnings of male incumbents of 283 Census occupational categories on the median years of schooling completed by male workers in the occupations and on the SVP required of their incumbents. The results of the regression are provided in equation (2); the occupations included in the calculations are the same as were used in the previous regression analysis. Sources of data not defined earlier are given with Table 2, which presents correlations, means and standard deviations of variables. Standard errors for parameter estimates are given below them in parentheses.

$$\text{M. Ern} = -49.1 + \underset{(105.6)}{391.5}\ \text{M.Ed} + \underset{(137.6)}{412.1}\ \text{SVP} \qquad (2)$$
$$R^2 = .22$$

Equation (2) indicates that, on the average, an individual who has invested in specific training in order to enter an occupation would obtain lower wages in another occupation than he would get in one for which he has special training.[8]

Thus, there is a monetary incentive as well as a prestige incentive for job-seekers to look

[7]Of course, the inducement varies directly with the amount of prestige at stake, which in turn is shown above to vary with the amount of SVP. So we would expect to find lower rates of intragenerational occupational mobility among workers who had made the largest investments in SVP. This pattern is observed as less mobility among professional, technical and kindred workers than among members of other major Census occupation groups (Featherman, 1969: from tables on pp. 184-6; Featherman's sample is not representative of United States males).

[8]The intragenerational occupational mobility sources cited in the previous note are supportive of—or at least consistent with—this point.

Table 2. Zero-Order Correlations, Means and Standard Deviations of Variables in Equation (2)

	M.Ed	SVP	Mean	Std Dev
Median Years of School Completed (M.Ed)	1	.7079	12.04	2.92
Specific Vocational Preparation (SVP)	.7079	1	2.31	2.24
Median Earnings[a] (M.Ern)	.4385	.4228	5138	4023

[a]U. S. Bureau of the Census (1963b, Table 29)

for work in the vocation for which they have special training, and these two incentives can be viewed as providing a strain toward segmentation of competition among workers in the labor market along occupational lines. As I remarked earlier, these pressures are small where amounts of specific vocational preparation are small, indicating that less segmentation of labor market competition probably occurs among incumbents of occupations requiring smaller amounts of SVP than among incumbents of occupations requiring larger quantities of it.

Occupational Differences in the Social Organization of Labor Market Processes

The social organization of wage-setting differs substantially from one occupation to another. In some occupations the operation of supply and demand is mediated through negotiations between large formal organizations such as unions and/or employer organizations. For example, occupations utilized in steelmaking (e.g., puddler, pourer, caster, furnace operator) have long been heavily unionized, and many other occupations (e.g., secondary and primary school teachers) are becoming increasingly unionized. In some heavily unionized occupations, wages, hours of work, advancement and other aspects of wage-setting and work are determined in negotiations between bureaucratized labor monopolies (the unions) and, when employers are especially large firms or have banded together, labor market monopsonies. The nature and structure of worker organizations differ considerably from one occupation to another and, as a result, so does the impact of these organizations on the wage-setting processes affecting their members. For example, physicians do not generally join together to set standard rates for their services. However, there is evidence that medical associations have raised the cost of physician labor by restricting the number of M.D.'s produced by medical schools (Friedman and Kuznets, 1945). That is, medical doctors have used their collective strength to limit the number of physicians; the process of supply and demand does the rest. Another example can be seen in the approach to wage-setting taken by unions representing incumbents of manufacturing operative occupations. These unions have used collective bargaining to influence wage structures. In contrast to operatives and physicians, college instructors have used a loose professional organization, the AAUP, to muster the doubtful force of moral suasion in their quest for higher salaries (Schramm, 1972; 1975). And finally, incumbents of many occupations are not represented by any unions or professional organizations at all. The point being made is that the social organization of an occupation's incumbents impinges on the processes which determine their wages and working conditions, and that unionization is one dimension of this social organization.

There is evidence that the race and sex composition of an occupation's labor force also affects the social organization of wage-setting and work. Hodge and Hodge (1965) have argued that racial and sexual discrimination alters the nature of labor market competition in occupations having significant proportions of blacks and females in their labor forces. The Hodges argue that discrimination places blacks and females in economic circumstances which are substan-

tially weaker than those of comparable white males. As a result, Negroes and women are forced to accept wages lower than those demanded by white males who have similar qualifications. Blacks and women are forced by their more limited financial resources to play the unwitting role of scab labor, thereby lowering the wages of white males in the same occupation. Thus, racial and sexual discrimination alters the nature of labor market competition so that it becomes hostile, undercutting competition between visible groups of workers (white males, black males, women and so on) rather than competition between anonymous individual workers. If the Hodges are to be believed, the prevailing values and attitudes which affect the racial and sexual composition of an occupation's labor force also determine the nature of competition among its incumbents and the process by which this competition affects their wages.

Taeuber et al. (1966) suggest that the Hodges ought not to be believed. But their critique of Hodge and Hodge leads them to stress another aspect of the labor market which also leads to occupational differences in wage setting. This aspect is social judgments of the prestige of each occupation and/or its incumbents. Taeuber et al. argue that common judgments of the prestige of an occupation partially determine the price paid for the labor of its incumbents. Presumably the wage effects of these judgments operate in addition to the machinations of supply and demand in setting pay rates.[9] Reder (1955) draws similar conclusions: he argues that social judgments about fair pay impinge on the operation of supply and demand in setting the wage level of an occupation. These judgments (and their effects on wages) would seem to vary across occupations, as is reflected in the fact that minimum wage legislation has yet to be extended to cover all occupations. Similarly, Siegel (1971:Ch.IV) argues that the prestige of an occupation partially determines the hourly pay which it affords its incumbents. Siegel's argument is supported by an elaborate model of the American occupational structure. The point to be culled from these studies is that there is rather widespread support for the hypothesis that prevailing social judgments about an occupation affect the processes which set the wages of its incumbents. Because these judgments vary from one occupation to another,[10] the processes governing wage determination also vary across occupations.

Occupational Segmentation of Demand for Labor

"Occupations are collections of tasks which are differentiated primarily in the skill—the training and talent—necessary for their performance" (Siegel, 1971:8). So the attributes that make a person valuable to an employer as an employee in one occupation may have no bearing whatsoever on that person's value as an employee in another occupation. For example, a knowledge of bookkeeping procedures and the ability to use a calculating machine may be desirable attributes for accountants, but they are absolutely irrelevant characteristics for material handlers. Formal schooling may be more important for managers and officials than for welders. Candidates for a job as welder who have, say, four years of college would not be more desirable to employers than those with a high school diploma, other things equal. But candidates for a job as a company official would probably be substantially more attractive with a college degree than with only a high school diploma, *ceteris paribus*. Support for this reasoning can be seen in two empirical studies of intraoccupational wage dispersion by Thurow (1967) and Rees and Shultz (1970). Using the multiplicative model of income given in equation (3), Thurow found substantially different values for the expo-

[9] However, the Hodges have suggested that in spite of the weaknesses of their analyses, their argument is still stronger than Taeuber et al.'s criticism of it. There is probably some measure of truth in both the Hodges' "competition hypothesis" and Taeuber et al.'s alternative to it. Since both the competition hypothesis and the alternative are consistent with the general point I am raising—that socially-determined factors which vary substantially from one occupation to another affect the wage determination process—it does not seem worthwhile to do a major exegesis of the debate between Taeuber et al. and the Hodges. (For a more lengthy discussion of the Hodges-Taeuber et al. controversy, see Stolzenberg, 1973.)

[10] For some classic remarks on this subject see Weber (1964:250-4).

nents of education and experience in each of nine major occupation groups within race-region classes. Thurow used data from the 1960 U.S. Census of Population.

$$\text{Yearly Income} = a(\text{Years of Education})^{b_1} \text{Years in the Labor Force})^{b_2} \quad (3)$$

Using a linear, additive model of hourly wage rates of workers in the Chicago area, Rees and Shultz (1970:159) found different partial regression coefficients for years of schooling completed for the following occupations: typist, keypunch operator, accountant, tab machine operator, material handler, janitor, janitress, fork lift truck operator, punch press operator, truck driver, maintenance electrician and tool and die maker.

Additional insight into occupational differences in the wage effects of demand for labor can be gained from empirical examination of a subject usually left to economists: interindustry wage effects. Demand for labor is created by demand for the goods or services which it produces. The set of firms which produces a given good or service is called an industry—e.g., the advertising industry or the automobile industry—and economists have argued for many years that characteristics of the market for an industry's product affect the wages paid to persons employed in creating and selling that product (see, for examples, Dunlop, 1948; Garbarino, 1950; Bernstein, 1954:28-9; Butler, 1961). To the best of my knowledge, students of interindustry wage effects have not entertained the hypothesis that these effects are not the same for the incumbents of all occupations which are employed in an industry's labor force. That is, past studies have not investigated the possibility that industries which pay incumbents of some occupations higher-than-average wages do not also pay incumbents of other occupations above-average wages. My argument that labor markets are fragmented along occupational lines would be strengthened if it could be shown that industry wage effects differ from one occupation to another, since doing so would be consistent with the hypothesis that the wage effects of an industry's demand for labor operate differently on the wage determination processes of different occupations. The analysis presented in this section is designed to shed some empirical light on two questions which have not been answered by previous studies of industrial pay differentials: (1) are industry wage effects substantial *net of the effects of workers' education, experience, occupation and geographic location?* and, if the answer to this question is "yes," (2) is the wage effect of employment in a given industry the same for incumbents of all occupations employed by that industry, or do industrial wage effects vary across occupations? In keeping with the general thrust of this paper, I will show that industrial wage effects differ substantially across occupations. I also find that these effects are substantial net of the worker characteristics mentioned above.

In order to measure the wage effect of employment in various industries, I estimate the following model for the male employed, experienced civilian labor force (ECLF). I use data from the 1960 U.S. Census of Population 0.1 percent Public Use Sample.

$$\begin{aligned}\$ = {} & a + b_1 Occ + b_2 S + b_3 W + b_4 SMSA \\ & + b_5 Self + b_6 Ex + b_7 Ex^2 + b_8 Ed \\ & + b_9 Ed^2 + b_{10} EdEx + b_{11} EdEx^2 \\ & + b_{12} Ed^2 Ex + I_j.\end{aligned} \quad (4)$$

The symbol $ is the log implied hourly earnings of the individual. It is computed by a method based on Fuchs' (1967) procedure. The method is straightforward: the individual's earnings last year (1959) are divided by the number of weeks he worked during the year, yielding his average weekly earnings. The number of hours he worked during the Census week is then taken as an estimate of the number of hours he worked in a typical week that year and is divided into his average weekly earnings to obtain an estimate of his average hourly earnings during the previous year. The weakest step in this procedure appears to be the assumption that the number of hours worked during the Census week is typical of the number of hours worked per week throughout 1959. Further, because dollars earned per week is divided by the number of hours worked during the Census week, a usual assumption of least squares regression analysis becomes absolutely untenable: namely, the assumption that errors in measurement of the dependent variable (hourly wage) constitute an additive component of the variable's total variance. A solution to the nonadditive error component problem is provided by using the logarithm of

hourly earnings as the dependent variable in the analysis, rather than the actual hourly wage. The logarithm of the hourly wage measure is precisely equal to the logarithm of earnings in 1959 minus the logarithm of the number of weeks worked that year minus the logarithm of the number of hours worked during the Census week, which clearly indicates that the error components of the log annual earnings, log weeks worked last year, and log hours worked per week variables are all additive components of the error variance of the weekly earnings variable.[11] Thus $ (log implied hourly earnings) appears to be an adequate dependent variable to use for present purposes.

Occ is the average implied hourly income of employed male workers in the individual's occupation. Occ is included in the model to hold constant the wage effect of employment in the individual's occupation. Following Fuchs (1967), Occ is computed by summing the annual income of all employed incumbents in a given occupation in 1959, dividing this total income by the total number of weeks worked by incumbents during that year, and then dividing the quotient by the total number of hours worked by employed incumbents of the occupation during the Census week in 1960. If the number of hours worked during the Census week is an accurate indicator of the number of hours worked during an average week during the previous year, Occ provides a measure of the average income obtained per hour of work by incumbents of the occupation.[12]

S is a dummy variable for Southern residence, W is a dummy variable for residence in the West and SMSA is a dummy variable for employment in a standard metropolitan statistical area. These variables are included in the equation to hold constant the effects of geographic differences in money (as opposed to real) wages (see Block, 1948; Kerr, 1957; Galloway, 1963; Folger and Nam, 1967; Reder, 1957).

Self is a dummy variable for self employment.

Ed is the individual's number of years of schooling completed.

Ex is the individual's potential years of experience in the labor force.

Ex equals the individual's age minus his years of schooling completed, minus 6. The terms Ex^2, Ed^2, EdEx, $EdEx^2$ and Ed^2Ex are included in the wage model to allow for nonlinearities and nonadditives in the effects of schooling and length of labor force experience. These nonlinearities and nonadditives are not important for present purposes and are discussed in a variety of contexts and analyzed at length by Thurow (1967) and Stolzenberg (1973; 1974; 1975).

I_j is the wage effect of employment in the jth industry category, the industry in which the individual currently holds a job. The present analysis was done using a 70-category industry classification.[13] This classification is

[11] A second reason for using log hourly earnings rather than hourly earnings is that the distribution of log hourly earnings is probably more similar to the normal distribution than the distribution of hourly earnings, and is therefore probably more suitable for use in least squares regression analysis than hourly earnings. Wages and salaries have been found to follow the log normal distribution more closely than the normal distribution in both Great Britain (Hill, 1959) and the United States (Cramer, 1971:68). We plotted the log earnings distribution of males in the U.S. ECLF in 1959 on normal probability graph paper and observed that over about 65 percent of the distribution, the plotted points conformed to the straight line pattern which would be expected if earnings were log normally distributed. Although these results suggest that the distribution of hourly earnings of workers within detailed occupational categories are log normally distributed, they are of course neither strong nor conclusive evidence.

[12] We are, of course, interested in the average hourly earnings (not income) of workers, but it is of no account that we have used a measure of hourly income in these calculations: mean earnings of the employed male labor forces of Census detailed occupations correlate .97 with the mean incomes of employed males in these occupations. So for practical purposes, the two variables can be regarded as linear transformations of each other and therefore equally good for present purposes. (Calculated on the basis of mean income and mean earnings data for 68 detailed occupations for which both earnings and income means were published. Sources: U.S. Bureau of the Census, 1963a; 1963b.)

[13] This model produces the same results that would be obtained if 69 dummy variables representing employment in all but one of the 70 industry categories were included in a regression equation with the other variables, and if the dummy coefficient for each industry were added to the constant term to obtain the wage effect of the industry. The reasons that the dummy variable procedure was not used are as follows: first, several experiments using similar data to estimate very large

Table 3. Percent of Variance of Industry Variables Which Is Explained by 70-Category Industry Classification[f]

Variable	Data Sources	Unweighted[c]	Weighted[d]	N[e]
Depreciable assets per production worker	I	98.3%	98.6%	93
Net income per production worker	I, II	89.1	93.8	93
Percent unionization[b]	III	90.4	96.7	116
Percent unemployment, 1960	IV	99.5	99.9	149
Four-firm concentration ratio[b]	V	88.3	93.6	59
Percent of employees who are black males	VI	72.2	85.3	149
Average market power factor, 1958 - 1961[a,b]	I	84.7	93.7	119
Percent of firms with net income of one dollar or more	I	84.3	86.3	119
Average total assets per firm, 1953 - 1966	I	87.3	94.4	119

Notes:

[a]Market power factor is equal to the percentage of total business receipts in an industry accounted for by sales of firms in the industry having assets of $100 million or more. The market power factor of an industry measures the extent to which its sales are dominated by very large firms; it is a measure of concentration.

[b]Data provided by Barry Bluestone of the Institute of Labor and Industrial Relations, Wayne State University and The University of Michigan.

[c]Industries not weighted in computations.

[d]Industries weighted by their employment in 1960.

[e]N is the number of industries in the 149-category Census Classification for which data are available.

[f]Percentages of variance explained are computed by squaring the intra-class correlation coefficient of a one-way analysis of variance of the variable on the 70-category Industry Classification.

Sources:
I. U. S. Treasury Department, Internal Revenue Service (1963)
II. U. S. Department of Labor, Bureau of Labor Statistics (1968)
III. Adapted from V. Fuchs (1968), Appendix I, Table I-2.
IV. U. S. Bureau of the Census (1963a), Tables 210 and 211.
V. L. Weiss (1966), Table A-2.
VI. U. S. Bureau of the Census (1963b).

regression equations indicated that severe rounding errors would occur in the course of computations; second, the expense of obtaining the correlations to estimate the dummy variable equation parameters proved to be prohibitive. The I_j's were obtained by computing the regression of $ on the other variables and then computing the constant term for the equation using the means of $ and the regressors for members of the sample in each industry category.

a condensation of the 149-category U.S. Census 1960 Industrial classification. As Tables 3 and 4 both indicate, this 70-category classification does a remarkably good job of retaining the variance of key industrial properties across the full Census classification.

Equation (4) is estimated over the entire civilian employed male labor force as well as

Table 4. Comparison of Variance of Selected Attributes of the Male Employed Labor Force Which Is Explained by Two Industry Classifications, 1959 and 1960[a]

Attribute	Variance Explained		Percent of Variance Explained by Census Classification Which Is Explained by 70-Category Classification
	By Census Classification	By 70-Category Classification	
Hours worked per week[b]	16.9%	16.2%	95.8%
Hours worked per year	4.9	4.2	85.4
Yearly earnings	10.9	9.9	90.8
Average earnings per week	9.0	8.1	90.4
Average earnings per hour[c]	5.1	4.5	88.4
Log yearly earnings[d]	17.2	16.0	93.1

Source: Computed from U. S. Census of Population, 1960, Public Use 0.1 Percent Sample.

Notes: [a]Only males who were employed in 1959 are included in these computations.

[b]Hours worked during the census week, 1960.

[c]Persons who did not work during census week excluded.

[d]Males without earnings of one dollar or more excluded.

within each of four major occupational groups. Estimates of the first 12 terms of equation (4) are not important for present purposes, and are given in Stolzenberg (1973; 1975). Table 5 presents the R-squared statistics for equation (4), and for equation (4) with the industry terms, the I_j's, removed.[14]

Looking at Table 5, notice that the industry terms account for from 18 to 42 percent of the variance of $ which is explained by equation (4), depending on the major occupation category over which the equation is estimated. Over the total employed male labor force, the industry terms account for slightly less than a third (30 percent) of the variance of $ explained by the equation.

We can use these R-squared values to perform a formal test of the hypothesis that industry wage effects are indistinguishable from zero. The compound null hypothesis

$$H_0 : I_1 = I_2 = I_3 = \ldots = I_{70} = 0$$

can be rejected if the statistic

$$f = \frac{R_1^2 - R_2^2}{1 - R_1^2} \cdot \frac{N\text{-}L\text{-}K\text{-}1}{K}$$

is large (see Johnston, 1972:146-7). f is distributed as F with K degrees of freedom in the numerator and N-L-K-1 degrees of freedom in the denominator. R_2^2 is the R-squared for the equation with only the first 12 (=L) terms included. R_1^2 is the R-squared for the equation when 69 (=K) additional terms "capturing" the effects of the 70 industry categories are added. N is the number of cases over which the regression is estimated. For each major occupational group and for the total sample, the null hypothesis that the I_j's are all equal to zero can be rejected ($p \leq .01$). Inclusion of the industry terms makes a substantial contribution to the explanatory power of equation (4).

Thus, I have answered my first question about industry wage effects—they are sizeable.

[14] See Table 5 for the computational procedure used to obtain the R-squared statistic.

Table 5. F Tests to Determine Whether Industry Wage Effects Differ Significantly from Zero

Major Occupation Group[a]	N[b]	R^2 without Industry Terms	R^2 with Industry Terms[c]	% of R^2 Attributable to Industry Terms[d]	F
Total male employed labor force	37258	.216	.307	30%	69.74
Professional, technical and kindred; managers, officials and proprietors	8841	.182	.271	33	15.27
Craftsmen, foremen and kindred; operatives and kindred	5812	.108	.186	42	7.84
Clerical and kindred; sales workers	15381	.200	.273	27	20.86
Laborers; service workers	6224	.181	.221	18	4.50

Source: Computed from U. S. Bureau of the Census, 1960 Census of Population, 0.1 Percent Public Use Sample.

Notes: [a]Major occupation groups used in these computations are combinations of U. S. Bureau of the Census major occupation categories.

[b]Individuals who did not report an occupation are excluded from these computations.

[c]The industry terms cannot be determined from this table. The "R^2 with industry terms" is computed by calculating the value of $\hat{\$}$ for each case in the sample, and then dividing the variance of $\hat{\$}$ by the variance of $. In fact, this quotient is not a "true" R^2 but it is equal to the proportion of the variance in $ which is "explained" by the regression of $ on all the variables in equation (4), industry terms included. The "R^2 without industry terms" is the R^2 of the regression of $ on the first 12 variables in equation (4). All other parameters are invariant with respect to inclusion of the industry terms.

[d](R^2 with industry terms - R^2 without industry terms)/R^2 with industry terms.

Table 6 sheds some light on the second question I raised about industry wage effects. Do they vary across occupations? The upper triangle of the matrix in Table 6 presents Pearsonian correlation coefficients among the industrial wage differential scores computed over the entire male employed labor force and by major occupation groups.

Looking first at the top row of the matrix, note that the correlation between the industry effects for the sample as a whole and the effects for major occupation groups are lower than would be expected under the assumption of equal wage effects in all occupations. (Squaring these correlations, we are able to see that the industry wage effects for the total occupational distribution "explain" at most only 64 percent of the variance in the effects computed for the occupational subcategories—in the case of Clerical and Kindred and Sales Workers—and as little as 28 percent—in the case of Laborers and Service Workers.) Correlations between subcategory industry effects are even smaller than the correlations

Table 6. Measures of Association between Industry Wage Effects for Total Employed Male ECLF and for Major Occupational Groups

	Pearsonian correlation coefficients are given in upper triangle of matrix; values of Kendall's Tau are shown in cells of lower triangle				
	1.	2.	3.	4.	5.
1. Total male employed ECLF		.68	.71	.80	.53
2. Professional, technical and kindred; managers, officials and proprietors	.43		.55	.37	.32
3. Craftsmen, foremen and kindred; operatives and kindred	.55	.35		.51	.44
4. Clerical and kindred; sales workers	.66	.20	.34		.32
5. Laborers; service workers	.47	.20	.33	.36	

Source: Computations based on 1960 U. S. Census of Population 0.1 percent sample.

Note: This analysis is based on wage effects for 63 industry categories; seven categories for which there were insufficient data to calculate wage effects in at least one major occupation group were deleted from the analysis.

between the subcategory effects and the total sample effects. To test the hypothesis that the industry wage effects in various occupations are transformations of each other (but nonlinear transformations), I have computed Kendall's Tau as a measure of rank correlation among the wage effects in the major occupation groups and in the sample as a whole. These rank correlation coefficients are presented in the lower half of the matrix, Table 6. Note that the rank correlations range in value from .66 to .20 with an average of .39. Once again, these correlations are lower than one would expect if the industry wage effects were identical across occupations, approximately equal, or even merely similar enough to provide common rankings of industries according to the size of their wage effects.[15] So the evidence indicates that one cannot accept the hypotheses that industry wage effects are approximately equal across the occupational structure. Apparently, industries which pay above-average wages to professional and technical workers do not necessarily pay higher-than-average wages to laborers, for example.

In trying to make sense out of the fact that data on Chicago area firms and workers did not support the hypothesis of consistent company wage effects across occupation categories, Rees and Shultz (1970:46-8) concluded:

> Although we do discover some clusters of occupations in which wages are significantly related, we do not find many firms that are consistently high wage or low wage firms across the whole spectrum of occupations. It appears that a firm's position in the wage hierarchy of a labor market is not a simple thing to summarize, contrary to the impressions given in some of the previous literature that has examined narrower ranges of occupations.

Industries are just aggregations of firms which produce similar goods or services (and are therefore subject to similar needs for labor, to the constraints of similar technologies and to similar conditions in the market for their

[15] Scatterplots were also drawn and inspected. No strong nonlinear patterns of association were apparent.

product). So our findings of different industry wage effects in different major occupation groups fit rather well with the only other study we are aware of which brings data on more than just two or three occupations to bear on the question of whether or not industrial wage effects vary from one occupation to another. We might also note that Rees and Shultz came to the above conclusions after correlating establishment means of wage per hour of work for 12 detailed occupation categories. They found that only 11 out of the 66 possible correlations were significantly larger than zero at a 99 percent level of confidence, and that only 16 were significantly larger than zero at a 95 percent level of confidence. Thus, it seems unlikely that there is much consistency of industry wage effects across detailed occupations *even within each of the four broad major occupational groupings that I used in my calculations of industry wage effects.*

Thus, like other aspects of wage determination, industrial wage effects differ substantially across occupations.

SOME EXAMPLES OF APPLICATIONS

In the last section of this paper, I investigate factors which lead to fragmentation of wage-setting processes along occupational lines. The purpose of this section is to demonstrate that this reasoning can be fruitfully applied to the concerns of sociologists who study occupations and/or the process of socioeconomic achievement. Since it is impossible to prove deductively that a line of inquiry or a theoretical perspective will be fruitful, my arguments here must rest on my ability to provide a convincing example of the usefulness of the perspective I advocate. Limitations on the length of this paper make it impossible to give an extensive illustration, so I will have to base my case on two brief analyses. The subject of these analyses is the relationship between a worker's age and his earnings. I have chosen to examine occupational differences in the age-wage relationship because there has been and continues to be considerable sociological interest in understanding career patterns (e.g., Lipset and Malm, 1955; Blau and Duncan, 1967; Duncan et al., 1972). Further, since years of age (at least for men) is highly correlated with years of labor force experience, findings which are useful for sociological analysis of life cycle changes in earnings can be translated directly into terms which are useful to economists who are interested in subjects such as the acquisition and depreciation of human capital, on-the-job training and other related topics (see Mincer, 1971; Mincer and Polachek, 1974; Becker, 1964; Doeringer and Piore, 1966). And, finally, I have chosen to examine occupational differences in the age-wage relationship because this relationship fits well with the traditional concern of the sociology of occupations and professions with career patterns (see, e.g., Slocum's 1974 review of this literature).

In the following section, I argue that the relationship between a worker's age and his earnings depends upon the occupation in which he is incumbent. I will try to show that these differences can be at least partially explained in terms of factors which I have already identified as aspects of the segmentation of labor markets along occupational lines. I will look at two of these aspects, the nature of the tasks performed by incumbents of an occupation and the social organization of the incumbents themselves. More specifically, I will examine the connection between the physical demands of an occupation's tasks and the age-wage relationship, and the impact of unionization upon the age-earnings relationship.

Physical Demands of Work and Age-Wage Relationship

As a man ages, his earnings are driven *upward* by at least two forces: on-the-job learning which increases his productivity and the accumulation of seniority. But as a man ages, his earnings also tend to be pulled *downward* by at least two other forces: first, the decline of his health and physical strength and second, the increasing obsolescence of the job-related skills and knowledge which he acquired earlier in his life, in school, on the job or elsewhere. These forces do not affect men's earnings with equal strength throughout their lifetimes. Health and strength decline with increasing speed as men age: Figure 1 indicates that health disability days per person per year not only increase with age, but that the *rate* at which they increase with age also increases as individuals grow older. That is, the slope of the line relating age to disability

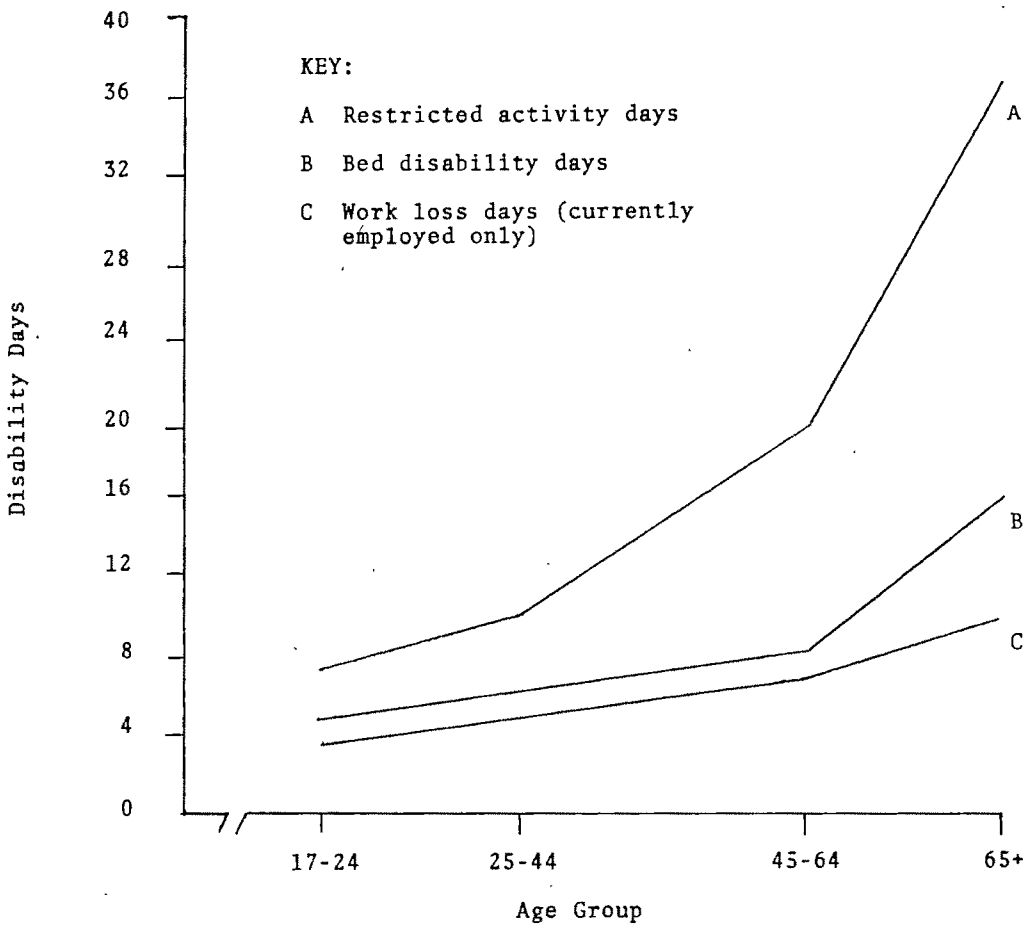

Figure 1. Number of Disability Days per Person per Year, by Type of Disability, for Males, July, 1962, through June, 1963 (U.S. Public Health Service, 1964:Table 13)

days per year is steeper for older persons than for younger persons.

Similarly, forces causing a positive association between men's age and earnings tend to decline in impact as workers grow older. For example, seniority is accumulated as workers grow older; but wage benefits of additional amounts of seniority tend to decline as the total amount of seniority accumulated increases (see Rees and Shultz, 1970). Also, one might speculate that workers acquire important on-the-job learning at a faster rate earlier in their careers than later, simply because they have more to learn at the beginning.

So, generally speaking, forces which cause a positive correlation between worker's age and earnings tend to be less powerful for older men than for younger males. And forces which cause a negative correlation between age and earnings tend to become more powerful for older males than for younger men. As a result, the relationship between workers' ages and their earnings tends to be positive for young men and negative for older men, leading to the well-known convex age-earnings profiles shown in Figure 2.

It seems worth pointing out that while the age-earnings profiles shown in Figure 2 have been used to make plausible inferences about earnings changes over the course of men's careers (see Becker, 1964; Miller, 1960; and Mincer, 1971, for studies which use such data for this purpose), I am not presently concerned with careers per se. Careers are patterns of earnings, occupations, jobs, etc. over the course of individual life cycles. To properly study careers, one needs time-series

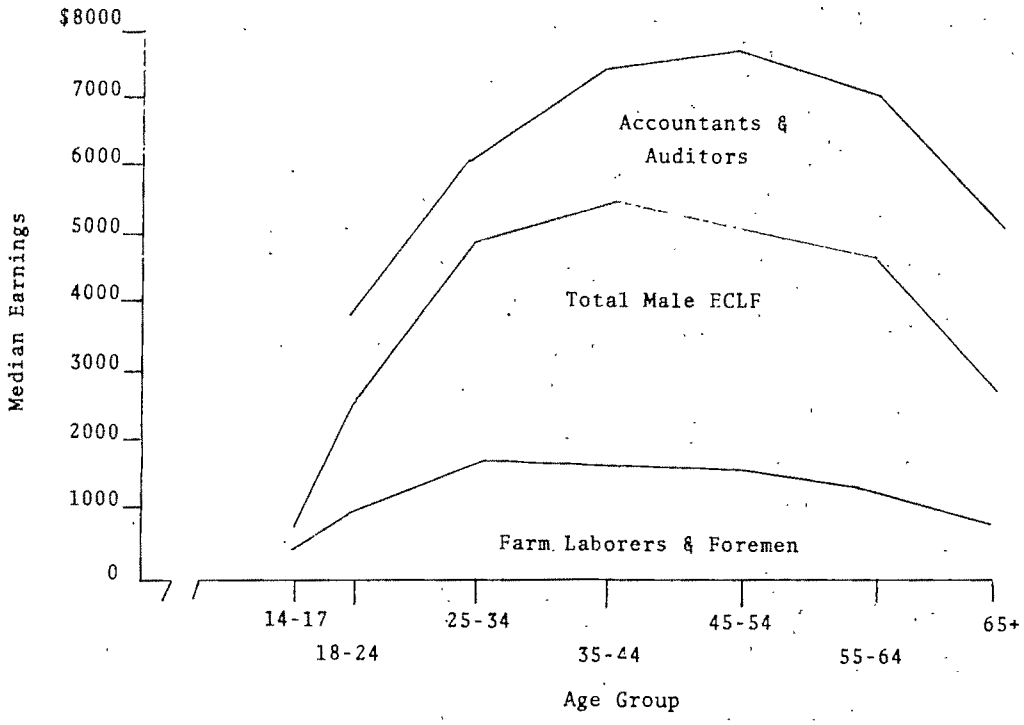

Figure 2. Median Earnings of Males, by Age, for the Experienced Civilian Labor Force and Selected Occupations, 1959 (U.S. Bureau of the Census, 1963a)

data on employment, earnings, etc. of individuals. My present concern is not with the different wages earned and jobs held by men as they grow older, but with age-related factors which make the labor of different aged men, all working at the same time, worth different amounts of money. Careers typically involve incumbency in more than one occupation (see Blau and Duncan, 1967), and an individual may reach his peak earnings either by moving into higher and higher paying jobs in a single occupation, or by moving into higher and higher paying occupations, or by some combination of both types of moves. A good theory of occupational differences in working conditions, wages, etc. could be combined with a good theory of occupational mobility to provide a theory of careers, but doing so is not the present concern. Some attention will be given to relating theories of careers and theories of occupations later in this paper, and I now return to consideration of age-earnings profiles.

My reasoning thus far about factors affecting the relationship between worker age and earnings is, obviously, a set of generalizations which can be applied crudely to employment in all occupations, or even to the subject of employment in general, without considering occupations. However, the thrust of this section, like the thrust of this entire paper, is that these generalizations apply differentially to incumbents of different occupations, and that their differential application can be predicted, or at least somewhat accounted for, by certain key characteristics of the occupations and their labor markets. Two key characteristics which I identified earlier are the nature of the tasks performed by an occupation's incumbents and the social organization of the labor market in which their wages are set. I will first discuss ways in which the nature of an occupation's tasks affects the age-wage relationship of its incumbents and then will turn my attention to consideration of ways in which the social structure of an occupation's labor market affects this relationship.

One would expect that in occupations requiring large amounts of physical exertion, younger men would tend to be the most

productive workers and would therefore get the best-paying jobs. In occupations requiring little or no physical exertion, younger workers would have no special abilities to offer (other things being equal, of course) and the age-earnings profile would tend to peak later than in occupations having heavy physical demands. For example, compare the age-earnings profiles of accountants and farm laborers in Figure 2. Notice that while accountants reach peak earnings in the age-range of 45 to 54 years, farm laborers peak at 25 to 34 years. These anecdotal examples are intended to be merely illustrative; some statistical analysis provides a more systematic test of my reasoning. For each of 105 Census intermediate occupational categories, I used Lagrangian interpolation to calculate the single year of age at which the male age-median earnings profile reached its maximum in 1959 (U.S. Bureau of the Census, 1963a). For estimates of the physical demands of these occupations, I turned to the U.S. Bureau of Labor Statistics estimated physical demands of 4,000 jobs listed in its Dictionary of Occupational Titles (U.S. Department of Labor, 1968). These DOT job characteristics were combined to obtain estimates of the physical demands of Census intermediate occupation categories.[16] Census intermediate occupation categories are rather broad, and the Department of Labor physical demand ratings are more vague than one would like, but these data do allow a crude test of my reasoning. If I am correct, there will be a negative correlation between the year at which occupational age-earnings profiles peak and the physical demands made by the tasks required of their incumbents. The zero-order correlation between the age of peak earnings and the measure of physical demands is −.45, which is consistent with my reasoning.

Unfortunately, there is a possible alternative explanation of the correlation between physical demands and year of peak earnings: occupations making heavy physical demands on their incumbents also tend to be occupations whose incumbents have relatively little schooling (the correlation between the physical demands of these 105 occupations and the median years of schooling completed by the male incumbents is −.62). Since men typically enter the labor force when they complete their schooling (see Blau and Duncan, 1967), incumbents of physically demanding occupations may reach peak earnings earlier simply because they start work younger than incumbents of less physically demanding occupations. To determine whether or not the "different starting times" hypothesis could account for the correlation between an occupation's age of peak earnings and its physical demands, I subtracted the median years of schooling completed by incumbents of each occupational category from the age at which the age-median earnings profile of male incumbents of the category peaks. I then correlated the difference obtained with the Department of Labor's physical demands score. The resulting correlation is −.20. Though modest in size, this correlation is significantly different from zero ($p \leq .05$) and has the predicted negative sign. Thus, the data support the hypothesis that the relationship between a worker's age and his earnings depends upon the occupation in which he is incumbent, and that this relationship is itself at least partly explainable by the nature of the tasks whose performance defines the occupation.

One might suggest other relationships between an occupation's age-earnings profile and the nature of its tasks. For example, one might hypothesize that the age of peak earnings is partially determined by the complexity of the tasks performed. That is, one might hypothesize that it takes relatively long periods of time to learn to perform complex tasks with maximum efficiency, and that therefore maximum earnings are reached only after relatively long periods of incumbency in an occupation. Operationalization of task complexity presents certain methodological problems, and present purposes do not seem to require construction of a very complete model of occupational differences in age-earnings profiles. It suffices to say that I have provided evidence that one characteristic of an occupation's work (physical demands) is a significant determinant of the age-earnings

[16] The DOT physical demand ratings fall on a five-point scale: 1 = sedentary; 2 = light; 3 = medium; 4 = heavy; 5 = very heavy. The actual coding of DOT job characteristics into Census occupation characteristics was done by the staff of the Institute for Labor and Industrial Relations at the University of Michigan under the supervision of Barry Bluestone.

relationship of its incumbents, and that other characteristics of occupations' tasks would appear to affect the extent to which youth or maturity tends to be a qualification for obtaining the best-paying jobs in the occupation.

Social Organization of Labor Markets and the Age-Wage Relationship

A second characteristic of occupations which I hypothesized to affect their age-earnings profiles is the social organization of the labor market in which their incumbents' wages are set. Like the nature of an occupation's tasks, the social organization of its labor market is conceptually multifaceted, and data on each different dimension of labor market social organization are difficult to obtain. Because at least some data are available on occupational differences in unionization, I will confine my discussion to the effects of unionization on an occupation's age-wage relationship.

Unionization is a particularly important and interesting aspect of labor market social organization for at least three reasons which bear on wage determination. First, unionization changes the wage-setting process from negotiations between individual workers and individual employers (or between individual workers and bureaucratized employers) to negotiations between two or more formal organizations, the employee union(s) and the employer organization(s). Thus unionization makes questions about bureaucratic structure and bureaucratic effectiveness relevant to understanding the wage-setting process. Second, unionization changes the labor market from anything vaguely resembling the normative economic ideal of "perfect" competition into the machinations of monopolies (the unions) and frequently, monopsonies (the purchasers of labor, the employers). And third, because American unions have traditionally sought job security and economic protection for their middle-aged and old-aged members as key bargaining goals, there is reason to suspect that the age-wage relationship is different in heavily unionized occupations than in occupations which are not heavily unionized. I will now devote some attention to the relationship between unionization and age-earning profiles. In particular, I hypothesize that the age-earnings profiles of incumbents of more heavily unionized occupations decline less after they peak than the age-earnings profiles of incumbents of less-heavily unionized occupations.

There is a scarcity of data on the unionization rates of occupations. (Abundant data exist on unionization by industry; see Lewis, 1963.) As a result, correlation or regression analysis over the entire surface of the U.S. occupational distribution is not possible here. However, some test of my reasoning about unionization effects is made possible by comparing four occupations which differ substantially in the extent to which their incumbents are unionized, but which would seem to be similar otherwise. These four occupations form the universe of 1960 U.S. Census detailed occupation categories representing operators of large motor vehicles. The categories are Locomotive engineer, Locomotive fireman, Truck driver and deliveryman, and Bus driver. Locomotive engineers and locomotive firemen are among the most heavily unionized workers in the contemporary United States. The U.S. Department of Labor has estimated that all locomotive engineers and firemen working for railroads are union members (personal communication, 7/74). And in 1970, a Department of Labor survey found that 79 percent of *all* railway and railway express industry employees (including management, office staff and all other workers) were union members (U.S. Department of Labor, 1972). That same survey found a unionization rate of about 40 percent for drivers and deliverymen. Thus, while data do not allow precise estimates of unionization for these occupations, it seems safe to say that the occupations Locomotive engineer and Locomotive fireman have much higher unionization rates than the occupations Bus driver and Truck driver and deliveryman. It also seems fair to say that these occupations are similar; that is, that the tasks they involve require roughly equivalent abilities of the persons who perform them and that the tasks are roughly comparable (though the machines upon which the tasks are performed are quite obviously different). Notice in Figure 3 that the age-median earnings profiles for the occupations Bus driver and Truck driver and deliveryman fall more sharply after they peak than the profiles for Locomotive engineer and Locomotive fireman. Indeed, the Locomotive engineer profile is monotonically increasing

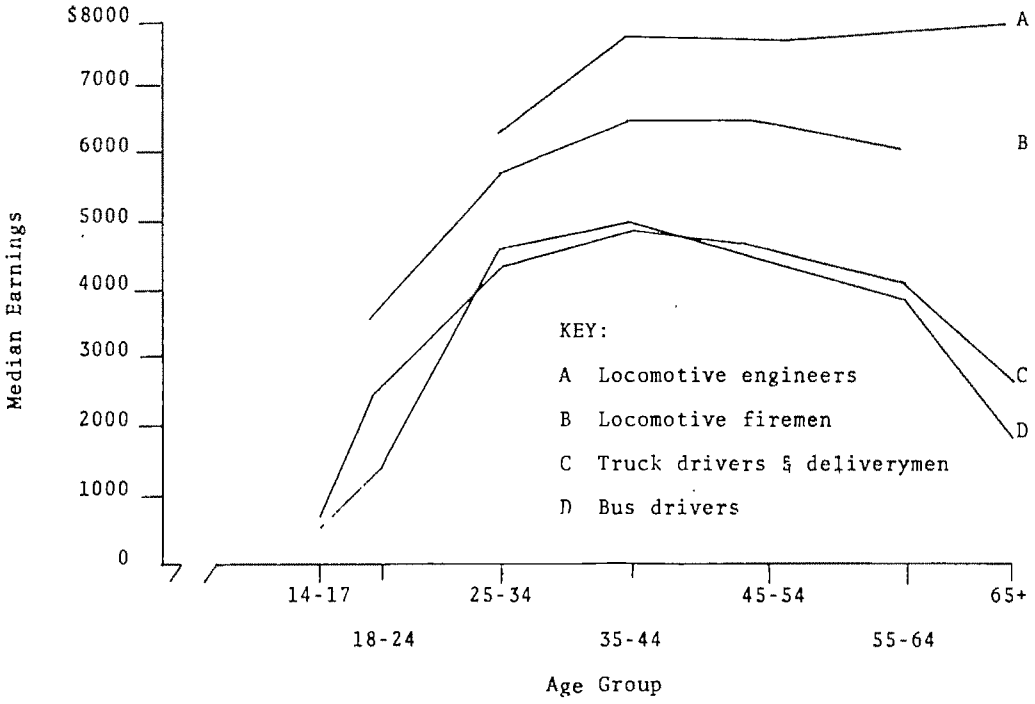

Figure 3. Median Earnings of Males, by Age, by Occupation, for Operators of Large Motor Vehicles, 1959 (U.S. Bureau of the Census, 1963a)

over its entire range. (Similar patterns are found in 1970 Census data, but published 1970 statistics do not provide the age-earnings profile for Locomotive fireman, or else they combine Locomotive engineer and Locomotive fireman.)

These data provide a weak test of my hypothesis about the relationship between the shape of an occupation's age-earnings profile and the extent to which the occupation's labor force is unionized. However, they are at least consistent with my reasoning and can be cautiously accepted as support for it. Perhaps it is worth stressing again that my point in this paper is not to provide in-depth investigation of occupational differences in age-earnings profiles. Rather, my point is to draw attention to the occupational segmentation of labor markets and to the sociological importance of this segmentation. In my two brief analyses of occupational differences in the age-earnings relationship, I have suggested that both occupational differences in the nature of demand for labor and occupational differences in the mechanisms by which supply and demand interact to set wages have consequences for the relationship between a worker's age and his earnings. More importantly, I have tried to show that it is possible to construct and empirically test hypotheses about these occupational differences in wage-setting, and that such hypotheses come from consideration of the very factors which lead to occupational fragmentation of labor markets.

CONCLUSIONS

My major purpose in this paper has been to argue that the processes governing earnings attainment vary from one occupation to another. I have approached this argument from two overlapping perspectives, one economic, the other sociological. Taking the sociological viewpoint, I have reviewed evidence that social factors affecting the earnings attainment process vary substantially across occupations. Looking at earnings from the economic perspective, I have examined the structure of labor markets and have reviewed evidence which strongly suggests that they are

fragmented along occupational lines. These investigations have led me to suggest that there are three major sources of occupational variation in the processess governing earnings attainment: factors leading to occupational differences in demand for labor, factors leading to occupational differences in supply of labor and factors leading to occupational differences in the social organization of work and wage-setting. There is already a great wealth of empirical research on the supply, demand and social organization of labor in different occupations, and I tried to demonstrate that these different types of studies (which are rather widely separated from each other in the sociological and economic literature) can be related to each other rather easily if one adopts the occupation-as-market perspective I advocate here. Once fitted together, these different types of analyses complement each other in shedding light on the process of earnings attainment and wage-setting.

In support of my arguments, I have offered two sorts of evidence: evidence based on empirical analyses of earnings, occupations and related topics, and evidence based on deductions about the nature of individual economic behavior and the structure of labor market demand and supply. I have supported my deductive arguments with empirical tests of some assumptions on which they rest. While some readers may object to one or another of my pieces of evidence, I believe that the support for my central argument is broad-based enough that it cannot be rejected without also calling into question substantial numbers of widely accepted empirical and theoretical studies of earnings, labor markets and occupations.

I believe that the arguments and findings I present here are important for at least two reasons. First, and most significantly, it would seem that the processes by which earnings and wage structures are determined are essentially aggregate level relationships which are best studied at the level of analysis on which they operate. That is, these processes are relationships among the social organization of employers and workers, the supply and demand for various goods and services; the supply and demand for members of the labor force who have the skills, abilities and social qualifications required to produce those goods and services, the social evaluation of occupations and similar factors. It seems that these processes are best studied at the aggregate level of analysis on which they operate, since doing otherwise would achieve a Procrustean fit of group-level phenomena into an individual-level theory. If I have demonstrated that the earnings determination process is differentiated along occupational lines, then I have also demonstrated that this aggregate level phenomenon can be properly investigated by examining the occupational structure of wage- and earnings-setting processes. To show that occupation-level analyses of earnings can provide insight into problems of longstanding sociological interest, I reported two brief analyses of factors which determine the relationship between workers' age and earnings. An extended demonstration analysis could not be given in this article, but there should be no shortage of other applications of the arguments I make here to problems of sociological interest.

Finally, I also believe that the arguments and findings I present here are important because they draw out a number of key complementarities and communalities between economic and sociological theory and research on earnings determination. Finding or publicizing overlap between two accepted theories or disciplines not only increases parsimony, but it also facilitates application of research done under one paradigm to analysis done under the other. Thus, if labor markets are fragmented along occupational lines as I have argued, then research on occupational differences in earnings attainment processes can be (and ought to be) applied to problems in the analysis of labor markets. And research done on labor market differences in wage-setting can be applied to problems in the study of occupations and socioeconomic achievement.

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COMMENTS

ON "DE-PARSONIZING WEBER"

(COMMENT ON COHEN ET AL., ASR APRIL, 1975)

I frankly do not find this paper in which Whitney Pope has associated himself with two coauthors any more acceptable than I did Pope's paper (1973) on my interpretations of Durkheim on which I wrote a recent comment (Parsons, 1975). The authors have repeatedly accused me of "distorting Weber's meaning." I'm afraid I must come back with a claim that, however that may be, they have distorted my meaning.

They rely very heavily on *The Structure of Social Action* (Parsons, 1949), my first book which was initially published 38 years ago in 1937, and, on the whole, play down later writings on Weber. This, to be sure, did have an extended treatment of Weber's work, adding up to four chapters. I did, however, explicitly warn readers of that book that the treatment in it of the work of the four authors I considered—namely, beside Weber, Alfred Marshall, Vilfredo Pareto and Emile Durkheim—was not a general secondary account of their contributions to social science, but a study which focused on certain relatively specific problems. The authors of the paper do not mention this limitation, but it is crucial to understanding what I was trying to do (Parsons, 1949:v). The main objective of the book was to attempt to clarify certain problems of the relation between economic theory and sociological theory.

It was this objective which justified the inclusion of Marshall at all since Marshall was an economist, not a sociologist. My treatment of Marshall in the book was highly selective and that of the other three authors not quite so much so, but still to a very substantial degree. In the case of Weber, my most important starting point was Weber's famous study, *The Protestant Ethic and the Spirit of Capitalism* (1958), in which he assumed what has seemed to many to be the paradoxical position that certain categories of religious commitments have played a particularly important part in the genesis of the special type of modern capitalism, which he called rational bourgeois capitalism, with which he, Weber, was concerned.

Of course, I was not merely aware of the long-standing conception of economic self-interest, but indeed used this as my own starting point. I was particularly intrigued with Weber's hypothesis that a major component of what concretely would be called economic behavior was not, however, to be understood only in quite the traditional terms of classical and neo-classical economic theory. This also provided the principal, initial point of reference for my concern with the normative aspects of social action and the structure of social systems. The authors of the paper are quite right that I put a very strong emphasis on these normative aspects. This is not in the least to say that I was unaware of other aspects of social action, particularly in Weber's work, but I had special reasons in the design of that particular study to emphasize the importance of the normative.

Nearly at the beginning of their paper, the authors introduce what seems to me to be a truly egregious misinterpretation of my views. That is, they say that economically rational action is not normatively oriented but is oriented in terms of interests toward considerations of what they call "practicality." It seems to me that nobody could have even an elementary understanding of economic theory and take this position. Of course, the concept "economic rationality" involves a crucially central normative element. It concerns selection of means to given goals on the part of either producers or consumers or both on the basis of normative standards of rationality which, in the economic case, have to do with the balancing of utility on the one hand, cost on the other. (Of course the concept does not deal with broader meanings of rationality.) Weber was entirely aware of these matters, and the authors are simply wrong when they deny that there is a normative component in Weber's concept of *Zweckrationalität*. Indeed, Weber (1968:212-301) used the conception of economic rationality as the point of reference for formulating this type; though he extended its applicability beyond the economic sphere to include, above all, political rationality but

also cognitive and technological varieties and others (Weber, 1968:1158-211).

In his study of the Protestant Ethic, Weber (1958) introduced another component which had not played a part in the main tradition of economic theory, namely a set of value-commitments anchored in religion. He, of course, associated this with the religious movement which he called "ascetic Protestantism" and illustrated particularly with materials from English Puritanism and to some extent, American sources. This religiously based orientation valued economic rationality but differed from the standard economic treatment by not treating consumption as the primary given goal of economically rational action. Rather, it held that productive activity in the economic sense came to be treated as a moral obligation through the treatment of economically productive roles as "callings," in the sense of a word commonly used in Puritan literature. In Weber's formal classification of types of action, this component of the capitalistic economic behavior with which he was concerned would not be kept classified under the heading *Zweckrationalität*, but rather the other category of rational action would be used, namely *Wertrationalität* (Weber, 1968:3-62).

This case may, however, be used to illustrate a very important point about Weber's modes of procedure as a theorist. Though he formulated what he called "ideal types" and the two types of value orientation belong to this category, he never maintained that they could not occur empirically in combined form (Weber, 1968; Parsons, 1949:520). Indeed, the economic behavior which he focused on the Puritans was both to a very high degree economically rational in the traditional sense and an attempt to implement a value commitment independent of considerations of personal advantage, notably in the utilities of consumption.

It is worth noting that the mode of analysis which Weber first introduced with respect to the relations between ascetic Protestantism and capitalism turned out to be of much broader significance than its application to the field of economic action only. Robert Merton's famous work (1970) on science and society in 17th century England picked up hints made by Weber himself about the relevance of this value complex to concern with scientific investigation. Here, the concept self-interest, in the relatively specific economic sense of it, becomes of secondary importance, but rationality of even more accentuated importance, and clearly includes rationality which is normatively oriented to the proper selection of means to achieve whatever the goals of the activity may be (Merton, 1970; Parsons and Platt, 1973). There are other comparable fields of rational action which I have mentioned, such as Weber's own special concern with the political, but a very important one happens to be the legal. A fact should be remembered here which the authors of the paper do not mention, that Weber was originally trained in jurisprudence and only further in his career became disciplinewise first an economist, then a sociologist.

There is a related set of issues on which a brief commentary needs to be made. This starts with the authors' treatment of Weber's concept of traditional action. It seems to me that they are guilty of a confusion of concepts. Both traditional action as a type of action and traditional authority are ideal types which specify types of action but do not specify motivation. Practically throughout his formulation of a schema of ideal types, Weber carefully avoids commitment to specific correspondences between type of action on the one hand, motivation on the other. It seems to me that the stress which the authors lay on habitation confuses this issue because I would think of habituation as primarily a psychological category of motivation. Again and again, Weber stresses that the motives for conforming to any given type of order may be exceedingly varied. They may include factors of habituation; they may, however, approach the pole of the fullest rationality. What the motivation of the participant actors may happen to be is an analytically distinct question as compared to the typology of action or of types of political order.

There is one particularly important example I should like to elaborate a little further. One of the categories Weber introduces is that of *Brauch* which I translated as "usage" (Weber, 1968:29-31). The authors seem to think that it was one of the more egregious of my misinterpretations of Weber that I imputed a normative component to this category; I am very careful to say *component*, not that the category as a whole is primarily normative. This seems to me both intrinsically correct and a correct interpretation of Weber's meaning. It applies in innumerable contexts such as, for example, *rules* of etiquette.

There is, however, one particular case which is very conspicuous and illustrates my point admirably. This concerns linguistic usage. Indeed, one of the primary historic contexts in which the word "usage" has developed is in the English discussions of language. It seems to me that it would be a strange interpretation of the findings of modern linguistic analysis to maintain that in the usage of language there is no normative

content whatever, though to be sure most of our actual choices of words and construction of sentences certainly contain a large element of habituation.

The dominant recent school of linguistic theory, revolving around the name of Chomsky, is completely unequivocal on this point. To put it in Chomsky's terms, the "deep structures" of a language are unable *alone* to generate very large numbers of specific utterances—at what Chomsky calls "surface levels" (Chomsky, 1957; 1972). The deep structures are utilized through a series of what he explicitly calls *rules* of transformation. Surely the concept rule is a normative concept and is so meant by Chomsky in this context. This is not simply habituation in the psychological sense. Indeed Chomsky himself has some very sharp critical remarks about the attempt to reduce linguistic phenomena to phenomena of habit by, for example, such behaviorist psychologists as Skinner (1957).

The above considerations lead directly to another topic on which I feel the authors have taken an altogether untenable position. This concerns a concept which figures very prominently in the Cohen, Hazelrigg and Pope article, namely that of *interest*. In their discussion of the economic case, as I have noted, they use the concept of interest to obliterate the normative component of economically rational action, denying that it exists or plays any part in Weber's thinking. However, they also generalize far beyond this case. Indeed, they quote from Weber's own work the phrase "material and *ideal* interests." They quote a famous passage of Weber's which I myself have quoted with approval, I think, a number of times. This is to the effect that it is not "ideals which determine courses of action, but the interests of actors." Weber's conception of the range of such interests, however, far transcended the usual categories of economic and political interests. Notably, under such headings as ideal interests, he included very explicitly the interest in religious salvation, the interest in the growth of knowledge through scientific research and many others.

It might help clarify this much vexed problem area if I introduce the phrasing used by W. I. Thomas (1931). I think it would be a correct interpretation of Weber's position that ideas serve, to quote Thomas, "to define the situation." For action defining the situation in this sense, however, does not by itself motivate actors to attempt to implement implications of this definition of the situation. Additional components of the complex of action must be taken into account in order to solve the motivational-implementation problem. Weber (1958) gave an almost paradigmatic example of the necessary kind of analysis in his treatment of how the religiously pious Puritan was motivated to engage in economically productive activity. The authors are quite right in stressing that there had to be a concern with the problem of personal salvation as well as the special set of religious beliefs which originated in Calvinism.

There is a subtle and continuing tendency when such a term as "interests" is used to imply that the "really" important interests are so-called "material interests" or belong in the sphere of what the authors call practicality. It was clearly Weber's position, however, that this was far too restricted a formulation of the concept of interest. Indeed, as a sociologist of religion, Weber made enormous contributions to the clarification of the relevance to concrete action of quite other categories of interest. Apropos of my initial statement it should, I think, be clear that I had special reasons in the design of *The Structure of Social Action* for being particularly concerned with this phase of Weber's work. My relative underplaying of Weber's political sociology was not a simple function of failure to understand it, but rather of selective interest in a certain limited set of problems. The total neglect by the authors of the paper of this consideration seems to me to be one of the main defects of their treatment.

At this point, a brief statement on my views of Weber's political sociology seems, however, to be essential. My reading of the Cohen et al. paper gives me the impression that none of the three authors possesses a thorough command of the German language. This impression persists; though they do, from time to time, quote certain German words which Weber used, notably, in the present context, the word "*Herrschaft*." As they note, a few years ago I had a certain discussion in a review (Parsons, 1972) of Reinhard Bendix's work (Bendix and Roth, 1971) about the translation of that word. The authors follow Bendix in uniformly translating it as "dominance." I made a careful distinction, which Weber also makes (1956:122-4), between legitimate and non-legitimate *Herrschaft*. Where legitimate *Herrschaft* was involved, I used the term "authority," reserving other modes of expression for the non-legitimate cases.

It seems to me that the authors seriously misinterpret the significance of the concept of legitimacy in Weber's theoretical structure. As the authors clearly recognize, Weber was far indeed from believing in a single factor view of the determination of human action. They are quite correct in quoting him as saying

that, once a normative order has been established, motives of conforming behavior, insofar as it exists, may be exceedingly variant, including commitment to legitimacy of the order, but also various aspects of self-interest and the like. The question of the range of possible motives for conformity, however, should be clearly distinguished from the question of the institutional grounding of a system of normative order. Here, in the latter context, it seems to me that the concept of legitimacy is far more crucial than the authors of the paper will concede. I believe that Weber knew what he was doing when he used modes of legitimation as the primary basis of his famous classification of types of authority. (I prefer the word "authority" to "dominance.") This, of course, is not in the least to say that empirically, dominance without legitimation historically has not played an extremely important role. This, however, is not the issue with which I am concerned.

To me, the question of the balance Weber drew between the factors involved in the legitimation of normative order in society and the factors of self-interest in ignoring or defying such legitimation is an empirical, not a crucially central theoretical question. Weber certainly went very far in emphasizing the latter set of factors, but this does not, as I have just said, dispose of the theoretical significance of the concept of legitimacy in his conceptual scheme. I should again call attention, which the authors do not, to the fact that Weber was in the first instance trained in jurisprudence and was deeply imbued with the importance of law. To be sure, his personal experience was mainly with law in the Prussian tradition, which we may conclude was a rather special and extreme case among modern systems of law. Nevertheless, for example, in my address in Heidelberg on the occasion of the German Sociological Association's celebration of the centenary of Weber's birth, I stressed the centrality of the sociology of law in Weber's thinking (Parsons, 1965), whereas Professor Bendix (1965) stressed the centrality of the political sociology. It is perfectly clear that the three authors of the present paper follow Bendix in this respect. I do not, however, think they make the case that this must be *the* primary model.

This commentary clearly does not cover all the points which might have been discussed relative to the Cohen, Hazelrigg and Pope paper. I think, however, that it covers a sufficient range so that the grounds of my objection to their point of view should not be too obscure. I, for example, do not find their paper particularly distinguished for specificity of reference, to my own work in particular.

The issues in this controversy, as was true in the controversy with Whitney Pope over Durkheim (Pope, 1973; Parsons, 1975), and Pope's rejoinder clearly, however, are serious. I do not think it is a question of simple "alternative opinions" about Weber's work. I have, of course, stressed the senses in which my initial study of Weber was not meant to be and was not a general assessment of evaluation of Weber's sociological theory. However, the charge of distortion, which the authors of the paper have leveled against me is certainly applicable to them, I think, both in their interpretation of Weber's own work and of my own commentaries on it. At any rate, I hope I have made clear that the problems which Weber's work presents to the social scientist are far more complex than the authors of this paper adequately take into account.

Perhaps I may be permitted, in the final conclusion, a note of wonderment that I am accused of being an "old fashioned evolutionist." I certainly am an evolutionist in the field of human action. My evolutionism, however, is quite different from that of the late 19th century, of which, perhaps, Herbert Spencer (1925-9) may serve as the primary example. This point is barely mentioned, with no attempt at grounding, but I think it is a symptom of the polemical orientation of the authors rather than of their critical responsibility and acumen.

Also, I may call attention, as the authors do not, to the fact that, nearly twenty years after publication of *The Structure of Social Action* in 1937, my own orientation to the focal problem of the latter book—the relations between economic and sociological theory—underwent a major transformation. This is documented in the book *Economy and Society* (Parsons and Smelser, 1956).

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REPLY TO PARSONS

Parsons' commentary is disappointing inasmuch as it fails to advance the discussion substantially beyond the points made in our article. Apparently, Parsons' thinking about Weber has *not* changed, for although he states that we were wrong to rely so heavily on his *The Structure of Social Action* (1949), he gives no evidence of any interpretation of Weber that differs fundamentally from that in *The Structure* and his review of Bendix' work (1960). Although we allegedly distorted his views on Weber, Parsons demonstrates no specific distortions except for our failure to weigh the emphasis of his (1964) Heidelberg address (Parsons, 1971) against the emphasis of his numerous other works on Weber. Instead of citing supposedly neglected changes in his viewpoint, he devotes much of his commentary to a reaffirmation of his early argument—most importantly, his "very strong emphasis on these normative aspects" of Weber. Parsons says he had "special reasons to emphasize the importance of the normative." The question is whether this emphasis can accurately be attributed to Weber.

TYPES OF ACTION ORIENTATION AND TYPES OF SUBJECTIVE MEANING

Our paper argues that Parsons distorted Weber, chiefly by systematically overplaying normative and understating nonnormative elements in Weber's sociology. We began with Weber's four types of action orientation and three types of subjective meaning. Parsons leaves the bulk of our analysis unchallenged; we consider those points where he takes issue with us.

Weber (1968:25) defined traditional action as action "determined by ingrained habituation." Parsons asserts that our analysis confuses the issue by stressing the "primarily. . .motivational psychological category" of habituation. However, the emphasis on habituation is not ours, but Weber's (1968:25; see also 31, 312, 333). Parsons' quarrel is with Weber, not with us. Our original point was that Weber's category was not primarily normative. Parsons does not challenge us on this—the central issue.

Parsons continues to misread Weber's usage category. Weber's definition (1968:29, 333) did not mention norms nor does Parsons cite passages in Weber including any such reference. Whereas in *The Structure* Parsons (1949:678) cited "standards of 'good taste' " as an example of usage, he now adds a second example, namely, "rules of etiquette." In neither instance, however, does Parsons locate his example in Weber. Rules of etiquette are typically guaranteed by sanctions; consequently, they more closely approximate Weber's category of "convention" (viz. Weber's reference [1968:34] to "the rules governing. . .social intercourse" as an example of convention) than they do "usage."

Convention has a normative component but usage does not.

A more serious disagreement between Parsons and ourselves concerns the interpretation of *Zweckrationalität*. Parsons asserts a "central normative element" in Weber's concept. Resolution of the issue turns on the definition of normative: since Parsons' works contain varying, sometimes conflicting, definitions of norm and normative, using passages from *The Structure* we (1975:231) carefully explicated the definition employed in our paper, and according to *this* definition, Weber's category contains no normative elements. Further, Parsons cites nothing from Weber identifying a component of *Zweckrationalität* that is normative in terms of this definition. Nor did Weber himself refer to the concept as a normative category. Weber's category could be made normative only by abandoning the definition of norm used by Parsons in *The Structure*.

Raising the issue of whether the behavior of ascetic Protestants as described by Weber is *Zweckrationalität* or *Wertrationalität*, Parsons calls their behavior *Wertrationalität*. This classification is ironic because in the following paragraph Parsons notes that Weber's formal classification of types of action are ideal types and that empirically given action typically falls into more than one of Weber's categories. We agree with Parsons on the *wertrational* aspects of this behavior, but argue that it was also partly *zweckrational*, a means as well as an end in itself.

In *The Protestant Ethic and the Spirit of Capitalism*, Weber (1958:109-10) emphasized that in an age which stressed the importance of the afterlife "the question, Am I one of the elect? must... have forced all other interests into the background," and Weber (1958:172) referred to "the religious valuation of restless, continuous, systematic work in a worldly calling, as...the surest and most evident proof of rebirth and genuine faith." He (1958:178; emphasis added) felt that "Protestant Asceticism...created the force which was alone decisive...: the psychological sanction of [faithful labour] through the conception of this labour as a calling, as the best, often...the only *means* of attaining certainty of grace." Weber argued that ascetic Protestants followed the work ethic in large part to achieve certainty of their salvation and thereby rid themselves of the terrible anxiety concerning their fate in the hereafter and also (Weber, 1946:312-7) to remain members in good standing of their sects (with the privileges of partaking of the Lord's Supper and enjoying a good credit rating). Thus Parsons' assertion that Weber's Puritans acted independently "of considerations of personal advantage" is contrary to Weber. In short, the behavior of the ascetic Protestants was in part *Wertrationalität* and normative, in part *Zweckrationalität* and nonnormative. While the normative component of the Puritan's behavior was greater than that of the behavior of most religious actors described by Weber, even here the normative components *(Wertrationalität)* never achieved ascendancy over ideal interests and the nonnormative component *(Zweckrationalität)*.

As for the fate of the calling and the *wertrational* aspects of action it entailed, Weber (1961:270) held that "today the concept of the calling is a *caput mortuum* in the world." Identifying the presuppositions and characteristics of modern capitalism, Weber (1961:208-9) identified one such "feature [as] free labor. . .a class compelled to sell its labor services to live... Workers who in the formal sense voluntarily, but actually under the compulsion of the whip of hunger, offer themselves." Furthermore, "all economic activity in a market economy is undertaken and carried through by individuals acting to provide for their own ideal or material interests" (Weber, 1968:202). Weber's primary concern with the teachings of the great Protestant reformers lay not in their religious doctrines per se, but rather in the part played by those doctrines in giving rise to and shaping modern western capitalism. For him the doctrines (notably that of the calling) of the Reformers, which enjoined a *wertrational* response, had the unintended consequence of helping to establish modern capitalism, which creates and selects the subjects and the behavior it needs; accordingly economic behavior frequently entails a dominant *zweckrational* component.

HERRSCHAFT

In an earlier work Parsons (1960:752) rejected Bendix' translation of *Herrschaft* as "domination," preferring instead to substitute "leadership." Our paper indicates why "domination" is preferable to "leadership" as a translation of *Herrschaft*. In his commentary Parsons does not reiterate his preference to translate *Herrschaft* as leadership; indeed, he does not say how he now prefers to translate it. Our specific disagreement now centers on whether "legitimate domination" or "authority" is preferable as a translation of *legitime Herrschaft*. One argument in favor of the former is that the translation of *legitime Herrschaft* as authority suggests that *nichtlegitime Herrschaft* be translated as

nonlegitimate authority (a self-contradictory concept).

Weber (1968:943) first distinguished between *Herrschaft kraft Monopolie* (citing "monopolistic domination in the market" as "the purest type") and *Authorität* ("power to command and duty to obey"). The famous three-types of *legitime Herrschaft* come later and classify *only* the *legitimate* subportion of all *Herrschaft*. Parsons implies that *legitime Herrschaft* was the most important with the other types constituting a theoretically uninteresting residual category. Such an approach ignores Weber's stress on market power. Parsons emphasizes the normative aspect of domination (i.e., the belief in legitimacy) and underplays the analysis of domination as a structure of power. In contrast, Weber (1968:941-2) defined *Herrschaft* as power and considered both its structural aspect and the extent to which, in the case of *legitime Herrschaft*, that structure was or was not reinforced by beliefs in legitimacy. While Parsons' approach implies that obedience to *legitime Herrschaft* derives primarily from the belief in legitimacy, Weber (1968:31, 53-4, 212-4, 941-7), emphasizing both the importance of power and self-interest, noted instead that obedience resulted from various factors with the belief in legitimacy being highly of *variable* and, therefore, sometimes of minimal importance as a cause of obedience. In particular, Weber saw obedience as highly dependent on the way in which socially structured power differences in turn structured self-interests conducive to obedience.

OUR USE OF *THE STRUCTURE OF SOCIAL ACTION*

While Parsons questions our use of *The Structure*, we had good reasons for relying on it to a significant degree: it is Parsons' longest, most comprehensive, most systematic, best known and most influential statement on Weber. Given *The Structure*'s classic standing, the interpretations derived there (and not only of Weber) deserve attention. Notwithstanding, our reliance on *The Structure* was far from an exclusive one: we include 15 references and quotations from four works by Parsons which followed *The Structure*. Where appropriate, as in treating Parsons' analysis (1960) of Weber's political sociology, we used later works to show his position on a particular topic. However, it was appropriate to do so only to the degree that Parsons' later works qualified or rejected the earlier interpretations; although Parsons' later works do sometimes accord different emphasis to the different works in the totality of Weber's writings, Parsons has not identified any interpretations propounded in *The Structure* to which he no longer subscribes. We question whether Parsons does not want to have it both ways. On the one hand, he implies that we were mistaken to rely heavily on *The Structure*; on the other, he asserts that the interpretations derived therein remain valid and he continues to defend them. Parsons should have identified specific instances to demonstrate where our use of *The Structure* produced an erroneous rendering of his position, but he has not.

REPLY TO ADDITIONAL ALLEGATIONS BY PARSONS

In a reply which contains not a single specific page citation, Parsons refers to the lack of "specificity of reference" in our work. Further, using Parsons' first chapter on Weber (1949:500-38) as a sample, *The Structure* contains a per page average of 4.5 page citations and quotes compared to 25 for our article. Notwithstanding his observation, Parsons cites no passage we should have noted but did not, nor did he otherwise show how the failure to include additional specific references detracted from the quality of our work.

Parsons notes that we say little about Weber's sociology of law. But our paper is not an exhaustive treatment of Weber and our focus was necessarily circumscribed by Parsons' own commentaries. In *The Structure*, Parsons said little about Weber's sociology of law. (The index to *The Structure* contains two relevant entries.) Parsons here comes uncomfortably close to taxing us with his own failing.

Parsons infers (without saying on what basis) that none of us commands a thorough knowledge of German. However, he identifies no adverse effects of this presumed deficiency. His only specific complaint—our preference to translate *Herrschaft* as domination—is odd since here we follow Bendix, whose credentials as a German scholar are impeccable.

Parsons rejects our interpretation of *Zweckrationalität* as one which could not possibly be put forth by anyone who had "even an elementary understanding of economic theory." However, our discussion concerned not economic theory but rather Parsons' interpretation of Weber's concept. Parsons simply does not document that our

allegedly insufficient understanding of economic theory resulted in errors in our understanding of Weber. Similarly, in rejecting our interpretation of *Brauch* (usage), Parsons appeals to "the findings of modern linguistic analysis" and refers to the work of Chomsky. But these references are irrelevant to the assessment of our interpretation of the meaning Weber attached to *Brauch* and of the meaning which Parsons attributes to Weber. (Incidentally, the "transformational rule" concept is *not* necessarily normative, *unless* normativity is equated not only with empirical regularity but also, in this case, with the regularity or orderedness of "deep structure.")

Parsons identifies a "subtle and continuing tendency" on our part "to imply that the 'really' important interests are...'material interests.' " The tendency is so subtle we are unaware of it. At no point do we state such a view. The Weberian concept of interest is inclusive and subsumes both material and ideal interests; the latter are just as real, important and motivating as material interests. Both in our paper and in this reply, we cite instances of Weberian actors motivated primarily by ideal interests.

Our point is not that Weber discounts ideal interests but rather that Parsons minimizes the importance of interests in Weber's sociology. This is not to say that Parsons does not sometimes, as in parts of his commentary, correctly identify the importance of interests in Weber's scheme. But though this stress is correctly noted at points, at others, particularly when the absolute or relative importance of normative elements are in view, Parsons discounts the importance of interests so that relative to norms they (like other vital elements in Weber's sociology) are relegated to a subordinate role.

Finally, we wish to clarify our reference to Parsons as an "old-style evolutionist." We did not have the evolutionism of Herbert Spencer particularly in mind; evolutionary theory has developed substantially during the past three decades, and it was against the current, "neo-evolutionary" theorization (as represented in the varied and careful works of Ernst Mayr, V.C. Wynne-Edwards, *inter alia*) that our perception of "old-style" was constructed. As a close reading of the works will clearly confirm, Parsons' *Societies: Evolutionary and Comparative Perspectives* (1966) is much closer in the logical structure of its theoretical thinking (typological rather than populational-structuralist) to the work of Durkheim than to that of e.g., Mayr's *Animal Species and Evolution* (1963).

CONCLUSION

Parsons notes "special reasons" for *The Structure*'s "selective interest" in a limited set of "relatively specific problems"; *The Structure* was not intended as a general analysis of Weber's sociology. Certainly not every commentary on Weber could or should purport to be a general interpretation. Of course, Parsons' analysis in *The Structure* has been interpreted for years as just such an interpretation and we might regret that while it was being so interpreted, he did not come forth to remind those using it in this way of the limited nature of his analysis. Indeed, we may wonder if comments such as his relatively recent observation (1967:41n) that *The Structure* "may still be the most comprehensive analytical treatment of Weber's accomplishments in theory" do more to reinforce or dispel such an approach to his work. Be that as it may, a selective focus explains only the incompleteness of Parsons' interpretations. Beyond such incompleteness, we argued that distortion existed both in innumerable specifics and in the general cast given Weber's work. This distortion cannot be excused by an appeal to "selective interest."

We agree with Parsons that our differences over the proper interpretation of Weber are serious and not simply a matter of equally valid alternative interpretations. Beyond this, Parsons suggests that we are the ones who distorted Weber; however, at no point does he cite passages in Weber at variance with one of our stated interpretations. Parsons further claims that we distorted his meaning. Of course, we must defer to Parsons in the interpretation of his intended meanings; however, nowhere does he show that one of our interpretations is inconsistent with that part of his writings we cited in deriving it. *Prima facie* when, as in the instance of the meaning of norm and normative, Parsons' own definitions change, then interpretations based on his writings may no longer accord with current or retroactively intended meanings. Rather than documenting the distortions attributed to us, the bulk of the relevant portion of Parsons' commentary simply re-asserts his basic position as first developed in *The Structure*, thereby establishing even more firmly the grounds on which we conclude that he distorted Weber. Though our paper does not purport to be a general interpretation of Weber, we hope to have corrected an influential part of the written record and in so doing to have identified and clarified, as have others (e.g., Gerth and Mills, 1946; Bendix, 1960; Nelson, 1974), some key elements in Weber which need to be given due

accord in any general interpretation of his work.

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THE MYTH OF "TERRITORIALITY" IN VAN DEN BERGHE'S "BRINGING BEASTS BACK IN"*

(COMMENT ON VAN DEN BERGHE, ASR DECEMBER, 1974)

It is with sincere distress that I write this comment on van den Berghe's essay, "Bringing beasts back in: toward a biosocial theory of aggression" (*ASR* 39 December, 1974). But, the fact that this *Review* has lent its authority to such misinformation compels a response. The urgency arises from the fact that in van den Berghe's essay one finds a significant intrusion into sociology of the "territoriality-aggression" myth at a time when that myth is suffering a well-deserved death in anthropology (and even in the popular press). Consider the following (non-exhaustive) list of errors:

(1) Van den Berghe cannot get his argument in logical order. For most ethologists who believe in a biogenic "territoriality," such a mechanism arises in a species because it is a means of *preventing* aggression; it is not a *cause* of aggression. For van den Berghe, it is both simultaneously (p. 787). How are we to interpret this proposition? If it is equally cause and preventative, there should be no *net* relationship between aggression and territoriality; in that case, the model is largely moot. If territoriality is more a preventative than a cause of aggression, there should be a *negative* association—the opposite of what he wishes to argue. But, if territoriality is more a cause than a cure for aggression, the instinct should never arise in the first place (since it would not be selected for by evolutionary processes). The actual argument that ethologists make, which van den Berghe misses, is that territoriality arises as a mechanism for distributing species members evenly among limited resources, but that certain circumstances (largely, increased population) can produce infringements of territory, with aggression resulting as a "wired-in" response. Even this argument is inadequate.

(2) Van den Berghe presents *no evidence* to support *any assertion* he makes with regard to human behavior. He asserts, for example, that humans are more aggressive than other species (p. 780), but cites neither data nor authority,

*Thanks to Mark Baldassare for helpful assistance and comments.

solely his personal observations. Worse yet, he often ignores the evidence which does exist. (See also, Mazur and Robertson, 1972:164-6.)

(3) Van den Berghe *asserts* that humans are "overcrowded hopelessly" (p. 780), ignoring inconvenient facts such as the general *positive* correlation between the size and density of settlements and the wellbeing of their residents (Hawley, 1972; Richardson, 1973; Fischer, 1973).

(4) Van den Berghe *asserts* that man is territorial and is territorial by "biogenic determination" (p. 782). Here is the key argument. I invite the reader to search pages 781 to 783 and try to find a shred of evidence presented there to support his analysis besides assertion, analogy and anecdote. Even on theoretical grounds, van den Berghe's argument is faulty. The basis for his conclusion that territorial behavior is biologically determined is that he can find instances in all societies of which he knows of territorial defense. Even were it factually correct, this "evidence" would not prove the point, for such universality can be the result of universal social structural prerequisites. Similar behaviors—for example, people's defenses of their prestige, or their social influence, or their time—are just as universal, but that does not logically suffice to establish an instinct—a "prestige instinct," an "influence instinct," nor a "biogenic temporality."

There are methods and standards for determining the instinctiveness of a behavioral pattern. Anthropologists evaluate, for example, its degree of stereotypy, its presence in evolutionary proximate species, and its evolutionary advantage. On grounds such as these, the general conclusion has been that human territorial behavior is not biogenically determined (see Montagu, 1973; Martin, 1972; Mazur and Robertson, 1972:20-3; Nelson, 1974).

(5) There are respectable arguments and some data suggesting that aggressive behavior may have a biological component, though van den Berghe does not present any (p. 782). He makes a reference to testosterone levels, but there is good reason to believe that hormonal levels are as much a consequence as a cause of aggression. The state of knowledge here seems to be that human biology *facilitates* aggression, but does not *determine* it—Ardrey, Morris, Lorenz et al. notwithstanding (see Nelson, 1974; Montagu, 1973; Scherer, et al., 1975:Ch. 1,2).

(6) Van den Berghe *asserts* quite confidently that crowding causes aggression, in "blissful ignorance" (to use his phrase, p. 779) of the evidence. First, it is wrong to imply that ethologists have discovered such a relationship among animals (p. 786). Density is as likely to be associated with passivity, or with nothing, as with aggression (e.g., Baldwin and Baldwin, 1972; Montagu, 1973). Second, it is wrong to assert that "urban proletarians" have higher rates of violent crime than do "uncrowded peasants" (p. 786). It may "seem" that way to van den Berghe, but does not "seem" that way in the data. In many contemporary nations (e.g., Mexico) and in many earlier historical periods, violence is or was greater in rural than in urban areas (cf. Wolfgang and Ferracuti, 1967; Mulvihille and Tumin, 1969; Lane, 1969). Finally, there have been in recent years a great number of empirical studies attempting to link crowding to aggression among humans, and they generally show *no* significant independent association (e.g., Freedman, 1972; Schmitt, 1963; Loo, 1972; Freedman et al., 1972; Ittelson et al., 1970; Mitchell, 1971; see Fischer et al., 1974). Van den Berghe is wrong, again.

In sum, there is little of either supported fact or plausible theory in "Bringing beasts back in." There is in that essay, however, a significant misconception, one worthy of further comment. Van den Berghe treats "territory" in a manner which is, to borrow Marxist terminology, a "reification," even worse, an "ideological mystification." That is, he attributes to territory an ontological independence in human affairs; he treats it as a category of *sui generis* importance. (Consequently, people can have biological drives directed at territory, just as they have ones directed at food.) This is probably not the best way to understand territory in human behavior. Territory, or space, is no more the essence of "territorial behavior" than is brightly colored paper the essence of economic behavior.

Instead, space, like time, is a medium of human activity, holding a different significance depending on the activity involved. Examples: for a farmer, space is a positive resource; and, if he is to earn a living, he must keep intruders out. For a nightclub owner, it is also a resource; but he must bring people *in*. For a stockbroker needing but a desk and phone, it is a trivial resource. Conversely, space can be a cost, as in the case of the suburban adolescent separated by an exasperating distance from the "bright lights" (even while it may be a resource for his parents because it separates them from urban "undesirables"). The point is that territory is not meaningful as an autonomous, concrete object. It takes on various meanings depending on how people use it or confront it. Consequently, it is unlikely that humans operate with a socially-blind territorial instinct.

This understanding of territory in human behavior is, as is van den Berghe's, speculative.

But, it is far more consistent with current scientific knowledge.

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A COMMENT ON VAN DEN BERGHE'S BIOSOCIAL MODEL OF AGGRESSION

(COMMENT ON VAN DEN BERGHE, ASR DECEMBER, 1974)

Pierre van den Berghe's contribution to a biosocial theory of aggression (1974) confirmed a heuristic I have found to be of value in reviewing such pieces: the number of citations to primate behavior studies involving baboons, macaques and gibbons minus the number of citations to studies involving chimpanzees and gorillas is directly proportional to the innate violence attributed to human nature. Yet, my initial citation count did not totally prepare me for the lack of care with which he spins his sordid evolutionary tale.

Unlike Mazur (1973) who at least attempts to identify homologies, van den Berghe drifts through the primate series picking his analogies with human behavior where they seem to fit his own impressions of human cross-cultural similarities as shored up with anecdotal evidence. For man's obvious territoriality, it is the gibbon; for his hierarchic proclivities, it is the baboon; and then of course there is aggression. Everyone knows that man is the most violent creature on the face of the earth! The justification he cites for his selective analogies is that salient behavioral discontinuities evolve in the space of a few thousand years and that they result in forms which are highly related to a species' level of innate aggression as moderated by population pressures and socially created needs.

The former is not only suspect on genetic grounds (cf. Alland, 1973), but it would suggest more heterogeneity in the human gene pool that he constantly asserts. Even more questionable is his "Causal Model of the Relationship between Population Pressure, Aggression, Territoriality, and Hierarchy." For one, I am entirely unclear about the domain subsumed in the model. He explains the differences between both species and contemporary human cultures with his model and he also uses it to account for how man evolved his biological propensities. This would suggest that population pressure, for example, is a salient ambient condition in both *modifying* the genome and *moderating* the behavior coded in it. This is interesting speculation, but since ethologists have yet to discover anything similar, it relies on a highly unique and serendipitous development in evolutionary history.

Finally, van den Berghe's consistent use of the term "innate" is highly inappropriate. Many notable ethologists have dropped their use of such terms in apparent deference to the inexorable interrelationship between acquired and inborn factors and to the complexity of the biopsychology of development. Labeling a propensity "resistant to learning" seems a more suitable and precise convention.

There is little question that we should bring the beasts back in, but we shouldn't abandon care in opening the cage. Pithecomorphizing man is no less a sin than anthropomorphizing the apes.

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CROSS-SPECIES COMPARISONS OF AGGRESSION
(COMMENT ON VAN DEN BERGHE, ASR DECEMBER, 1974)

The lack of an accepted paradigm for comparative primate sociology may explain the inconsistency between van den Berghe's claim that man is "higher on. . .aggressivity. . .(than) nearly all of. . .the higher primates" (1974:778), and my own claim that humans are among the least aggressive primates (Mazur, 1973; Mazur and Robertson, 1972). There are (at least) two ways to compare human and nonhuman social behavior, and these may lead to opposite conclusions, as in this case. On the one hand, we may focus on the total society as the unit of analysis for cross-species comparisons; on the other hand, we may compare normal face-to-face interaction, either between familiars or strangers. If we choose total societies for our comparison, then large-scale warfare between nations certainly qualifies man as the most aggressive primate.

I believe it more appropriate to make cross-species comparisons at the level of normal face-to-face interaction, primarily because nonhuman primate societies rarely exist above that level. Nearly all troops, which have been studied, are small enough so that most, if not all, individuals appear to know each other. To compare aggression within such primate troops to international warfare seems, to me, wholly inappropriate. One ought to compare human and nonhuman aggression in similar situations, either within established small groups, or between strangers in face-to-face interaction, or in any other social activity *which exists for both species.* The same problem occurs in cross-species comparisons of status systems. Van den Berghe (1974:781, fn. 5) denies that the dominance order of a primate troop is fundamentally different from the stratification system of a nation, emphasizing similarities between a coalition of dominant baboons and the upper class of human society. Perhaps he is correct, though I doubt it. In any case, the interpretive problem is minimized if we make our comparisons while holding social activity constant across species, comparing human and nonhuman status within the context of the established small group; then empirical regularities become relatively clear (Mazur, 1973).

What evidence says that nonhuman primates are less aggressive than man in face-to-face interaction? Contrary to popular myth, monkeys (and other animals) *do* kill each other, and not infrequently. Zuckerman's classic study notes that of about 150 baboons introduced into a zoo colony over a six-year period, 38 died in fights (1932:220-2). Fatal fighting has also been reported between wild baboons (Johnson, 1972:19). I was casually observing a well-established semi-free-ranging colony of

macaques when an adult male killed a juvenile; that was during a three month period of observation. I have never witnessed a human murder during my 35 years. In 1973, Atlanta had the highest murder rate of any American city, 21.8 per 100,000 population (FBI, Uniform Crime Reports, 1973:78), suggesting that even in that city, most people never witness a murder.

Aggression is not limited to killing, but also includes physical assault and threat thereof (van den Berghe, 1974:779). In a recent national sample of Americans over age 17, only 28% of respondents report *ever* having been punched or beaten by another person and only 16% report being punched or beaten as an adult. Seventeen percent report having been threatened with a gun, or shot at. One and one-half percent report someone taking something from them by force during the last year (Davis, 1973:60-1). These rates are unpleasantly high on an absolute basis, but compared to other primates, they are relatively low. Humans—at least Americans—rarely are threatened or assaulted. In contrast, most prosimians and monkeys have threats and attacks as part of their daily existence; wounds are common. Even chimpanzees, usually characterized as gentle animals, show an ample rate of fighting (van Lawick-Goodall, 1971). Of all primates, man probably has one of the lowest rates of aggression against his fellows during normal face-to-face interaction.

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REPLY TO FISCHER, MOBERG AND MAZUR

My main article (van den Berghe, 1974) was of necessity sketchy because of space limitations and tentative because of the fragmentary nature of the evidence. Ethology is a young discipline, and *human* ethology is in its infancy. My intent was not to state dogma, but to move sociologists away from dogmatic social environmentalism and to suggest a conceptual framework wherein human behavior could be reinterpreted in terms of a complex interplay between biogenic, ecological and sociogenic factors. I am delighted that my piece is provoking controversy and even "sincere distress," and I fully expect to be shown wrong on a number of points. I generally prefer to opt for fertile error rather than for sterile exactitude.

Fischer raises several important issues, a full reply to each one of which would require a piece of article length. His most basic criticism comes last. He accuses me of treating territory as a "reification" or "ideological mystification" with a *sui generis* reality. My definition of territoriality (the defense of fixed space against use or occupation by conspecifics) is strictly based on observable behavior. Some species exhibit that behavior, while others do not. Territoriality is a short-hand descriptive label derived from concrete, observable behavior. There is no mystification involved. The behavior of a species determines whether territory is important for that species, not the mere abstract existence of physical space. However, when Fischer speaks of "universal social structural prerequisites" as an alternative explanation to mine for human territoriality, I read in that functionalist jargon a genuine ideological mystification.

Second, Fischer accuses me of presenting the case for a partly biogenic basis for human territoriality by "assertion, analogy and anecdote." Whether I "assert" or "suggest" is a matter of semantic interpretation. I *meant* to suggest, not assert, because the evidence is still fragmentary. An "anecdote" is merely evidence one wants to discount. There too, Fischer's charge is nothing more than loaded semantics. I could produce hundreds of instances of territorial behavior in human groups as diverse as Pygmy hunters, Masai herdsmen, Peruvian peasants, American under-

graduates, Tibetan monks and Soviet concentration camp inmates. Specific references from the anthropological literature are scattered and unsystematic because few anthropologists have used the concept of territoriality for humans; but still, one would have no difficulty in mustering scores of references, so much so that the burden of proof is on those who want to deny the territoriality of *Homo sapiens*. Let Fischer document a few stable human groups, every member of which is equally "at home" in any part of the group's habitat, and I will gladly withdraw my argument. (By "stable" group, I mean anything more permanent than, say, a subway crowd or a concert audience. The test would be any group that makes collective use of a given space for at least a couple of days, e.g., passengers in a ship, guests in hotels and the like. Perhaps the crucial test is spending a night together, for there is remarkable stability of sleeping space in our species, but that remains to be investigated.)

In short, the difficulty is not in multiplying instances of territorial behavior in humans, but in documenting its *absence* in some groups. Using the kind of observational techniques that ethologists have developed, it would be difficult to escape the conclusion that *Homo sapiens* is territorial. Some researchers (Austin and Bates, 1974) have successfully used ethological concepts and observation methods on human groups, thereby documenting in detail what we already knew by experience and "anecdotes."

The crux of Fischer's criticism of my use of territoriality for humans is that my comparisons between human and other animal behavior are based on analogies rather than homologies. Fischer, therefore, urges me to adopt the kind of phylogenic "proof" that, for instance Mazur (1973) has used in dealing with hierarchy in humans. The phylogenic argument runs as follows: if certain specific features (in this case, behavioral syndromes) are present among closely related species, then this is strong evidence of biogenic origin. Thus far, the argument is valid. However, in its negative form it is not. If a behavioral syndrome is found in one species but not in related ones, this does not *invalidate* the biogenic hypothesis. There are many cases of convergent evolution and numerous instances of closely related species exhibiting sharply different forms of behavior. Often a behavior trait is unique to a species, but quite stable in that species, even in widely different environments. Among primates, a good instance is the contrasting behavior of closely related baboon species, which I mention in my article (1974:781), and which is widely reported in the primatological literature (Kummer, 1968; 1971).

The general conclusion is that one must always be prepared to entertain the hypothesis that behavior, though species-specific, is the product of the interaction of genetics and environment, so much so that to conceive of nature and nurture as *alternative* explanations is no longer tenable. Despite Fischer's attempts to reduce my position to one of rigid instinctivism, I emphatically take an eclectic view of the determination of vertebrate, mammalian, primate and human behavior (1974:777, 787). This position now represents the consensus of many behavioral scientists of widely different pursuasions and disciplinary backgrounds (Crook, 1973; Lehrman, 1962; Schneirla, 1973; Tinbergen, 1968). The burden of evidence leans against *both* the more strongly instinctivist theorists à la Lorenz (1966) and the dogmatic environmentalists à la Montagu (1973). At the risk of being dogmatically undogmatic, I would argue that any theory that rejects the importance of biology, ecology, learning or culture in the explanation of human behavior is no longer tenable.

Let us return specifically to territoriality. Territoriality is obviously not a single identifiable feature, like, for example, a backbone, which appears at one point in the evolutionary process, and then is transmitted to thousands of species. Territorialty is merely a descriptive concept applied to a wide range of behavior which is often species-specific, and which probably represents hundreds of cases of convergent evolution. Different species are territorial in very different ways. To argue that, because chimpanzees, gorillas, baboons and many other primates are not territorial, therefore territoriality in humans is sociogenic is similar to arguing that, because wings are uncommon among mammals, the flying behavior of bats is a cultural adaptation and that all bats fly because of "universal social structural prerequisites."

It is most probably true that to compare the territorial behavior of humans and, say, gibbons is to engage in analogy, not homology. The same is true when one compares the flying adaptations of, say, birds, insects, mammals and fishes. The absence of homology does not disprove the biogenic hypothesis. What most social environmentalists mean, however, when they decry the comparison of human and non-human behavior as analogical is that *Homo sapiens* is such a unique animal that his behavior cannot meaningfully be compared with that of any other species. Such an extreme position is sheer dogmatism. Every species is unique in

some respects, but why should we have the monopoly of uniqueness in *all* features of our behavior?

Social scientists often exhibit a curious double standard in their use or rejection of analogies. They frequently object to analogies when the issue is biological, while happily wallowing in them when comparing human societies. When historians show reluctance to use concepts that have been analogically derived from historically unrelated societies (e.g., "caste" in India and in the United States, or "feudalism" in Japan and Europe), social scientists often accuse them of being historicist, non-nomothetic and anti-scientific. How many social scientists, for instance, would object to the use of the term "irrigation" in both pre-Columbian Peru and ancient Egypt because this is a "mere analogy"? Yet, the most enthusiastic generalizers between human societies suddenly become dogmatic historicists when it comes to human biology.

As yet, there is no firm proof of the biogenic basis of territoriality in humans, nor indeed of the territorial behavior of many other species. Unless a specific form of behavior can be causally linked to a specific biological mechanism (such as a gene or a hormone), and unless the causal link can be established through experimentation, no proof-positive exists. Such firm proof will be difficult to establish in humans because of the great complexity of our territorial behavior, and because that behavior is *also* partly determined by ecological and sociocultural factors. Any expectation of territoriality being caused by a single gene is almost certainly wrong. The more we are able to link human and non-human behavior to biological factors, the more the complexity of those linkages becomes apparent and the more crucial intervening environmental variables are seen to be. (The literature on the links between testosterone and aggression, for instance, is a case in point.) We are not blindly instinctive animals. Neither are other mammals. We *do* have a vast capacity to modify our behavior through learning. But our behavior also remains recognizably human under a wide variety of environmental conditions. We find it normal for a human to shoot a prowler in his home, but we would find it very odd if we saw our neighbor mark the boundaries of his garden by urination. So would an Eskimo or a Zulu.

To summarize, there is sufficient universality of territorial behavior in our species and sufficient uniformity in its specific expressions to lead to a *presumption* of *partial* biogenic *predisposition*. Fischer may attempt to discredit my position through a *reductio ad absurdum* (including trotting out Ardrey, whose stand I explicitly reject), but I equally reject environmental and biological dogmatism.

The issue of the relationship between crowding and aggression is still an open one. It actually has more to do with *spacing* than with territoriality, two separable phenomena. I mentioned crowding only in passing, and in a form which could hardly be more tentative and cautious (1974:786). Many species show great flexibility in the size of their territories, depending on environmental conditions, while remaining consistently territorial. The relationship between spacing, aggression and territorialty is obviously a complex one, probably mediated by resource competition, itself the product of a multiplicity of ecological factors. There is, however, definite evidence of a relationship between crowding and aggressive behavior in some species, e.g., rats (Calhoun, 1962).

This leaves the question of the evolutionary argument for human territoriality. I do claim that human territoriality both prevents and provokes aggression, because we have numerous instances of both. That is not a theoretical argument; it is an empirical statement. As such, it does not have to be put into a "logical order." As we know nothing of the evolution of territorial behavior in humans, we can only speculate about it. This I do only in passing and I clearly label my remarks as speculative. Fischer's criticism on this point is simply beside the point. I do not attempt an evolutionary explanation of territoriality.

In contrast to Fischer's comments which merited a reply of considerable length, Moberg's do not. He so consistently misreads me that one must question either his intelligence or his honesty. For instance, he takes me to task for not stressing "the inexorable interrelationship between acquired and inborn factors," when this was in fact a central theme of my article pp. 777, 778, 782, 786. Elsewhere, he interprets me as explaining human territoriality through the gibbon analogy and human hierarchy through the baboon analogy. I did nothing of the sort. I merely suggested, as an empirical generalization, the seemingly inverse relationship between territoriality and hierarchy among primates and made the case for an *unusually* high degree of *both* in *Homo sapiens* (pp. 784, 785). The "sordidness" of my "evolutionary tale" seems to be in the eyes of the reader. After reading Moberg's remarks,

the temptation to "pithecomorphize" is indeed strong.

With Mazur we are happily returning to the realm of serious intellectual discourse. His basic point on "the lack of an accepted paradigm for comparative primate sociology" is well taken, as our disagreement neatly illustrates. The problem is, alas, as grave in comparative *human* sociology, as witnessed, for instance, by interminable arguments on comparative rates of social mobility between societies, or comparative power of men and women across cultures, or a number of other hot topics. The more a subject matter is ideologically laden, the greater the disagreement is on interpreting the facts. This is amusing, but unfortunate for those who believe that actual behavior rather than ideology should settle empirical questions. Mazur and I are clearly in the same camp here.

Although I can see no compelling argument for excluding organized, premeditated, collective violence from the definition of human aggression, I do agree with Mazur that this type of aggression has been carried to much greater lengths in our species than among any other vertebrates and, thus, that this very special kind of aggression is not easily comparable cross-specifically. That we should e nasty in uniquely human ways may mplicate the construction of cross-specific dices of aggression, but it does not make us s aggressive. However, I am prepared to gue with Mazur on his chosen ground of ce-to-face aggression.

On the simian side, Mazur's Exhibit A is e famous Hamadryas battle of London Zoo. aboons in general and Hamadryas baboons in articular are indeed among the more ggressive primates: there is a lot of fighting for dominance between males; fighting between troops sometimes breaks out when their paths cross; Hamadryas males control the movements of their bonded females by neck bites, and so on (Kummer, 1968; 1971). The massive killings at London Zoo reported by Zuckerman, however, were clearly the result of completely unnatural conditions of spacing, confinement and sex ratio, and are about as representative of normal Hamadryas behavior as the behavior of, say, ten men and four women confined together in a prison courtyard would be typical of human behavior. The killings resulted much more from Zuckerman's ignorance of Hamadryas behavior and social organization than from the aggressiveness of his charges. Non-human mammals under wild conditions occasionally kill each other, but these killings have a much more accidental character than many human killings do: that is, killing among non-human mammals does not seem to be a regular feature of certain types of ritualized violent encounters as is the case with certain forms of human ritualized, violent encounters (such as dueling). Rates of lethal violence probably vary greatly from species to species. We do not have good enough statistics to settle the argument, but I would still put my money on *Homo sapiens* to win that contest.

Mazur's Exhibit B is a survey of the *reported recollection* of violent episodes by Americans. The two italicized words cast a compounded doubt on the validity of the findings. (Who knows what incidence of violent encounters baboons would report, a crucial test for Mazur who is concerned with the cross-specific comparability of the data!) I find the results of that study difficult to accept at face value. That nearly three-fourths of Americans have never been physically disciplined by their parents, have never had a violent tustle in a contact sport, have never had a fist fight on a school playground, unendurably stretches my credulity.

Admittedly, my 42 years of experience are a few more than Mazur's 35, and the following evidence is going to be dismissed by Fischer, Moberg et al. as "anecdotal." However, my sample of eye-witness observations of human aggression includes some ten or twelve clashes between policemen and civilians in places as far distant as Peru, South Africa, Nigeria, Kenya, the Congo, Belgium, France and the United States, including clubbing, stone-throwing, tear-gassing and the fatal shooting of two people; about eight or nine periods ranging from a few minutes to a couple of hours under aerial strafing and bombardment in France and Belgium; a ritual battle in Peru in which a dozen participants were wounded, one fatally so, by sling-shot propelled stones, and the sight of a dead soldier just shot with a 45-caliber pistol during a peacetime guardroom brawl. In addition, I have witnessed hundreds of such trivially routine acts of physical aggression as the physical disciplining of children by adults, violent disputes between children (including almost daily fights between my sons), angry exchanges between players and spectators during soccer, football and other games, soldiers' brawls in bars, and the like.

Besides my direct observations, a considerable number of people in my immediate circle of kinsmen and friends have been the victims of human aggression. A graduate student in my department was recently shot dead in his Seattle living room. The parents of a close friend were murdered in their bed by teenage

burglars in New York. The brother of a friend and colleague was assassinated in South Africa. Relatives of several friends died in Nazi concentration camps. A cousin of mine was slowly tortured to death in the Congo. My grandfather narrowly escaped being shot as a hostage of the Germans in Paris.

Like Candide, Mazur may reassure us that things could be much worse and suggest that we thank the gods for belonging to such an amiable, peace-loving species. All things considered, I would rather take my chances among the baboons of London Zoo than among the inmates and guards of Attica State Prison, Dachau or the Gulag Archipelago. So, I think, would Mazur.

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CONTINGENT CONSISTENCY AND THE ATTITUDE-BEHAVIOR RELATIONSHIP: A COMMENT*

(COMMENT ON ACOCK & DEFLEUR, ASR DECEMBER, 1972)

Acock and DeFleur's article "A Configurational Approach to Contingent Consistency in the Attitude-Behavior Relationship," examines the effects of social influence variables on the relationship between attitude and behavior. The essence of the article is captured in their statement: "The configurational model is premised on the assumption that interaction between variables (i.e., configurations) will substantially increase the ability of the variable complex to predict behavior over and above the variables operating separately" (p. 716). The present paper explicates the conditions under which a statistical interaction model is theoretically meaningful and examines inadequacies in the (Acock and DeFleur) research which cast doubt on their claim to have substantiated such a model.

CONDITIONS FOR MEANINGFUL INTERACTION MODELS

A model which allows for the effect of attitude on behavior to be contingent upon the levels of other relevant variables is theoretically promising. Few researchers have explicitly incorporated this notion into their research designs and statistical analyses. (See Liska, 1974, for a review.) A specific substantive interaction model must delineate which interactions among variables are expected and what form and direction they will take, if it is to generate useful (falsifiable) research hypotheses. The article by Acock and DeFleur fails to outline a theory or to specify interactions.

First, contingent consistency "theory," as posited by the authors, simply involves a restatement of the definition of statistical interaction (i.e., the relationship between attitude and behavior is modified, depending on the level of some other variables—in this case two social influence variables). There is no attempt to spell out the processes that underly the proposed modifying effects of the social influence variables.

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Second, references by Acock and DeFleur to general field theoretic notions are insufficient to generate a set of refutable hypotheses. The authors (p. 721) broadly postulate that "The independent effect of the configurations is positive." Any number of potential outcomes could be considered "support" for this hypothesis. The authors' failure to specify clear cut hypotheses has consequences in their subsequent analyses and interpretations. Although Acock and DeFleur *estimate* all possible interactions among independent variables, they do not *examine* all of the interactions, and in several places, they state that they have no expectations regarding certain estimated interactions (pp. 723-4). But the expectations they do have are never made explicit.

Besides these limitations at the theoretical level, the analysis employed by Acock and DeFleur is inappropriate and misleading. We [illegible]t address this issue and conduct a more [illegible]ative analysis of their data.

METHODOLOGICAL PROBLEMS

Although the major thrust of their article is [illegible]at variables interact to produce effects over [illegible]d above simple main effects, Acock and [illegible]Fleur do not direct a single empirical [illegible]parison to this assertion. One purpose of [illegible] paper is to provide a set of comparisons [illegible]ch do address this issue. Before presenting [illegible]efforts to this end, it is necessary to recall [illegible]e details of the original article.

[illegible]cock and DeFleur examine the effects of [illegible]e variables on voting behavior in a mock [illegible]tion concerning legalization of marijuana. [illegible] independent variables were: attitude [illegible]chotomized as for or against legalization); [illegible]rceived family position (dichotomized as [illegible]t against or against legalization); and per[illegible]eived peer position (trichotomized as for, [illegible]eutral or against legalization). The independent variables were scored by "dummy variable coding" (Gujarati, 1970); interaction terms were formed by multiplication of the independent variable vectors. Acock and DeFleur then estimate the parameters of equation (1) below.

$$Z = b_0 + b_1X_1 + b_2X_2 + b_3X_3 + b_4X_4 + b_5X_5 + b_6X_6 + b_7X_7 + b_8X_8 + b_9X_9 + b_{10}X_{10} + b_{11}X_{11} \quad (1)$$

where:

Z is the conditional probability of voting for marijuana;

b_i's are unstandardized regression coefficients;

X_1 = 1, if the subject has a favorable attitude toward legalization;
= 0, if otherwise;

X_2 = 1, if the subject does not perceive his family as opposed;
= 0, if otherwise;

X_3 = 1, if the subject perceives his peers as favorable;
= 0, if otherwise;

X_4 = 1, if the subject perceives his peers as neutral;
= 0, if otherwise;

$X_5 = X_1X_2$, $X_6 = X_1X_3$, $X_7 = X_1X_4$, $X_8 = X_2X_3$, $X_9 = X_2X_4$;

$X_{10} = X_1X_2X_3$, $X_{11} = X_1X_2X_4$.

Acock and DeFleur report that equation (1) explains 7.9% more of the variance in voting behavior than does attitude (X_1) alone. This increment is construed by the authors to show strong evidence for the efficacy of the configurational model. As Liska (1974) has noted, however, this comparison does not provide evidence for such a conclusion. Clearly, the extra predictive power of equation (1) may be due solely to the additive effects of the social influence variables and not to the interaction terms at all.

Another mode of analysis used by Acock and DeFleur involves generating a series of predicted cell proportions, based upon equation (1). These proportions should be exactly equal to the observed cell proportions, since equation (1) is a saturated model. (See footnote 2 for a more complete discussion of this issue.) While examining the entries of a cross tabulation may be interesting, it does not speak to the issue of whether an additive or an interactive model better explains the data.

Finally, in interpreting their results, Acock and DeFleur discuss their estimated regression coefficients as if they were estimates of effects. Dummy variable coefficients should not be interpreted as effects in the usual sense; indeed, the size of the coefficients is dependent upon the omitted category (Cohen, 1968). Effect parameters for such data would be better estimated by effect coding (Kerlinger and Pedhazur, 1973) or a weighted means decomposition (Melichar, 1965) or the various schemes proposed for the log linear framework (Goodman, 1972).[1]

[1] Log linear models would seem to be the presently preferred mode of analysis for data such as Acock and DeFleur's. We performed such an analysis and reached substantive conclusions equivalent to our model 2 discussed below.

Table 1. Generated Data Set - Acock and DeFleur (1972) and Cell Percentages Predicted from Equation 2

Independent Variables*			Number Voting		Percent Voting	Predicted Percentage Equation 2
Own Attitude	Perceived Family Position	Perceived Peer Position	Yes	No	Yes	
+	+	+	16	1	94.1	83.5
+	+	n	2	3	40.0	64.4
+	+	-	1	2	33.3	53.3
+	-	+	37	8	82.2	83.5
+	-	n	13	4	76.5	64.4
+	-	-	6	8	42.9	53.3
-	+	+	0	1	0	47.0
-	+	n	0	4	0	2[illegible]
-	+	-	0	1	0	16.[illegible]
-	-	+	3	5	37.5	47.0
-	-	n	5	12	29.4	27.8
-	-	-	14	56	20.0	16.8

* +: favorable

-: unfavorable

n: neutral

None of the various analyses performed by Acock and DeFleur directly address the question of the superiority of the interactive model over an additive or main effects model. Our efforts to produce such a comparison were hampered by the fact that Acock and DeFleur report only the correlations among the independent variables in equation (1). Any reanalysis requires the correlations with the dependent variables as well. To this end, we regenerated Acock and DeFleur's data matrix from the regression coefficients and the frequency base information in their Table 1 (p. 722). The regeneration produced the data matrix in our Table 1.[2]

Table 1 served as the basis for the analy[illegible] that follows. The procedure we adopted is [illegible] incremental R^2 approach outlined in K[illegible]linger and Pedhazur (1973:179-93). In situ[illegible]tions where there are unequal numbers o[illegible] observations in the cells of a design, the independent variables are correlated both with each other and with the derived interaction terms. Under such conditions, some portion of the variation accounted for by a regression equation cannot be uniquely attributed to any of the variables in the equation, but is instead considered to be variation due to the joint effects of the independent variables (Duncan, 1970; Finney, 1972; Alwin and Hauser,

[2]The regeneration was necessary because the original data were not available to us. Professor Acock has affirmed that our regeneration differs from his own regeneration by rounding errors. Regardless of whether we use the data he regenerated or our own, the analyses yield equivalent substantive interpretations. The regeneration proceeded as follows.

The frequency base data in Table 1 (p. 722) were used to create a joint frequency distribution among the independent variables. Next, we generated a table of observed cell proportions from the published regression weights. These were, in turn, multiplied by the corresponding frequencies from the first step to produce a data matrix. A crucial step in this procedure is the generation of the observed

Table 2. Increments to R^2 for Various Effects in Acock and DeFleur[1]

	Source	Formula[2] for Increment	(Observed Increment)	F
Main Effects	Attitude	$R^2_{Z.1234}-R^2_{Z.234}$	(.088)	26.12*
	Family Position	$R^2_{Z.1234}-R^2_{Z.134}$	(.003)	<1.0
	Peer Position	$R^2_{Z.1234}-R^2_{Z.12}$	(.048)	7.06*
Two-Way Interaction Effects		$R^2_{Z.1-9}-R^2_{Z.1234}$	(.028)	1.65
Three-Way Interaction Effects		$R^2_{Z.1-11}-R^2_{Z.1-9}$	(.004)	<1.0

*p<.05

[1]The incremental R^2 procedure and formula for the F tests may be found in Cohen (1966:435) or Kerlinger and Pedhazur (1974:179-193).

[2]Subscripts refer to variable numbers as specified in equation (1).

[illegible] deal with this problem, variables [illegible]signed to blocks which enter the [illegible] equation in a stepwise procedure. [illegible] common block-ordering strategy for [illegible]uch as Acock and DeFleur's involves [illegible]tering main effects first, followed by two-[illegible]y interactions and then the higher-order [illegible]eractions in ascending order. Within each [illegible]ck of variables, the variance can be further [illegible]rtitioned into unique and joint effects of [illegible]e variables. Our analysis of the Acock and DeFleur data utilizes this logic and appears in Table 2.

The results of our analysis suggest that, for the given partitioning of the variance, the substantive conclusions to be reached from Acock and DeFleur's data are as follows:[3] (1) own attitude and perceived peer position each have independent effects on the dependent variable, with most of the main effects variation due to the joint effects of the independent variables; (2) there is no evidence in the present data for the efficacy of an interactive model; (3) the analysis suggests that a simple additive model containing own attitude and perceived peer position would be the most parsimonious model that adequately accounts for the behavior.[4] Equation (2) below presents the regression estimates for such a model.

[illegible]ll proportions. Equation (1) is a full regression [illegible]odel wherein all possible variable interactions have [illegible]een included. A necessary feature of such a model (one that uses all available degrees of freedom) is that the predicted cell means must equal observed cell means. (In the case of a dichotomous dependent variable, cell means are equivalent to conditional proportions or odds.) In several instances, the Acock and DeFleur regression weights yielded estimated cell means that were unreasonable. For example, subjects whose own attitude was negative, but who perceived their family and peers as positive, had a predicted cell proportion of −.035, an impossible value. Other estimated proportions, when applied to the corresponding cell frequencies might suggest that 4.3 persons in a particular cell had voted for legalization. In all such cases, we rounded to the nearest whole person. We are aware of no mathematical or statistical reasons for these problems. Neither our regeneration nor Acock's exactly reproduce the published correlation or regression coefficients. Our estimates for equation (1) were: $Z = .200 + .229X_1 - .200X_2 + .175X_3 + .094X_4 + .104X_5 + .219X_6 + .242X_7 - .175X_8 - .094X_9 + .389X_{10} - .175X_{11}$. R^2 for this equation is .356.

[3]Our interpretation of the data is based upon substantive rather than statistical significance; Acock and DeFleur also recognize the limitations of statistical inference for this study in their footnote 5.

[4]Each of the two-way (X_6 – X_9) and three-way (X_{10} and X_{11}) interaction terms were examined for significant effects over and above an additive model containing own attitude and perceived peer position. Only X_9, the two-way interaction between family positive and peers neutral, showed a statistically significant effect ($p < .05$). Considering the large number of tests performed, the small sample size for this configuration ($n = 5 + 4 = 9$) and the lack of any theoretically meaningful specification for such an effect, it seems prudent to attribute this result to chance pending further research.

$$Z = .168 + .365X_1 + .302X_3 + .110X_4. \quad (2)$$

The cell percentages predicted by this model appear in column 4 of Table 1. The model does quite well at predicting the percentages in cells having large numbers of observations; larger errors are encountered for cells with small numbers of observations.

The reader will note that we modified our interpretation of Table 2 with the phrase, "for the given partitioning of the variance." Although the strategy employed corresponds to general sociological practice, there is clearly nothing which demands this approach. If a researcher has good theoretical or empirical reasons for believing that interaction effects are more important than main effects, the interaction terms can be entered into the regression equation before main effects. While most sociologists enter main effects first (in the name of parsimony), it is well known that, given certain patterns of significant variable interaction, main effects must be interpreted either with great care or not at all (Campbell and Stanley, 1966:29); this may argue for entering the interaction terms first. It is our firm belief that this issue belongs to the class of issues dealing with "specification errors" and that entering main effects first should be done on theoretical grounds rather than tradition.

When we performed an analysis that entered interaction terms ($X_5 - X_{11}$) into the regression equation prior to entering main effects terms ($X_1 - X_4$), we found that terms $X_1 - X_4$ as a group explained only an additional 1.7% of the variance in voting behavior. This exercise highlights a problem in using data collected by usual sampling procedures to demonstrate the efficacy of an interactive model over a main effects model. When interaction terms are highly correlated with main effects, whichever terms enter the regression equation first will be credited with all of the explained variation the variables share. These problems do not occur in experiments because experiments are generally designed to have equal numbers of observations in each cell, yielding all main effects and interaction terms uncorrelated. Sociologists could design their studies to yield equal cell frequencies; this would necessitate oversampling some segments of a population and undersampling others. Such a strategy allows the researcher to address theoretical issues rather than predictive/practical issues. Usual sampling procedures can lead the researcher to miss theoretically important, but empirically infrequent, interactions.

In summary, although the notion that the relationship between attitude and behavior depends on the levels of other relevant variables is quite appealing to us, we find no theoretical or empirical support for this proposition in the article by Acock and DeFleur. Elaboration of the configurational approach awaits further theoretical development and the use of methodologies more suitable for the examination of interactions.

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REPLY TO SUSMILCH, ELLIOTT AND SCHWARTZ*

It is annoying to be criticized for failing to fulfill claims which one has not made. Susmilch and his associates apparently did not read our closing paragraph in which we (Acock and DeFleur, 1972:726) state openly that: "Overall, the present research should be regarded as exploratory and its conclusions tenative." In view of this rather clear portrayal of the modest status of our work, we were surprised to see our article evaluated as though we had claimed to be presenting a rigorous theory that was purported to be fully "substantiated" by our data. And frankly, in spite of our critics' statistical exercises, we continue to feel that our approach to the study of attitude-behavior relationships within a contingent consistency framework holds considerable promise for further research.

We have more detailed objections to the ks of the critics. They show a lack of anding of the difference between the gent consistency model as a theoretical ation and a formal definition of tical interaction. Thus, they confuse retical issues with statistical issues. We review each briefly.

eoretical Criticisms

First, they contend that the contingent nsistency model "simply involves a restate- nt of the definition of statistical interac- n. . . ." While space does not permit an aboration of the contingent consistency odel, two points are in order: (a) we point t that the joint effects of substantive ariables is the key point of the article and (b) tatistical interaction appears in only one of our five hypotheses. We stated:

> Thus, the key is neither a person's attitude, *nor* such social influences as his perception of the opinions of certain reference groups, when these variables are allowed to operate separately, but the joint operation of such variables. The joint ation may be expressed by adding the dent effects and their interactions (if e variables operating simultaneously.

Focus of the contingent consis- entation involves the combination of attitudes with situational considerations. Our article utilizes this viewpoint, as indicated in its title, and adds the notion that in *may* add even further to clear predicti

The second theoretical criticism is do not have a set of refutable hy because any number of potential c could be considered supportive. How examine each individual regression co and indicate whether it provides port," "some support" or "strong for the corresponding hypothesi hypothesis is tied to one or more terms in the equation. Thus, even tho feel our expectations are not made we felt they were completely obvi model states rather simply that perce support and/or family support wh bined with attitude will have a increment on the conditional proba behaving according to the attitude.

Statistical Criticisms

Their major statistical criticism is interactions do not add substantially opposed to a more simplified model the interactions do not have a su unique contribution to the explaine tion. We maintain that these crit inappropriate to our data, particula sidering the radically unequal n's. T diffusion of multiple regression techn the social sciences in the last fi (mainly from econometrics) has di the standardized procedures they u dealing with drastically unequal n's a configurations. One clear statement problem was made five years ago by Watts (1970:234).

> Such a standardization is of interest o feels that the entire joint distrib regressors is both fixed in the popul well represented by the sample.

This may be illustrated by a single from our research. The interaction te favorable attitude toward legaliza marijuana combined with perceivi peers and family as also favorable unstandardized regression coefficie effected by unequal n's) of .351. Th the conditional probability for vo legalization of marijuana to .941. Wit interaction terms, the more "parsin model in their equation (2) r prediction of .835 for the same confi of variables. Substantively, adding the tion term in our model we move fro prediction to a virtually perfect predi

It is not difficult to reconcile sucl

*We wish to thank James R. Lincoln for his comments. Also, we wish to thank Mitsuyo Hanada

interaction terms adds only 3.2% (i.e., .028 + .004) to the overall explained variation for the entire sample. Simply, the interaction applied only to those few subjects who fit into the configuration. While the interaction is important to predicting their behavior, it does not (nor should it) account for the total variation of the entire sample. The increment to R^2 and unique variance approaches they utilize are hopelessly confounded with the empirically observed frequencies for each configuration when our data were collected in 1968 at a rural university. If the data were gathered today and the same law were applied (measured by our unstandardized regression coefficients), the R^2 increment and unique component would probably be much larger. This would be because more students today would probably perceive friends and family as favoring the legalization of marijuana. Thus, the criteria of our critics are confounded with the empirically observed frequencies in our 1968 sample and are in no way theoretically relevant to the value of the contingent consistency framework.

Although we are entirely opposed to their use of unique variance as a criterion for the reasons mentioned (and covered more extensively in the econometrics literature), one method of standardization is less plagued by the order of entry and the other problems mentioned, namely, beta weights.

> The use of beta coefficients (standardized only for variance) is subject to the same sort of criticism—they retain their meaning only so long as there is no intervention by man or nature to change the variances used for standardization. But where β_i is only crippled as a guide to policy, ϕ_i is totally disabled. (Cain and Watts, 1970:234)

The main problem with the beta weights is that the terms which show little variation in a particular sample (which applies primarily to interactions because of drastically unequal n's in our sample) are attenuated without regard to the variation they would have in other samples and at other times. In spite of these "crippling" problems, the beta weights for the interactions are reasonably large in comparison to those for the main effects. For the complete model with variables X_1 to X_{11} (X_5 to X_{11} representing interactions), the coefficients are .225, -.173, .163, .057, .095, .216, .166, -.076, -.005, .193 and .080, respectively. Since attitude was expected to play a central role, it is not surprising that it has the largest beta weight. More relevantly, the interactions contain the second, third and fifth largest weights. When one considers that these are attenuated by the relatively low empirical frequencies they represent, based on the data as opposed to a replication today, they suggest that the interactions should not be entirely dismissed.

A second statistical criticism, which is at least implicit, is that our results are not statisticaly significant. They note in footnote 4 that they concur with us about the problems of statistical inference and say their criticism is "based upon substantive rather than statistical significance." However Table 2, which is used to justify equation (2) in their comment, does consider statistical significance. Several points about significance tests can be made.

First, the X_4 in their second equation is not statistically significant, having an F ratio of 1.85 by our computations, and it has no effect on the standard error of the estimate to three decimal places. Its increment to R^2 is .006. Also, they agree with us that tests of significance are very limited in this because of the extreme multicolinear therefore, one might simply put all t effects in as a block and then see interactions add to this result. If we en main effects and then dump in the ent of interactions including the three-way (X_{10} and X_{11}), we find the increment X_{10} interesting in several ways. X_{10} has unstandardized regression coefficient of .4 a beta weight of .231, an F ratio of 6. (with 1 and 196 degrees of freedom) and a 2.2% to the explained variation. This ef over and above the set of main effects is trivial. Specifically, the unstandardized coe cient indicates that those students who ha favorable attitude, who perceive their peer favorable and who perceive their families supportive are 41.5% more likely to vote f the legalization of marijuana than th predicted value based on the additiv components of these three variables. We fully appreciate the inferential limitations of our sample. However, in a descriptive sense at least, this is not a trivial effect.[1]

[1] They indicate that the main effects add only 1.7% if entered as a block after the interactions. Another alternative is to treat each variable as a "field." Some fields consist of single variables (main effects); some fields consist of two or more variables (interaction terms). While conventional practice enters interaction terms last, a "field" approach is consistent with treating all variables equally and using a stepwise solution without blocking. When this was done, attitude emerged first followed by four interaction terms (X_6, X_7, X_{11} and X_{10}). After this, the main effects added only 1% to the explained variation. We have included this note because it duplicates their statement. We did not

While these points respond to the implicit arguments of Susmilch et al. concerning tests of significance, we would like to stress that our own thinking about tests of significance has been modified over the last few years. When the study was designed in 1966-1967, there was little literature in the social sciences on multiple regression with interaction terms. Unfortunately, the design of the study was such that tests of significance lack power to ect substantively interesting results. If we at to repeat this study today, one of two es would be essential. Either we would ter much larger sample to obtain reliable opp s of the regression coefficients, or we not lect variables and operationalizations on t uld make the interactions less highly or d with the other variables. The first omi lternatives would have no effect on and ted value of the criteria Susmilch et Let e (which we consider irrelevant), but cate low sufficient power to provide a the ful test of significance. The second att h would also provide more power for at significance of the interaction terms a uld have a considerable increase in the ted values of the unique variance uted to the interactions.

A third criticism concerns our use of the m "effect" when we are using dummy as osed to effect coding. It is true, as they , that the size of the coefficient depends he omitted category. If there is no *a priori* heoretical ground for determining the tted category, dummy coding is limited effect coding may be more appropriate. us examine our case. Our omitted gory (the one serving as a baseline) was configuration consisting of an unfavorable itude, unfavorable perception of peer group titude and unfavorable perception of family titude. This configuration has the lowest robability of voting to legalize marijuana within the contingent consistency framework. This represents a theoretical and not an empirical criterion. The remaining vectors were coded in such a way that they tied into specific hypotheses. This is carefully discussed our article (Acock and DeFleur, 72:719–20, 722–4). We feel strongly that y coding rather than effect coding was ppropriate for our model. Further- hough they criticize our use of the l to give a single example of how we misinterpreted any regression coefficient in the equation.

A number of other points are made in their paper which we consider less relevant. However, one of these deserves comment. Our critics indicate that their efforts "were hampered by the fact that Acock and DeFleur reported only correlations among the independent variables. . . ." Also, in footnote 3 they say the original data were not available to them.

Such comments are unnecessary. The correlation matrix was added to the article at the request of a reviewer who was concerned about multicolinearity (a legitimate concern) and was directly responsive to this concern. Most articles using multiple regression do not provide this much information although such a procedure might be considered as a new policy.

It is true that they were not provided the original data in the sense of a deck of IBM cards. However, all their analysis required is a set of correlations and the means and standard deviations of each variable. We provided them with the correlations we had as well as the means and standard deviations. Moreover, the complete regression equation appears in the article and this can generate the original data deck subject to rounding error.

Conclusions

Several conclusions are in order. First, the criteria used by our critics for evaluating our model in terms of unique variances are inappropriate. This is the most conservative and least appropriate procedure for evaluating interaction when multicolinearity is present. Second, their theoretical criticisms are based on a misunderstanding of the contingent consistency model. Third, apart from their criticisms, future research using our model must be designed to have sufficient power to overcome the extreme multicolinearity inherent in the framework. This third point, which Susmilch et al. do not develop, is the original article's major limitation and it requires that the work be viewed as even more exploratory than we suggested originally.

Finally, we feel that the contingent consistency model is well maintained in spite of their challenge to statistical interaction. We indicated earlier that the joint operation of attitude with situational variables whether viewed additively or multiplicatively was the key to successful prediction of behavior. Whether our complete model, their equation (2) or a combination of the main effects and a single three-way interaction term is used, the *combination* of attitude with situational

al article because we were not variables and we lacked a afford a powerful test of

variables leads to good predictions of behavior. Without this combination, the predictions based on either attitude *alone* or on situational factors *alone* are ambiguous.

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Washington State University

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ITEMS *(Continued)*

Generation-II" project. **HARRY P. TRAVIS** is a doctoral candidate whose dissertation is concerned with the stratification of women. Other research interests are mortality differentials, status effects on fertility and log-linear models. **JOHN N. KOFFEL**, a former University of Wisconsin graduate student, independently pursues interests in stratification and the sociology of learning.

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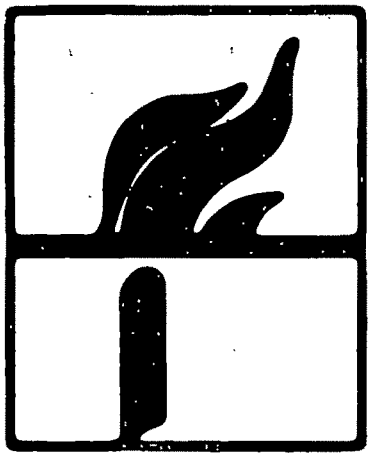

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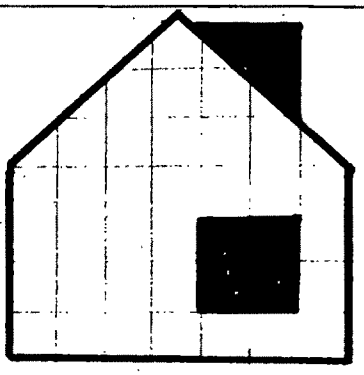

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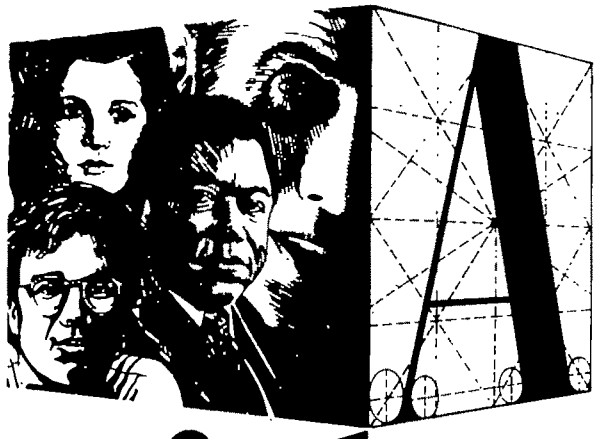

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AMERICAN SOCIOLOGICAL REVIEW

Official Journal of the American Sociological Association

DECEMBER, 1975 VOLUME 40 NUMBER 6

ARTICLES

Presidential Address: Two Methods in Search of a Substance ..... LEWIS A. COSER 691

Social Structural and Psychological Antecedents of Self-Estrangement and Powerlessness ..... LUTHER B. OTTO AND DAVID L. FEATHERMAN 701

The Effects of International Economic Dependence on Development and Inequality: A Cross-National Study ..... CHRISTOPHER CHASE-DUNN 720

Cultural Conceptions, Mental Disorders and Social Roles: A Comparison of Germany and America ..... J. MARSHALL TOWNSEND 739

Socioeconomic Status and Criminal Sentencing: An Empirical Assessment of a Conflict Proposition ..... THEODORE G. CHIRICOS AND GORDON P. WALDO 753

Components of Sustenance Organization and Nonmetropolitan Population Change: A Human Ecological Investigation ..... W. PARKER FRISBIE AND DUDLEY L. POSTON, JR. 773

Socioeconomic Status and Religious Participation ..... CHARLES W. MUELLER AND WELDON T. JOHNSON 785

Race and Socioeconomic Segregation ..... BRIGITTE MACH ERBE 801

A Structural Control Model of Organizational Change ..... NORMAN P. HUMMON, PATRICK DOREIAN AND KLAUS TEUTER 813

COMMENTS AND REPLIES

Comment on Allison and Stewart (ASR August, 1974)

Productivity among Scientists: A Replication and Elaboration ..... MICHAEL A. FAIA 825

Reply to Faia ..... PAUL D. ALLISON AND JOHN A. STEWART 829

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Comment on Ball-Rokeach (ASR December, 1973)
Is There a Subculture of Violence? ..... STEPHEN MAGURA 831
Reply to Magura ..... SANDRA J. BALL-ROKEACH 836
Comment on Burgess and Neilsen (ASR June, 1974)
"An Experimental Analysis of Some Structural Determinants of Equitable and Inequitable Exchange" ..... FRANK NEWPORT 838
Reply to Newport ..... ROBERT L. BURGESS AND JOYCE M. NIELSEN 843
Comment on Long (ASR February, 1974)
An Untested Hypothesis: The Effect of Size of Public Assistance Benefits on Migration ..... WILLIAM W. PHILIBER AND ROBERT SEUFERT 845
Reply to Philliber and Seufert ..... LARRY H. LONG 847

ERRATUM 850
EDITORIAL CONSULTANTS 851
INDEX TO VOLUME 40 854

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December, 1975

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(continued on 849)

AMERICAN SOCIOLOGICAL REVIEW

DECEMBER, 1975 VOLUME 40 NO. 6

PRESIDENTIAL ADDRESS: TWO METHODS IN SEARCH OF A SUBSTANCE*

LEWIS A. COSER

State University of New York at Stony Brook

American Sociological Review 1975, Vol. 40 (December):691-700

In tune with the overall theme of these meetings, this Presidential Address is an exercise in the uses of controversy. I am perturbed about present developments in American sociology which seem to foster the growth of both narrow, routine activities, and of sect-like, esoteric ruminations. While on the surface these two trends are dissimilar, together they are an expression of crisis and fatigue within the discipline and its theoretical underpinnings. I shall eschew statesmanlike weighing of the pros and cons of the issues to be considered and shall attempt instead to express bluntly certain of my misgivings and alarms about these recent trends in our common enterprise; let the chips fall where they may.

Building on other students of science, Diane Crane (1972) has argued that scientific disciplines typically go through various stages of growth accompanied by a series of changes in the characteristics of scientific knowledge and of the scientific community involved in the study of the area. In stage one, important discoveries provide models for future work and attract new enthusiastic scientists. In the next stage, a few highly productive scientists recruit and train students, set priorities for research and maintain informal contacts with one another. All this leads to rapid growth in both membership and publications. But in later stages the seminal ideas become exhausted and the original theories no longer seem sufficient. At this point a gradual decline in both membership and publication sets in, and those who remain develop increasingly narrow, specialized, though often methodologically highly refined, interests. Unless fresh theoretical leads are produced at this point to inspire new growth, the field gradually declines.

Such stages of growth and decline are, of course, not limited to the sciences. In other spheres of culture, religion and the arts for example, similar phenomena have been observed (cf. Thomas O'Dea, 1966; Max Weber 1963; Alfred Kroeber, 1957). One need only think of the creative effervescence in the communities of Christ's immediate disciples and their direct successors in contrast to quotidian routines and ritualized devotions of the later stages in what had now become the Church of Rome. Or consider the art of Byzantine icon painting where, after the early creative period, the same motives, even techniques, were endlessly repeated so that it takes a specialist to distinguish between paintings executed not just decades but even centuries apart. In religion and the arts, however, innovation is not a necessary condition for flowering and appeal, but in the sciences, when no innovation is forthcoming *rigor mortis* is not far away.

The findings of Crane and others in the sociology of science typically refer not to a whole branch of knowledge but only to sub-fields within such branches. It would therefore be wrong to apply these findings to sociology as a whole, composed as it is of a wide variety of sub-areas each with its own pattern and rhythm of growth. Yet permit me nevertheless roughly, and per-

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haps rashly, to sketch what I consider the present condition of sociology as a whole.

By and large, we are still in the second stage of growth, the stage of lively development, of creative ability, and innovative effervescence. Yet there now appear a number of danger signs suggesting that the fat years of the past may be followed by lean years, by years of normal science with a vengeance, in which not only the mediocre minds but even the minds of the best are hitched to quotidian endeavors and routine activities. This seems portended by the recent insistence among many sociologists on the primacy of precise measurement over substantive issues.

The germ of the idea for this address came to me earlier this year when a friend of mine, the editor of a major sociological journal, explained with some pride that, no matter what the substantive merits of the paper might be, he would refuse to accept contributions using old-fashioned tabular methods rather than modern techniques of regression and path analysis. I gather, for I have respect for his opinion, that he meant that he would not accept articles requiring modern methods of data analysis that do not make use of such techniques. Yet, though his intentions are undoubtedly excellent, I submit that such an orientation is likely to have a dynamic of its own and that, inadvertently perhaps, it will lead to a situation where the methodological tail wags the substantive dog, where as Robert Bierstedt (1974:316) once put it, methods would be considered the independent and substantive issues the dependent variable.

My friend's voice is, of course, not a lonely one. In fact, he expressed what is tacitly assumed or openly asserted by a growing number of our colleagues. Fascinated by new tools of research, such as computers, that have come to be available in the last decades, and spellbound by the apparently irresistible appeal of techniques that allow measures of a precision hitherto unattainable, many of our colleagues are in danger of forgetting that measurements are, after all, but a means toward better analysis and explanation. If concepts and theoretical notions are weak, no measurement, however precise, will advance an explanatory science.

The fallacy of misplaced precision consists in believing that one can compensate for theoretical weakness by methodological strength. Concern with precision in measurement before theoretical clarification of what is worth measuring and what is not, and before one clearly knows what one is measuring, is a roadblock to progress in sociological analysis. Too many enthusiastic researchers seem to be in the same situation as Saint Augustine when he wrote, on the concept of time, "For so it is Oh Lord, My God, I measure it but what it is I measure I do not know" (Saint Augustine 1953:35).[1]

No doubt, modern methods of research have immeasurably advanced sociological inquiry. Only sociological Luddites would argue that computers be smashed and path diagrams outlawed. What I am concerned with is not the uses but rather the abuses of these instruments of research. They serve us well in certain areas of inquiry, but they can become Frankenstein monsters when they are applied indiscriminately and, above all, when their availability dictates the problem choices of the investigator so that trivial problems are treated with the utmost refinement.

The sheer availability of new methods encourages their use and seems to release the user from the obligation to decide whether his problem or findings are worthy of attention. By way of illustrating this let me quote from the summary of a recent paper by Oksanen and Spencer (1975) in one of the official journals of our Association, *The American Sociologist*: "A rather large degree of explanatory power has been achieved by our regression model, in terms of overall goodness of fit and in terms of significant variables. It is of considerable interest to learn that high school performance is an invariably significant indicator of 'success' in the [college] courses examined." Abraham Kaplan's (1964:28) delightful formulation of the Law of the Instrument comes to mind here: "Give a

[1] In fairness to St. Augustine, modern physics tends to agree with his position. "Time is a primitive element in the logical structure of physics," state W. H. Cannon and O. G. Jensen (1975), "consequently physics does not explicitly define time but rather specifies operational procedures for its measurement in units of seconds."

small boy a hammer, and he will find that everything he encounters needs pounding."

The fact is that, though in principle these new methods and technologies could help us achieve greater theoretical sophistication, they are used as "magic helpers," as a shortcut to, or even replacement for, theoretical analysis rather than as a means for furthering it. An insistence on the use of these refined methods, no matter what, makes it fall prey to Kaplan's law.

It would be easy, and perhaps entertaining, to go on quoting similar instances, but each of us can easily supply other examples. Let me instead return to the serious problems now faced by our discipline, many of which have been created, or at least accentuated, by the revolution in methodology and research technology.

Our new methodological tools may well be adaptable to deal with a great variety of topics and problems, and I hope they are. However, the data needed for path or regression analyses are much harder to come by in some areas than in others, and in many of them it would take a great deal of sophistication to discover and handle useable indicators. Consequently; under the pressure to publish to avoid perishing, or to gain promotion, or simply to obtain the narcissistic gratification that comes from seeing one's name in print, it is more attractive to do what is quick and easy. This is so in every scholarly field and even in the healing arts. In psychiatry, for example, it leads to prescribing drugs instead of psychotherapy, often not as a result of deliberate choice between alternative diagnoses and prognoses, but simply because drug therapy is easy to administer and promises quick results, superficial though they may be. In the world of scholarships, moreover, not only the choice of technique but even the choice of the problem tends to be determined by what is quick and easy rather than by theoretical considerations or an evaluation of the importance of the questions that are raised. Moreover, the uses of a sophisticated technological and methodological apparatus gives assurance, but often deceptive assurance, to the researcher.

Sociology is not advanced enough solely to rely on precisely measured variables. Qualitative observations on a small universe can provide theoretical leads that may at a later stage become amenable to more refined statistical treatment. To refrain from using descriptive data because they may lend themselves only to tabular presentation will not only diminish our theoretical powers but will retard the refinement of statistical analysis as well.

Training the new generation of sociologists not to bother with problems about which data are hard to come by, and to concentrate on areas in which data can be easily gathered, will result, in the worst of cases, in the piling up of useless information and, in the best of cases, in a kind of tunnel vision in which some problems are explored exhaustively while others are not even perceived.

There is at least some evidence that we tend to produce young sociologists with superior research skills but with a trained incapacity to think in theoretically innovative ways. Much of our present way of training as well as our system of rewards for scientific contributions encourages our students to eschew the risks of theoretical work and to search instead for the security that comes with proceeding along a well traveled course, chartered though it may be by ever more refined instruments of navigation. J. E. McGrath and I. Altman (1966) have shown this in instructive detail for small-group research, but it applies in other areas as well.

Careers, especially those of people with modest ambitions, can be more easily advanced through quantity rather than quality of publication. This leads to an emphasis on methodological rigor, not on theoretical substance. One way to publish rapidly is to apply "the [same] procedure, task, or piece of equipment over and over, introducing new variables or slight modification of old variables, and thereby generate a host of studies rather quickly" (J. E. McGrath and I. Altman, 1966:87). The formulation of theories, moreover, is time consuming, and may not lend itself easily to publication in journals increasingly geared to publishing empirical research, and to reject "soft" theoretical papers. There exist, then, a number of factors in our present systems of training and of rewards that exercise pressures on incoming generations of sociologists to refine their methods at the expense of developing innovative lines.

This is not inherent in methods per se, but it is, let me emphasize again at the risk of

repetition, a temptation for lesser minds. And here as elsewhere inflation has set in. However, it is important to note that even the better minds, those who have been able to use the new methods innovatively, are *nolens volens* geared to deal with problems, important as they may be, for which these methods promise quick results. Even in the serious work that is being done with the help of the new statistical techniques there lurks the danger of one-sided emphasis.

Stratification studies of recent years will illustrate this point. This field has benefited a great deal from modern path analytical methods whose power is perhaps shown at its best in Blau and Duncan's *The American Occupational Structure* (1967). Path analysis allows these authors systematically to trace the impact of such factors as father's occupation, father's educational attainments, and son's education and first job on the son's placement in the occupational hierarchy. It allows for the first time the assessment in precise detail of the ways in which occupational status in a modern industrial society is based on a combination of achieved and ascribed characteristics. It permits, in fact, the assessment of the contributions of social inheritance and individual effort in the attainment of socioeconomic status.

Yet, to use an important distinction by John Goldthorpe (1972), this research contributes to the understanding of the *distributive,* not to the *relational* aspects of social class. The focus is predominantly on the impact on individual careers of differences in parental resources, access to educational institutions and the like, or they center attention upon individual characteristics of people variously placed in the class structure. There is no concern here with the ways in which differential class power and social advantage operate in predictable and routine ways, through specifiable social interactions between classes or interest groups, to give shape to determinate social structures and to create differential life chances. The first and only entry under path analysis in the 1966-1970 Index to *The American Sociological Review* (vols. 31-35) refers to a paper by Hodge and Treiman (1968) tracing the effects of the social participation of parents on that of their offspring. There were only two papers analyzing problems in social stratification with the aid of path analytical methods in the 1973 (vol. 38) volume of *The American Sociological Review,* and both (Kelley, 1973, and Jackman and Jackman, 1973) deal with the *distributional* aspects of social stratification or with the characteristics of individuals in the class hierarchy.

The 1974 volume of *The American Sociological Review* published three papers on stratification using path analysis, two of which (James N. Porter, 1974; Alexander and Eckland, 1974) deal again with distributional aspects. The *Rose Monograph Series* published by the A.S.A. had issued twelve titles up to the end of 1974. Of these, four, that is one-third, deal with problems of stratification and use highly sophisticated research methods. Their titles speak for themselves: "Socioeconomic Background and Educational Performance" (Robert M. Hauser, 1972); "Attitudes and Facilitation in the Attainment of Status" (Ruth M. Gasson, Archibald O. Haller and William Sewell, 1972); "Looking Ahead: Self-conceptions, Race and Family as Determinants of Adolescent Orientation to Achievement" (Chad Gordon, 1972); and "Ambition and Attainment: A Study of Four Samples of American Boys" (Alan C. Kerckhoff, 1974). It would appear as if authors and editors of the series are fixated on the problems of making it.

Yet a class system is not only a distributive system, in which individuals are assigned to their respective niches in terms of background and training, nor is its analytical significance exhausted by individual characteristics of people who make their way within it; it is also a system that is shaped by the interaction between various classes and interest groups differentially located within the social structure. It is a system, moreover, in which command and coercion play major parts. Classes and other socioeconomic groups use their resources so as effectively to maintain or advance their positions and to maximize the distribution of material and social benefits to their advantage. Exclusive concern with the distributive aspects of stratification directs attention away from the socio-political mechanisms through which members of different strata monopolize chances by reducing the chances of others. Max Weber (Gerth and Mills, 1947), building on Karl Marx, saw this with exemplary clarity when he stated that, "It is the most elemental economic fact that the way in which

disposition over material property is distributed among a plurality of people . . . in itself creates specific life chances. According to the law of marginal utility this mode of distribution excludes the non-owners from competing for highly valued goods. . . . It increases . . . the power [of the propertied] in price wars with those who, being propertyless, have nothing to offer but their labor. . . ."

One need not accept Marx's dichotomous scheme of class analysis in order to agree that classes are linked in asymmetrical relationships. The notion of a class of owners of means of production is dialectically bound to the notion of a class of non-owners. Just as in the classical Indian caste system, as Louis Dumont (1970) has shown, the purity of the Brahmans is inseparable from the impurity of the Untouchables, so the central characteristics of the class systems is not that there are propertied and the propertyless but that they are mutually interdependent. Randall Collins, arguing against a narrowly defined sociology of poverty, puts the matter well when he writes, "Why some people are poor is only one aspect of the same question as to why some people are rich: a generalized explanation of the distribution of wealth is called for if one is to have a testable explanation of either particular" (Collins, 1975:17).

A system of stratification consists in relationships between groups or categories of men and women which sustain, or alter their respective access to life chances. It is one thing to investigate the ways in which, for example, people manage to attain the status position of medical practitioners in American society, it is quite another to analyze the institutions that help the American Medical Association to monopolize the market for health care by restricting access. What needs analysis is not merely the ladder to medical success but those institutional factors that contribute to the maintenance of a system of medical service that effectively minimizes the life chances of the poor (Kelman, 1974).

Analysis of the distributional aspects of stratification systems can dispense with considerations of social and political power; concern with the relational aspects, however, directs attention to the power contentions that make for the relationships which establish differential class privileges, and create patterned conflicts between unequally benefited contenders. When no question is asked about who benefits from existing social and political arrangements, stratification research, no matter how sophisticated its methodological tools, presents a "bowdlerized" version of social reality. When the causes and consequences of differential location in the class structure remain unanalyzed, the whole area of research so brilliantly opened up by Robert K. Merton's (1968: chapters 6 and 7) seminal anomie paradigm remains unexplored.

I am not arguing, let this be clearly understood, that concern with the structures of power and exploitation is necessarily better than preoccupation with the pathways to individual mobility. There is surely a need for both types of studies. I believe, however, that the methodological tools that are available help focus on the latter. It must be added—lest I be accused of technological determinism—that such restrictions are also rooted in the prevailing American ideology of individual achievement. But taken together, the ideology combined with the use of statistical methods in limited areas, prevents the growth of our discipline and curtails our ability to strive for a full accounting and explanation of the major societal forces that shape our common destiny and determine our life chances. If the computer and the new methodological tools we possess now are not yet adequate for handling some of the issues I have raised, then let us at least press forward with theoretical explorations even if they should later have to be refined or modified by more precise empirical research. Let us not continue on a path about which one may say with the poet Roy Campbell (1955:198): "They use the snaffle and the curb all right. But where is the bloody horse."

Another symptom of the decline of a discipline, as Diana Crane (1972) indicates, is exclusive insistence on one particular dimension of reality and one particular mode of analysis by cliques or sects who fail to communicate with the larger body, or with one another. Under such conditions, a community of scholars will gradually dissolve through splitting up into a variety of camps of ever more restricted esoteric and specialized sects, jealously fighting each other and proclaiming that they alone possess the keys to the kingdom, while others are not just in

error, but in sin. Under such conditions the only dialogue between antagonistic camps is a dialogue of the deaf. Such tendencies have also become apparent in the last few years of the history of our discipline. This brings me to my second topic of examination, the assessment of ethnomethodology.

If I understand correctly, ethnomethodology aims at a descriptive reconstruction of the cognitive map in people's minds which enables them to make sense of their everyday activities and encounters. It is a method that endeavors to penetrate to the deeper layers of the categorical and perceptual apparatus that is used in the construction of diverse realities. The method also aims at a rigorous description of ordinary linguistic usage and speech acts. As such it seems aggressively and programmatically devoid of theoretical content of sociological relevance. Limiting itself by a self-denying ordinance to the concrete observation of communicative codes, subjective categorizations, and conversational gestures, it underplays the behavioral aspects of goal directed social interaction. It focuses instead on descriptions of definitions of the situation, meaning structures, conversational exchanges and the mutual modifications of images of self in such interchanges. Ignoring institutional factors in general, and the centrality of power in social interaction in particular, it is restricted to the descriptive tracing of the ways in which both individual actors and students of their activities account for their actions.

Ethnomethodologists put particular stress on the contextuality of accounts and meanings, their imbeddedness in the interactive context, their "situated" nature. Given the constitutive situatedness of any act, it is asserted that no objective generalizing approach is possible in the social sciences which by their very nature can only provide ideographic description. In some versions of ethnomethodology, inter-subjectivity is consciously neglected so that one ends up with a view of individual actors as monads without windows enclosed in their private and unshareable universes of meaning.

As distinct from path analysis and similar methods, ethnomethodology has not found ready acceptance within our discipline, in fact it has never sought such acceptance. It has consciously limited its appeal to devoted followers united in the knowledge that they possess a special kind of insight denied to outsiders.

Ethnomethodology claims access to types of knowledge not accessible to the sociological *vulgus*. Write Zimmermann and Pollner (1970), for example, on the ethnomethodological reduction, one of the mainstays of the method: "The reduction does not generate research that may be regarded as an extension, refinement, or correction of extant sociological inquiry. . . . The reduction constitutes as its phenomenon an order of affairs that has no identifiable counterpart in contemporary social science." More typically still than the oft-repeated insistence that ethnomethodology has a unique subject matter is the esoteric and particularistic nature of the pronouncements of its practitioners. Consider, for example, a paper by David Sudnow (1972) entitled "Temporal Parameters of Interpersonal Observation" which turns out to deal with the glances people exchange with one another or direct at the passing scene. It is concerned, as the author elegantly puts it, with "the issue of glance timing importance" (1972:273). "Let us consider," he states, "the situation of 'walking across the street,' where an orientation to be clearly so seen is held by virtue of the noted presence of a rapidly approaching vehicle. Here a familiar traffic situation may be regularly imagined where a mere and single glance is expected, where the sufficiency of the mere and single glance is criterial for bringing off safe passage. . . and where, as a consequence, the concern for a correspondence between the 'details' of what we are doing and what we are seen at a single glance to be doing, may be of paramount concern" (1972:269). When I try to explain to my four-year old grandson that he should always be careful when crossing a street, I say to him, "Always watch for passing cars." I do not think that Sundow's jargon conveys anything more. Each field, to be sure, must construct its own defined terms, but what is developed here is a restricted code of communications rather than open scientific vocabulary (Bernstein, 1971).

It is much too facile simply to poke fun at a group of people who profess central concern with linguistic aspects of interactive processes and yet seem unable to handle the vernacular. But the fact is that such language diseases have sociological significance in the develop-

ment of particularistic communities of True Believers. To begin with, esoteric language erects barriers against outsiders and confirms to the insiders that they have indeed a hold on some special truth. But there is more, such jargon, as the philosopher Susanne Langer puts it, "is language which is more technical than the ideas it serves to express" (1973:36), so that it can successfully camouflage relatively trivial ideas. Moreover, esoteric jargon may serve to bind the neophyte to his newfound anchorage. People tend to value highly those activities in which they have invested a great deal. Having invested considerable time and energy in mastering an esoteric vocabulary, people are loath, even when some disillusionment has already set in, to admit to themselves that what has cost them so much, might, after all be devoid of genuine value. Hence the particularistic vocabulary is not due to happenstance; it serves significant functions in marking boundaries and holding members.

Yet another characteristic with obvious functional value that ethnomethodologists share with similar close groupings in other scholarly areas, is the characteristic habit to limit their footnote references almost exclusively to members of the in-group or to non-sociologists, while quoting other sociologists mainly in order to show the errors of their way. There is, in addition, a peculiar propensity to refer to as yet unpublished manuscripts, to lecture notes and research notebooks.

It will be recognized that the characteristics I have outlined are those of a sect rather than of a field of specialization. I here define a sect as a group that has separated in protest from a larger body and emphasizes an esoteric and "pure" doctrine that is said to have been abandoned or ignored by the wider body. Sects are typically closed systems, usually led by charismatic leaders and their immediate followers. They attempt to reduce communication with the outside world to a minimum while engaging in highly intense interactions between the True Believers (Coser, 1974). Sects develop a special particularistic language, distinctive norms of relevance, and specialized behavior patterns that effectively set off the believers from the unconverted, serve as a badge of special status, and highlight their members' differentiation from the larger body of which they once formed a part.[2]

Yet what is functional for the sect is, by the same token, dysfunctional for those who are not among the elect. Blockage of the flow of communication is among the most serious impediments of scientific developments. A science is utterly dependent on the free exchange of information between its practitioners. Preciseness and economy in information flow makes for growth, and blockages lead to decline (cf. Crane, 1972). But the language of ethnomethodology, as James Coleman (1968; 130) once put it, makes for, "an extraordinarily high ratio of reading time to information transfer." More generally, an esoteric language can only serve to dissociate a body of people who were once united in common pursuits. As in the story of Babel in Genesis, "And the Lord said, 'Behold they are one people, and they have one language; and this is only the beginning of what they will do. . .Come, let us go down, and there confuse their language, that they may not understand one another's speech'."

Even though the sect is still quite young, the splits and fissions that typically beset sectarian developments have already set in. I do not profess to be knowledgeable about the detailed grounds of these developments (see Attewell, 1974 for an excellent mapping and critique), but shall only sketch some of them very roughly. At present, the ethnomethodology of Garfinkel differs significantly from that of Sacks which, in turn, is far removed from the concerns of Blum or the researches of Cicourel. Some versions are, in fact, solipsistic, others attend to intersubjective meanings, some admit the existence of invariant rules and procedures that transcend situations, others deny the possibility of any analysis that is not situation-specific. Some find philosophical anchorage in the German idealistic tradition and its Husserlian offshoots, others make use of British linguistic philosophy and seem to have replaced the guidance of Alfred Schuetz by that of Ludwig Wittgenstein. Some concentrate on the analysis of unique events, others attend to invariant

[2] For an earlier analysis of sect-like characteristics in sociology cf. Coser (1955).

properties of situated actions. The only thing all of them still seem to hold in common is the rejection of the possibility of an objective study and explanation of society and history, and a celebration of that long-dead warhorse of German idealistic philosophy, the transcendental ego.

Concern with the hypertrophy of wordage among ethnomethodologists and their other sectarian characteristics should, however, not preempt all of our attention. It is axiomatic among sociologists of knowledge that the origin of ideas does not prejudice their validity. It is possible that important and fruitful ideas may indeed develop in sectarian milieus. This has, in fact, often been the case, from the inception of puritanism to the emergence of psychoanalysis in the Viennese sect of Freud's immediate disciples.

Yet, when one turns to the problems that ethnomethodology tries to illuminate one is struck, for the most part, by their embarrassing triviality. We have already encountered Sudnow's "glancing research." Schegloff (1968) has spent productive years studying the ways in which people manage to begin and end their telephone conversations. I am not denying that "Studies of the Routine Grounds of Everyday Activities" (Garfinkel, 1967) may uncover significant and valuable matters, but in my considered judgment what has so far been dug up is mostly dross or interminable methodological disquisitions and polemics. Bittner's (1967) fine studies of the police or Cicourel's (1968) analysis of juvenile justice and a very few other good studies are not enough to justify the enormous ballyhoo surrounding ethnomethodology.

In general, it would seem to me, that we deal here with a massive cop-out, a determined refusal to undertake research that would indicate the extent to which our lives are affected by the socioeconomic context in which they are embedded. It amounts to an orgy of subjectivism, a self-indulgent enterprise in which perpetual methodological analysis and self-analysis leads to infinite regress, where the discovery of the ineffable qualities of the mind of analyst and analysand and their private construction of reality serves to obscure the tangible qualities of the world "out there." By limiting itself to trying to discover what is in the actors' minds, it blocks the way to an investigation of those central aspects of their lives about which they know very little. By attempting to describe the manifest content of people's experiences, ethnomethodologists neglect that central area of sociological analysis which deals with latent structures. The analysis of ever more refined minutiae of reality construction, and the assertion that one cannot possibly understand larger social structures before all these minutiae have been exhaustively mapped, irresistibly brings to mind Dr. Johnson's pregnant observation that, "You don't have to eat the whole ox to know that the meat is tough."

Path analysis, as has been shown, is a method that found quick acceptance among wide circles in the sociological discipline because it provided technical means for more precise measurements hitherto unavailable; ethnomethodology in contrast, found acceptance only among a small number of practitioners huddled around a charismatic leader and his apostles. The first was widely communicated through the various informational networks, both personal and impersonal, available to sociologists; the second developed particularistic codes of communication that effectively restricted access to all but the insiders. Yet what both have in common is a hypertrophy of method at the expense of substantive theory. The first has been used as an encouragement to neglect important areas of inquiry even while it has brought about greater precision of measurement in other areas, some important, some trivial. The second lends itself at best to atheoretical mappings of cognitive categories, and deliberately eschews concern with most of the matters that sociology has been centrally concerned with ever since Auguste Comte. In both cases, I submit, preoccupation with method largely has led to neglect of significance and substance. And yet, our discipline will be judged in the last analysis on the basis of the substantive enlightenment which it is able to supply about the social structures in which we are enmeshed and which largely condition the course of our lives. If we neglect that major task, if we refuse the challenge to answer these questions, we shall forfeit our birthright and degenerate into congeries of rival sects and specialized researchers who will learn more and more about less and less.

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SOCIAL STRUCTURAL AND PSYCHOLOGICAL ANTECEDENTS OF SELF-ESTRANGEMENT AND POWERLESSNESS*

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Estimates are presented for a structural equations model of the social structural and personality antecedents of two forms of early career alienation, self-estrangement and powerlessness. The sample consists of panel data from a cohort of 17-year-old men studied in 1957 and again in 1972 (N = 442). Analysis is by multiple regression based on correlations corrected for unreliability, a strategy relating measured variables to their latent constructs thereby making explicit issues of differential validity and reliability among indicators and the bearing of each on causal systems. The major finding is that the two forms of alienation are affected by different patterns of structural and personality antecedents originating at various stages in the first half of the life cycle. The model accounts for small to modest proportions of variance in self-estrangement and powerlessness and slightly reduces the covariation between the two forms of alienation.

A major focus of theory and research on personality and social structure has been the study of alienation, a concept described both as one of the five "essential unit-ideas of sociology" (Nisbet, 1966:8, 264-312) and "one of the most widely used and misused terms of our time" (Robinson and Shaver, 1971:161). Others have argued the conceptual debate in sufficient detail to establish that, at a minimum, consensus is lacking regarding use of the term and a host of cognates. The issue of the generality versus separability of constructs has been addressed both conceptually (Browning, 1961; Scott, 1963; Schact, 1970; Seeman, 1959; 1972) and empirically (Dean, 1961; Neal and Rettig, 1963; 1967; Struening and Richardson, 1965; Cartwright, 1965; Finifter, 1970). Nonetheless, the literature reveals little agreement on definitions and operationalizations of the concept; taken collectively it is more characteristic of relatively discrete and unrelated studies than of cumulative additions to a coherent body of empirically verified propositions.

We conceive of alienation as a generic concept embracing multiple sources and multiple meanings.[1] Both the source and meaning of alienation have been subjects of

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[1] The standard reference for conceptualizing alienation is Seeman (1972) who identifies six historical and thematic variants: powerlessness, meaninglessness, normlessness, value isolation, self-estrangement and social isolation.

debate. The issue concerning the *source* is whether alienation is the result of general and diffuse cultural conditions (Durkheim's normlessness, Merton's disjunction between cultural goals and individual means, Weber's bureaucratization), or is the result of group-specific factors (Marx's conditions of work, Srole's lower socioeconomic status). The latter position presupposes that there are axes of differential concentrations of dysfunctional elements in the social structure along which the alienated are located. Thus, for example, investigators uniformly report that alienation is inversely related to levels of education, occupational prestige and income. The issue concerning the *meaning* of alienation involves the nature of the relationship between the individual and the object or referent of the alienated attitude: whether, for example, the individual feels he cannot adequately control his circumstances, feels that the course of events is incomprehensible or feels that he is engaged in activities which are not rewarding in themselves. It is difficult to abstract a basic meaning with which to unify the diverse usages without ignoring subtle but significant nuances of meaning. We would observe, however, that there is at least minimal consensus to the notion that *alienation,* however defined, *involves discordance*[2] *in the individual's perception of the real and the ideal,* "the world that is" contrasted with "the world that [he feels] should be." Ours is a social psychological perspective. We define the concept not as an objective attribute of social structure (anomy) but in terms of the individual's subjectively perceived failure of normative expectations regarding opportunity for the realization of valued goals or general conceptions of what is desirable. Alienation has existential reality for the actor apart from the outside judgment of observers. It means that the individual feels he cannot achieve his valued goals, whether these be personal or culturally shared. His goals and/or means may not be aligned with those rewarded by the dominant society. Alternatively, he may share the norms regarding goals and means but may feel frustrated in realizing them. Thus, alienation has multiple foci, each referent being a variety of alienation without the necessity of an empirical unity among the variations or the existence of an over-all theory about the structure of alienation. The form of alienation depends on the particular individual values involved. Because alienation is a subjective state and individual perceptions vary, levels of alienation vary across individuals, even as the precipitating conditions, whether social or psychological, vary across individuals.

The alienated—whether reference is made to the elderly, widowed, divorced or separated, minorities, rural residents, the downwardly mobile, persons of low education, low prestige occupations or low incomes—have in common that they do not have full access to the communication and interaction patterns of the larger society. Isolation jeopardizes the possibility of a good fit between individual and cultural expectations concerning goals and means. A sense of meaningful social engagement for an individual, the converse of alienation, results from adjusting goals to means and from aligning aspirations with social expectations. The implicit model is that such accommodation is taught by operant learning, tuition and modeling—the processes of socialization. Alienation is a consequence of inadequate socialization which is precipitated by social and psychological conditions which either facilitate or impair individual learning.[3]

[2] We consciously avoid using Merton's "disjunction" concept which refers to objective opportunities to meet culturally shared goals. We restrict the meaning of discordance to perceptual phenomena and individual goals which may, in fact, be at variance with societal goals. See Seeman's (1972) definition of "isolation." Seeman's varieties of alienation allow for both individual and societal goals, although in his more general discussion he defines alienation "as the obverse of values central to . . . society" (1972:474).

[3] We differ with Wegner (1975), for example, who restricts the definition of his dependent variable (alienation) to effects caused by social structural independent variables. From our perspective, the question of the antecedents of alienation should be resolved empirically, not by editorial fiat. In this regard, McClosky and Schaar (1965) do not ". . . approach alienation . . . as solely a product of personality dysfunctions apart from the nature of society . . .," as Wegner (1975:178) alleges. On the contrary, McClosky and Schaar (1965:39) reason that "far from denying the existence of sociological factors, an attempt to assess the contribution of personality to anomy is a necessary step toward clarifying their [i.e., sociological factors] real significance."

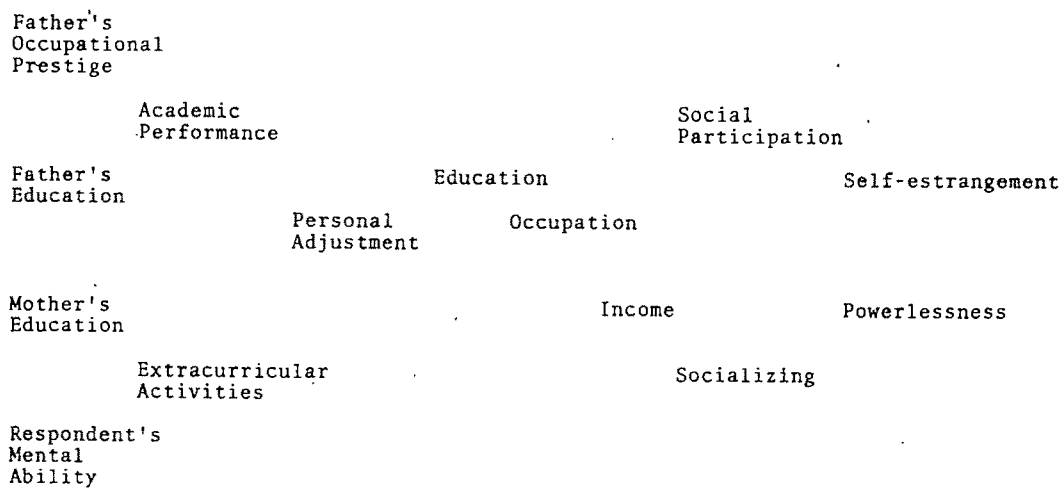

Figure 1. Schematic Diagram for a Fully Recursive Causal Model of the Social Structural and Psychological Antecedents of Self-Estrangement and Powerlessness.

The basic explanatory model proposed in the literature attributes alienation to social malintegration but largely ignores the psychogenesis of alienation. The suggestion that sociogenic models may be incomplete theoretical specifications was advanced by Srole (1956), who speculated on the possibility that the causes of alienation (anomia) might include psychogenic personality factors. However, finding that alienation was associated only with severe mental disturbance and was inversely related to socioeconomic statuses independent of mental disturbance, Srole concluded that social structural variables are the key antecedents and—like Durkheim, Marx and Merton—rested his inquiry by postulating that social conditions (by which he meant low socioeconomic status) were the independent variables of interest and importance. Almost without exception past research has explored the sociogenesis of alienation, but not the psychogenesis.[4] In contrast, the present study provides estimates of the relative and independent social structural and personality effects on two forms of alienation over the early stages of the life cycle.

[4] An exception is the McClosky and Schaar (1965) investigation of the psychological dimensions of alienation (anomy) which has influenced our own thinking.

The Conceptual Model

We specify a multi-stage conceptual model for estimating the antecedents of two correlated forms of early career alienation over a fifteen-year time period (Figure 1). The dependent variables are powerlessness and self-estrangement which correspond closely to Seeman's (1972) alienation variants by the same labels. Powerlessness relates to the individual's expectancy concerning the occurrence of the outcomes or reinforcements he seeks. It refers to his perceived inability to control his fate which renders him uncomfortable and may manifest itself in psychosomatic symptoms. Powerlessness is a feeling of exposure and vulnerability—the converse of mastery and autonomy, values central to American society. Self-estrangement refers to an individual's lack of intrinsic satisfaction from his activities, whether these are work related in the Marxian sense of the estranged worker, or refer to a feeling of lack of fulfillment arising out of more general life circumstances. Self-estrangement reflects a discrepancy between the individual's ideal self and his actual self. For the self-estranged, life is chronically flat, empty and boring, void of the vitality that the individual feels should somehow be there. He feels deprived of meaning, enslaved by unrewarding activities. The two variants of alienation are theoretically concieved unobserved variables reflected in

multiple measured indicators. The first indexes the individual's perception of his own ability to control his environment, a widely shared goal in contemporary society; the second indexes a highly personalized goal, finding meaning and a sense of fulfillment in one's daily routines. The two are interpreted not as reflections of a common latent construct but as two kinds of alienation differentiated by the specific values they imply. We do not postulate a structure of alienation but two variants of alienation that are correlated. Our conceptual model proposes that the covariation between powerlessness and self-estrangement has its source in common antecedents, both social and psychological, though the pattern of antecedents is not necessarily expected to be identical for the two forms of alienation.

We have defined alienation as a feeling of discordance between the individual's perception of what is and what ought to be. Individuals who, by accident of birth or socioeconomic achievement, have more resources thereby have more options with which to achieve their goals than do the disadvantaged. They are less likely to feel a discrepancy between the real and the ideal but enjoy a kind of psychic harmony with their environment. Moreover, their socioeconomic resources provide the wherewithal for various forms of social participation which facilitates alignment of personal aspirations with social expectations and accommodation of one's aspirations to his resources. Social integration provides the context for socialization. In interaction the individual learns aspirations appropriate to the society of which he is a part and appropriate to the resources he himself commands. But those who occupy less advantageous positions—because of poverty, the conditions of their employment, low education, psychological impairments or other forms of social isolation—have fewer resources and options at their disposal and suffer the consequences of less adequate socialization. These assertions reflect the empirical fact that there are negative correlations between socioeconomic status and various forms of alienation, even as there is a negative correlation between social integration and alienation. Our argument rests on the tacit assumption that values emanate from opinion leaders, persons in control of social institutions who themselves belong to effective elites and enjoy high socioeconomic status. Therefore, we expect a closer correspondence between personal and societal values, on the one hand, and personal aspirations and resources, on the other hand, among those of higher socioeconomic status and the socially integrated than among the socioeconomically handicapped and poor performers in an achievement oriented society.

We observe, however, that all who share the same social circumstances do not respond identically: all who enjoy advantageous socioeconomic status are not necessarily low on alienation (or high on sense of social engagement) and all who suffer socioeconomic deprivation are not necessarily more highly alienated—although the inverse relationship between socioeconomic status and various forms of alienation is a well-documented social fact (Srole, 1956; Erbe, 1964; Rushing, 1972). The differences in perceptions, the within group variability, suggest that individual intellectual and personality characteristics may have an independent effect on alienation. We have argued that alienation is a product of inadequate socialization, and that those enjoying higher socioeconomic status and those socially integrated have fullest access to the communication and interaction patterns of the larger society. Inadequate socialization, however, may result not only from that which is taught (or not taught) but also from that which is learned (or not learned). Obviously, what is taught and what is learned are not synonymous. Respondent's intellectual and personality characteristics have an independent effect on alienation inasmuch as these affect his perceptions and learning. We predict that those best equipped to perceive accurately their own resources and aspirations and to understand and assimilate societal expectations will be less alienated; therefore, including mental ability and personal adjustment as predictors of alienation should account for added variance in alienation and lower the covariation between powerlessness and self-estrangement. Incorporating both social structural and psychological antecedents into our conceptual model provides estimates of the relative influence of each and a more accurate accounting of the real significance of social structural effects on alienation, which has been the dominant focus of past research.

The reduced form of the conceptual model

examines the influence of background socioeconomic statuses (a social setting)—father's occupational prestige, father's education and mother's education—on self-estrangement and powerlessness. Given that the values of those in higher statuses are most closely allied with those most widely shared within a society and given that those in higher statuses have the most resources with which to realize their personal goals, we expect socioeconomic statuses to inversely affect respondent's alienation. Moreover, if alienation results from an unsatisfactory adjustment of aspirations to resources, which may be due to learning impairments contributing to inadequate socialization (Zigler and Harter, 1971), then respondent's innate intellectual capacities may be expected to have an inverse effect on early career alienation and to reduce further the covariation between the two forms of alienation.

The second stage of the conceptual model introduces three adolescent variables which we hypothesize will inversely affect adult alienation, statistically controlling on background socioeconomic statuses and mental ability. We interpret the first two, academic performance and participation in extracurricular activities, as separate but correlated dimensions of adolescent social integration. The role of student doubtless is multi-dimensional, but standards for role performance frequently differentiate academic performance (good grades) from social participation (peer acceptance). Some (Coleman, 1961) have argued that these two role elements are mutually exclusive, leading to separate student cultures; others hold that the two are compatible, if not causally linked (Spady, 1970; Rehberg and Schafer, 1968). We hypothesize that social integration in adolescence is possible, via either or both of these student role dimensions, and that adolescent social integration will have lagged effects on adult alienation inasmuch as access to communication and interaction patterns provides the context for learning social expectations and for adjusting one's aspirations to his resources, hence lowering perceptions of discordance.

That academic performance is influenced by background socioeconomic statuses and mental ability has been demonstrated (Sewell and Hauser, 1972; 1975) as has the association for children from families of higher socioeconomic statuses with membership in youth associations. The third adolescent variable is personal adjustment which enters the model as an unobserved variable reflecting measures of nervous tension, emotional stability and status anxiety. Adolescent personal adjustment is specified as being in part a consequence of social integration. An inverse association between socioeconomic statuses and personal adjustment has been noted in past research (Sewell, 1961; Clausen and Kohn, 1970). We expect that personal adjustment will negatively affect alienation, inasmuch as the adjusted personality is best able to interpret societal expectations and to realistically accommodate aspirations to resources. Estimating the relative effect of personality traits on alienation will test both the hypothesis of the psychogenesis of adult alienation and the hypothesis that personality characteristics are a source of spurious association between powerlessness and self-estrangement.

The relationship between adulthood environments and alienation is the most widely studied association in the literature and is a component of the more elaborate model we propose. The antecedents of socioeconomic attainments—education, occupation and income—and the functional relationship between the three have been elaborated elsewhere (Sewell and Hauser, 1972; 1975; Duncan, Featherman and Duncan, 1972). Estimating the antecedents of alienation in the context of the status attainment process (see the suggestion by Sewell and Hauser, 1972) has the added virtue of integrating two previously related but largely isolated research traditions in a causal system that regards alienation as a non-socioeconomic consequence of status attainment levels. Our model will provide independent estimates for the current adulthood socioeconomic indicators,[5] statistically controlling on the potentially spurious influences of antecedents. Past research has demonstrated that socioeconomic status is inversely related to various forms of alienation (Dean, 1961; Neal and Seeman, 1964), including dependent variables closely

[5]That the separate components may differentially affect dependent variables has been demonstrated by Hodge (1970), Hauser, Lutterman and Sewell (1971) and others.

related to our own (Inkeles, 1960). We expect a replication of previous findings, which is consistent with our theses that there is a built-in correspondence between the personal and social values of those in higher socioeconomic statuses who define social norms, and that persons with more resources are better able to achieve their personal goals. For the same reasons, we expect that incorporating current statuses into the estimating equations will reduce the covariation between self-estrangement and powerlessness.

We incorporate two dimensions of adult social integration as final predictors of alienation, social participation and socializing. The two variables tap the extent to which the respondent is involved in primary and secondary relationships. We have reasoned that social integration provides access to the processes of socialization which instruct the individual with respect to social expectations and to alignment of his aspirations with his individual resources, which promotes a sense of harmony with his social environment. The adult social integration indicators (hypothetically) intervene between concurrent socioeconomic statuses and the two alienation indicators in our conceptual model. That socioeconomic statuses are associated with memberships in organizations and patterns of socializing has been demonstrated by Hyman and Wright (1971) and Hodge and Treiman (1968; see also Hauser, 1969). Consistent with our thinking about structural contexts for learning (socialization), we view the relationships of socioeconomic attainments with alienation as being both direct and indirect. Education, occupational status and income (in possibly different ways) indirectly affect alienation by patterning social relationships which either facilitate or impede the individual's chances to perceive the social environment favorably. We assume that social integration, here indexed by socializing and voluntaristic associations, forges a closer sense of concordance between perceived self and social environment. Therefore, we specify that indirect effects of the separate socioeconomic statuses operate through social integration.

The direct linkages between status attainments and alienation are manifestations of socioeconomic achievement (or its lack) in a society with longstanding expectations for personal advancement. We ordinarily expect higher levels of alienation among persons with lower attainment levels, although the direct effects of the separate statuses need not be identical. Further, we might anticipate that while zero-order correlations between current statuses and alienation may be zero, the net effects need not be.

Although we are not concerned with all possible relationships in the fully-recursive model displayed in Figure 1, the specification provides estimates of total and direct effects for each variable statistically controlling on all antecedents. The analysis is essentially descriptive. Our primary interest is in estimating and describing the relative extent to which social structural and personality factors explain the variance in powerlessness and self-estrangement and account for the covariation between the two forms of alienation. The model provides a test of the hypothesis that the latter covariation represents spurious association with both social structural and psychological antecedents.

Data, Variables and Methods

The analysis is based on panel data with a fifteen-year interval between initial and follow-up waves. In the spring of 1957, a questionnaire soliciting socioeconomic background and personal data was administered to all 17-year-old male students enrolled in the Lenawee County, Michigan, high schools (N = 442). The county's status system was then characterized as having "a full range of social class levels: a few wealthy families, a number of families of professionals, many families of small business owners and many families of farmers, clerical, skilled and unskilled workers" (Haller and Miller, 1971:69). The sample covered 88% of the entire age cohort, all but 12% who had taken fulltime jobs and were no longer enrolled in high school.[6]

[6]To whatever extent leaving school by age seventeen introduces bias, the 1957 data are not truly representative of the age group residing in Lenawee County at the time of the original study. Follow-up studies showed that about five of six dropouts were sons of farmers (Jarrett and Haller, 1964:167). Thus, the findings of this study are generalizable to the seventeen-year-old male youth enrolled in Lenawee County, Michigan, high schools in the spring of 1957.
Unfortunately, there exist few panel data sets of any kind, to say nothing of a total absence of those relating to a seventeen-year-old male age cohort with a fifteen-year interval, with which to make sample

In 1972, 15 years after the original study, a concentrated follow-up effort located 354 (82%) of the original respondents of whom 340 consented to be interviewed by telephone on subjects concerning life cycle histories and current socioeconomic attainments.[7] Respondents were also asked to complete and return a mailback questionnarie consisting of measures of social integration and alienation. Of the original 442 participants in 1957, four were excluded from the analysis because of invalid data and eight were known to be deceased leaving a total of 430 eligible respondents in 1972. Three hundred twenty-seven of the eligible 1972 respondents were interviewed by telephone and 291 completed and returned the mailback questionnaire. An additional 13 respondents who were inaccessible by telephone completed and returned a special mailback questionnaire composed of items from the telephone interview and mailback questionnaire. Thus, the working sample for this analysis consists of 304 respondents or 70.69% of the 1957 participants. Extensive data checks revealed no evidence of nonresponse bias or sample attrition.

The conceptual model specifies background socioeconomic statuses and mental ability as exogenous variables. *Father's occupation* is coded into Duncan (1961) SEI scores. *Father's education* and *mother's education* are scored 0 for less than eight grades of formal education completed, 1 for 8 grades, 2 for 9 to 11 grades, 3 for 12 grades, 4 for some college and 5 for a college degree or more education. *Mental ability* is assessed with the Cattell IPAT Test of G-Culture Free-Scale 3A (Cattell and Cattell, 1950).

Adolescent social integration is measured by academic performance and extracurricular activities. *Academic Performance* is indicated by high school grade point average figured on a 4-point scale and retrieved directly from high school records. *Extracurricular activities* was operationalized by asking participants to identify their areas of participation on a 14-item checklist.[8] A score of unity was assigned each activity checked and respondent's level of participation was indicated by the summed total score. Adolescent *personal adjustment* is an unobserved variable reflected by three measured indicators: nervous tensions and emotional stability, subtests for factors Q_4 and C from the Cattell 16 Personality Factor Tests and status anxiety as measured by the Status Anxiety Index (Haller and Miller, 1971). The method of constructing all unobserved indicators is reported below. Background socioeconomic statuses, mental ability, academic performance and adolescent personal adjustment were measured in 1957.

Current socioeconomic statuses include respondent's level of education, occupational prestige and income, which were measured in 1972. *Education* refers to number of years of formal schooling completed. Respondent's *occupation* is coded into the Duncan (1961) SEI. *Income* refers to respondent's reported total annual earnings in the preceding year, 1971.

The analysis employs unobserved variables constructed of multiple indicators of adult social integration and alienation. Adult social integration is assessed by four objective indicators from which two unobserved variables are constructed, social participation and socializing. *Social participation* is a dimension of adult social integration which taps respondent's memberships in organizations and involvement in the political process. Extent of organizational memberships was

comparisons. Thus, extreme caution must be exercised in generalizing beyond the population sampled.

We do not attempt to compare our sample estimates with the Wisconsin data (Sewell and Hauser, 1972; 1975) because: the time interval between initial and follow-up waves is about twice as long for the Lenawee County data as the Wisconsin data; key variables are operationalized differently across samples; estimates from both data sets are standardized rather than unstandardized regression coefficients; the present estimates are based on correlations corrected for unreliability. Analysis for a thorough comparison of estimates from the two data sets is underway and will be reported upon completion.

[7] All instruments were carefully pretested and telephone interviewers were thoroughly trained by the research directors. Data gathering and coding was carried out under contract with the Wisconsin Survey Research Laboratory.

[8] The checklist of high school extracurricular activities included: athletics, band-orchestra, chorus-vocal, dramatics, debate, 4-H or FFA, school paper, annual, student government, hobby club and four opportunities to specify "other."

ascertained by a checklist of 15 voluntary organizations on which respondents indicated whether or not they were a member of each.[9] Respondent's score consisted of the total number of memberships he reported. A six-item index of political participation was constructed which provided indication of respondent's involvement over various levels in the political process.[10] A score of 1 was assigned for each category of participation acknowledged by the respondent and the scores across categories were summed.

A second unobserved adult social integration variable, *socializing*, a dimension tapping primary associations and informal visiting patterns, is constructed from a pair of single item indicators, visits with friends and friends visited. Respondents were asked, "How many times during the past week have you gotten together with friends?" and "About how many friends was that?" Respondent's numerical response is taken as his score on the indicators.

The two alienation variables, self-estrangement and powerlessness, are also unobserved constructs reflected in pairs of measured indicators. Following Seeman (1972:472-3), the *self-estrangement* dimension draws heavily from classic descriptions of the estranged worker carrying out unfulfilling and uncreative work. The construct is broader than work-related activity, however, and includes engagement in more general activities that are not rewarding in themselves. We tap this element of discordance indirectly with two single item satisfactions indicators, one a measure of work satisfaction and the other a measure of general satisfaction. Respondents were asked, "Taking all things into consideration, how satisfied are you with the way things are these days?" The single item indicator was scored on a 5-point Likert scale ranging from 1 (very dissatisfied) to 5 (very satisfied). Similarly, "Taking into consideration all things about your job . . ." respondents were scored 1 (very dissatisfied) to 3 (very satisfied).

Powerlessness refers to a low expectancy that one's own behavior can control the occurrence of personal and social rewards. The person feeling powerless believes that control is vested in external forces, powerful others, luck or fate. Life is capricious. He feels uncomfortable. Five items from the Neal and Seeman (1964) powerlessness scale provide the first component of the *powerlessness* construct. The second component reflects the element of discomfort as measured by nine psychosomatic symptom items from the Psychiatric Impairment Index (Langer, 1962).[11] Respondents were asked whether they were bothered in any of the nine ways frequently, sometimes, hardly ever, or not at all during the last few weeks. Responses were assigned scores of 3, 2, 1 and 0 in order of the response categories, and item scores were summed to form a total score for the index.

We estimated or assumed[12] validity coeffi-

[9] The membership list included: church groups, labor unions, veterans organizations, fraternal organizations or lodges, business or civic groups, parent-teacher associations, community centers, organizations of people of the same nationality, sport teams, country clubs, youth groups, professional groups, political clubs or organizations, neighborhood improvement associations, charity or welfare organizations and two open-ended response possibilities.

[10] The six categories of political activity were: (a) In recent elections did you make a campaign contribution, buy tickets, go to any political rallies, barbecues, fish fries, or anything like that in support of a candidate? (b) Did you talk to people trying to get them to vote for or against any candidate? (c) What about recent elections for local or county offices—like sheriff or mayor? How often have you voted in these elections? (d) In recent months have you ever tried to influence the opinions of others on some local issue by calling a meeting, circulating a petition, testifying at a hearing or doing something like that? (e) In recent months have you ever written a letter to a newspaper or public official, telephoned or talked directly with a public official regarding a public issue that concerned you? (f) Have you ever contacted an elected official to seek an adjustment on a personal matter?

[11] The nine items are: general aches and pains, trouble sleeping, nervousness or tenseness, dizziness, rapid heart beat, cold sweats, muscle twitches or trembling, worries which make me ill and headaches.

[12] We assumed the validity coefficients for current socioeconomic status variables provided by Siegel and Hodge (1968:37), for son's report of parental statuses as provided by Mason, Hauser, Kerckhoff and Poss (1972), and for the mental ability test as reported by Cattell and Cattell (1950). Academic performance was assumed to be perfectly valid inasmuch as the information was retrieved directly from school records. So that our estimates for the effects of extracurricular activities would be *conservative*, we assumed that the reliability of reports on extracurricular activities was at least as high as the reliability of son's report of father's occupation and education. The validity

cients[13] for all variables for the purpose of establishing reliabilities and estimated the inter-correlations of the unobserved and observed variables corrected for unreliability.[14] In establishing the intercorrelations we estimated an overidentified factor analytic model using Jöreskog's confirmatory factor analysis procedures which provide maximum likelihood estimates (Jöreskog, 1970; Jöreskog, Gruvaeus and van Thillo, 1970). The factor matrix (correlations for all observed and unobserved variables corrected for unreliability) upon which the analysis is based is displayed in Table 1.

Causal Structure Among the Antecedents of Alienation

Attention first focuses on the relationships among the exogenous and endogenous variables (Table 2). The zero-order correlation between X_5—academic performance and X_6—extracurricular activities ($r = -.04$) supports Coleman's (1961) thesis that the two are virtually orthogonal dimensions of the student role and that there is no covariation to explain. Their independence is further underscored by the pattern of antecedent effects. Past research prompts the expectation that background socioeconomic statuses and mental ability will influence academic performance, which is supported by our data. X_3—mother's level of education and X_4—respondent's mental ability provide significant[15] influences on the variance explained in respondent's academic performance ($R^2 = .360$).[16] Our specification is a much less satisfactory predictor of the nonacademic student role dimension, however, $R^2 = .0051$. Failure to explain more of the variance in extracurricular activities raises a question concerning the generalizability of the Hyman and Wright (1971) report associating children from higher status families with membership in youth associations. The finding is not supported by our data on participation in high school extracurricular activities.[17]

Our data provide only modest support for the sociogenesis of X_7—adolescent personal adjustment reported in previous studies. Indeed, summing the beta coefficients for the background socioeconomic indicators yields a near zero net effect, even though father's occupation and mother's education have significant (but offsetting) influences. Adolescent males with better educated mothers are more adjusted despite the positive association of mother's education and father's socioeconomic statuses and the negative effect of father's occupational prestige on adjustment. An interesting difference appears in the pattern of effects of the two student role

coefficients for measured indicators (here abbreviated as in the accompanying tables) are as follows: FOCC = .956, FED = .958, MED = .883, MA = .837, AP = 1., XACT = .960, ED = .966, OCC = .928, and INC = .923. (For a discussion of the consequences of under- or over-estimating measurement reliability, see Duncan, Featherman and Duncan (1972:chapter 6)).

[13] Validity, in this sense, is defined as the correlation between a true score and an observed score. This definition of validity presupposes the definition of reliability as the squared correlation between the true and observed scores (see Lord and Novick, 1968:55-63). These definitions presuppose two sources of variation in observed scores, a reliable component representing the true underlying dimension and an unreliable component representing random error of measurement. However, it is possible that the observed score reliably measures something in addition to or instead of the underlying true score of interest, e.g., nonrandom response bias. Unfortunately, such sources of variation can rarely be specified with the result that validity estimates may be exaggerated. This possibility prompts consideration of the square root of reliability as a ceiling estimate of validity (see Lord and Novick, 1968:72).

[14] A similar construction of unobserved variables for use in structural equation models was introduced to the sociological literature by Hauser and Goldberger (1971), was elaborated by Burt (1973) and is illustrated by Goldberger and Duncan (1973) and others.

[15] Use of the qualifier "significant" is reserved for relationships that are statistically significant. Statistical significance for standardized regression coefficients is conventionally defined by the criterion that the absolute size of a coefficient must twice exceed its standard error. The reader is reminded that data have been adjusted for unreliability and the impact of differential validity is explicit in our models.

[16] However, we do not assert that mother is more important than father as a source of academic motivation, as the averaged effects of father's occupation and education is not statistically different from the effect of maternal education (Featherman and Carter, forthcoming: fn. 5).

[17] It may be that participation in non-academic youth associations is a direct function of family income, a relationship we are unable to explore with the present data.

Table 1. Correlations Corrected for Unreliability for Background Socioeconomic Statuses, Mental Ability, Academic Performance, Extracurricular Activities, Adolescent Personal Adjustment, Adult Socioeconomic Statuses, Adult Social Integration, and Alienation

Variables[a]	X_1	X_2	X_3	X_4	X_5	X_6	X_7	X_8	X_9	X_{10}	X_{11}	X_{12}	X_{13}
X_1-FOCC													
X_2-FED	.481												
X_3-MED	.340	.611											
X_4-MA	.278	.268	.329										
X_5-AP	.258	.252	.329	.578									
X_6-XACT	.002	-.007	.018	-.057	-.040								
X_7-PA	-.046	.160	.295	.177	.283	-.110							
X_8-ED	.342	.379	.406	.537	.666	.163	.232						
X_9-OCC	.367	.285	.288	.502	.626	.242	.132	.817					
X_{10}-INC	.197	.239	.217	.139	.149	.209	-.001	.247	.251				
X_{11}-SP	.054	.141	.177	.078	.162	.395	.243	.347	.353	.353			
X_{12}-SOC	-.030	.024	.089	.051	-.031	.136	.051	.135	-.010	.163	.239		
X_{13}-SE	-.010	.044	-.038	.119	.052	-.180	-.255	-.131	-.191	-.226	-.286	-.077	
X_{14}-PWR	-.208	-.367	-.276	-.285	-.277	-.129	-.524	-.347	-.412	-.135	-.548	-.179	.401

[a]X_1 father's occupation; X_2 father's education; X_3 mother's education; X_4 mental ability; X_5 academic performance; X_6 extracurricular activities; X_7 personal adjustment; X_8 education; X_9 occupation; X_{10} income; X_{11} social participation; X_{12} socializing; X_{13} self-estrangement; X_{14} powerlessness

Table 2. Standardized Regression Coefficients, Standard Errors (In Parenthesis) and Multiple Correlation Coefficients for Variables at Each Stage of the Conceptual Model

	Dependent Variables[a]							
	X_5	X_6	X_7	X_8	X_9	X_{10}	X_{11}	X_{12}
Variables[a]								
X_1-FOCC	.070	.018	-.217*	.090*	.119*	.053	-.058	-.039
	(.052)	(.065)	(.060)	(.044)	(.036)	(.064)	(.056)	(.065)
X_2-FED	-.004	-.029	.040	.119*	-.032	.123	.006	-.079
	(.061)	(.076)	(.070)	(.051)	(.041)	(.073)	(.063)	(.074)
X_3-MED	.139*	.053	.269*	.066	-.068	.085	.004	.054
	(.058)	(.073)	(.067)	(.050)	(.041)	(.072)	(.062)	(.072)
X_4-MA	.514*	-.072	-.007	.184*	.066	.008	-.097	.080
	(.049)	(.061)	(.065)	(.047)	(.039)	(.069)	(.059)	(.069)
X_5-AP			.240*	.478*	.167*	.002	-.081	-.168*
			(.064)	(.048)	(.044)	(.080)	(.068)	(.080)
X_6-XACT			-.105*	.198*	.143*	.173*	.295*	.121*
			(.052)	(.038)	(.032)	(.058)	(.050)	(.059)
X_7-PA				.052	-.033	-.050	.251*	.034
				(.041)	(.033)	(.059)	(.050)	(.059)
X_8-ED					.653*	.055	.160	.472*
					(.046)	(.103)	(.089)	(.104)
X_9-OCC						.087	.175*	-.380*
						(.096)	(.086)	(.100)
X_{10}-INC							.243*	.145*
							(.049)	(.057)
X_{11}-SP								
X_{12}-SOC								
R^2	.360	.005	.174	.564	.717	.131	.358	.123

Correlation of residuals for X_5 and X_6 = - .017; X_{11} and X_{12} = .151.

[a]X_1 father's occupation; X_2 father's education; X_3 mother's education; X_4 mental ability; X_5 academic performance; X_6 extracurricular activities; X_7 personal adjustment; X_8 education; X_9 occupation; X_{10} income; X_{11} social participation; X_{12} socializing

*Absolute value of coefficient is at least twice as large as the standard error and the relationship is defined as statistically significant.

dimensions. If one sums the two beta coefficients, one notes the predicted positive relationship between adolescent social integration and personal adjustment, a crude analogue to alienation. Nonetheless, the two dimensions of adolescent social integration have conflicting influences. The academic role is primary. Social rewards for academic performance nurture personal adjustment. The non-academic or peer acceptance dimension is secondary and stands in opposition to the academic performance dimension with respect to personal adjustment. The inarticulation reveals possible strain in the adolescent role set resulting in diminished levels of personal adjustment. The variance explained is $R^2 = .174$.

The findings of Sewell and Hauser (1972; 1975) and others with respect to the antecedents of X_8—educational attainment are replicated in our data inasmuch as respondent's family socioeconomic statuses (indexed by father's occupation and education), his mental ability and academic performance, each positively influence his eventual level of educational achievement ($R^2 = .564$). Of particular interest is the fact that the non-scholastic dimension of adolescent social integration also furthers respondent's educational attainment, statistically controlling on the antecedents cited. Spady (1970) argues that extracurricular activities, like the academic curriculum, provide opportunities for acquiring attitudes and skills from which status goals evolve and upon which future educational achievements are based. That participation in extracurricular activities provides a socialization context that has later-life payoffs is supported by our data. Student role performance, whether within or outside the classroom has a salutary influence on eventual educational attainments.[18]

The extent of occupational status inheritance operating in the sample is revealed by the significant effect of father's level of occupational status on X_9—son's occupation (beta = .119). Again, performance levels on both dimensions of the student role predict attainment. Adding the expected influence of education to the regressors explains a substantial proportion of the variance in socioeconomic status ($R^2 = .717$), a value larger than that ordinarily reported for two reasons. First, the estimates suggest that successful student role performance, both academic and social, is an important predictor of occupational achievement, even for men of equivalent schooling. Second, the reader is reminded that the proportion of variance explained in our analysis is based on correlations corrected for unreliability/invalidity. Given the error structure of the model we specify, we expect larger proportions of variance explained than that estimated from the uncorrected matrices usually reported.

Analysis of the antecedents of X_{10}—income reveals but one significant and positive direct effect, that of participation in extracurricular activities. Collinearity among the other groupings of predictors—e.g., among the socioeconomic indicators—vitiates the significance of separate effects. The effect of extracurricular activities on income appears to be substantively significant, however, inasmuch as it is not collinear with background predictors. Given that performance on this dimension of the student role is an adolescent analogue to adult voluntaristic associations, it is interesting to note that the usual specification, that income provides the resources enabling social participation, is here reversed: adolescent social integration enhances earning power. The relationship is consistent with our socialization explanation that social integration facilitates learning societal expectation, in this case a success orientation measured by earnings. The variance explained in income is $R^2 = .131$.

The variance accounted for in X_{11}—social participation and X_{12}—socializing is $R_2 = .358$ and .123, respectively. Neither dimension of adult social integration is influenced by either background socioeconomic statuses or mental ability. We interpret the beta coefficients between the two dimensions of adolescent social integration and adult social participation as reflecting the stability of voluntaristic association over the fifteen-year period. Summing the beta coefficients yields a substantial net relationship; the more socially integrated one is in adolescence, the more involved he is likely to be in adulthood.

[18] The long-term lagged effects of participation in high school extracurricular activities on socioeconomic attainments and non-socioeconomic consequences of attainments are reported by Otto (1975;1976).

Further, those more adjusted psychologically as adolescents tend to join groups as adults, apart from the over-time stability of social participation. In adulthood, a higher income provides the discretionary resources necessary for voluntaristic participation, and net of earnings men in high status jobs are more involved in secondary associations.

X_{12}—socializing is not affected by psychological adjustment in adolescence, but participation in extra-scholastic activities forecasts involvements in enlarged primary relationships in adulthood. However, the scholastic achievers (within categories of completed schooling) are less occupied with socializing (number of times visited friends and number of friends visited). The better educated and those with higher earnings are more social in their life styles, but within categories of education and income, men in higher status jobs socialize less than other men. The opposite effects of occupational status on socializing and social participation suggests a need for further specification in the conventional thinking about the kind of "social participation" which is an extension of the formal work roles of professional workers (Axelrod, 1956). Our data support the explanation with respect to voluntaristic associations, X_{11}—social participation, but fail to confirm with respect to socializing. The zero-order correlation between the two forms of adult social integration is $r = .239$. The correlation of residuals, which takes into account the effects of spurious association due to common antecedents, is $r = .151$. The partial correlation indicates that our model does account for covariation in the two dimensions of adult social integration, albeit incompletely. The model suggests that psychological as well as sociological factors—some extending from adolescence—are important for an understanding of integration and that the causal structure of the two components is differentiated.

Causal Antecedents of Self-Estrangement and Powerlessness

Estimates of the social and psychological antecedents of X_{13}—self-estrangement and X_{14}—powerlessness are presented in Table 3, which displays regressors in blocks coinciding with the analysis and foregoing conceptual scheme. We shall discuss each block of regressors *seriatim* as it bears upon both self-estrangement and powerlessness.

The social causation argument (Dohrenwend and Dohrenwend, 1969) that low social status produces personal disintegration and the argument that sense of control is in part a product of status related socialization content (Rotter, 1966; Lefcourt, 1966), are only partially supported in Table 3. The family socioeconomic variables—X_1 through X_3—account for a mere 1 percent of variance in self-estrangement and 14 percent in powerlessness. We regard the small positive effect of paternal education on self-estrangement (X_{13}) in equations 5 through 8 to be artifactual (given the large negative coefficients for the adult socioeconomic statuses and the positive correlation of paternal and filial statuses). With respect to powerlessness (X_{14}), father's education seems to retain a negative effect, even as we move from the reduced form model (equation 1) to further elaborations with subsequent causal antecedents. Education provides control-relevant information, and perhaps fathers with higher schooling convey a sense of mastery to their sons in ways which mothers do not (see the small negative or zero coefficients for X_3). Perhaps sex-role modelling is important in this regard.

X_4—mental ability illustrates differential psychogenic effects on the two forms of alienation. The intellectually gifted believe in their own potential to be effective (X_{14}), on the one hand, yet experience more self-estrangement (X_{13}). Though feeling in control and enjoying socioeconomic success, a sense of fulfillment escapes them. The mentally able are the least able to reconcile the reality of their daily routines with the perhaps very high personal ideals to which they aspire as very able persons. Inasmuch as mental ability influences both forms of alienation independent of socioeconomic background effects, the incompleteness of purely sociogenic explanations of alienation is underscored.

The total effect (in the reduced form equations 3) of adolescent social integration, X_5—academic performance and X_6—self-estrangement, is negative in both variants of alienation, as predicted. However, the reduced-form estimates do not take into account the stability of social integration over time, or the positive relation between social

and academic involvements in school and socioeconomic attainment. With respect to powerlessness the adolescent effects are absorbed by adult social integration—i.e., adolescent social integration has an indirect effect via the stability of participation noted earlier. With respect to self-estrangement the negative effect of extracurricular activities is absorbed by the socioeconomic indicators, especially occupation. Controlling for adult status attainments, however, the academic dimension of integration has an unanticipated positive effect on self-estrangement.

The psychogenic source of adult alienation

Table 3. Standardized Regression Coefficients, Standard Errors (in parentheses) and Multiple Correlation Coefficients for Early Career Alienations [(A) Self-Estrangement; (B) Powerlessness] on Background Socioeconomic Statuses, Mental Ability, Adolescent Social Integration, Personal Adjustment, Current Socioeconomic Statuses, and Adult Social Integration

A. X_{13} - SELF-ESTRANGEMENT

Regressor Group:	1	2	3	4	5	6	7	8
Variables[a]								
X_1-FOCC	-.035	-.061	-.058	-.131*	-.104	-.060	-.049	-.053
	(.064)	(.064)	(.064)	(.062)	(.061)	(.061)	(.059)	(.059)
X_2-FED	.122	.118	.113	.127	.162*	.151*	.177*	.176*
	(.076)	(.075)	(.075)	(.071)	(.070)	(.068)	(.067)	(.067)
X_3-MED	-.101	-.139	-.129	-.037	-.018	-.043	-.024	-.023
	(.071)	(.072)	(.072)	(.070)	(.069)	(.067)	(.066)	(.066)
X_4-MA		.150*	.142*	.140*	.195*	.220*	.221*	.216*
		(.060)	(.069)	(.066)	(.066)	(.065)	(.063)	(.063)
X_5-AP			-.008	.073	.216*	.278*	.279*	.271*
			(.069)	(.067)	(.075)	(.075)	(.073)	(.074)
X_6-XACT			-.169*	-.205*	-.145*	-.092	-.055	-.034
			(.055)	(.053)	(.054)	(.054)	(.054)	(.057)
X_7-PA				-.338*	-.323*	-.335*	-.345*	-.328*
				(.057)	(.056)	(.055)	(.054)	(.056)
X_8-ED					-.300*	-.059	-.047	-.030
					(.077)	(.097)	(.095)	(.098)
X_9-OCC						-.369*	-.351*	-.344*
						(.094)	(.092)	(.095)
X_{10}-INC							-.215*	-.197*
							(.052)	(.055)
X_{11}-SP								-.067
								(.062)
X_{12}-SOC								-.013
								(.053)
R^2	.010	.029	.057	.152	.191	.230	.270	.273

Table 3. (Continued)

B. X_{14} - POWERLESSNESS

Regressor Group:	1	2	3	4	5	6	7	8
Variables[a]								
X_1-FOCC	-.036	-.001	.010	-.103*	-.096	-.044	-.044	-.073
	(.060)	(.059)	(.059)	(.051)	(.051)	(.050)	(.050)	(.043)
X_2-FED	-.301*	-.295*	-.300*	-.279*	-.271*	-.285*	-.285*	-.292*
	(.071)	(.070)	(.069)	(.058)	(.059)	(.056)	(.056)	(.049)
X_3-MED	-.080	-.031	-.006	.134*	.139*	.109	.109	.118*
	(.066)	(.066)	(.066)	(.058)	(.058)	(.055)	(.055)	(.048)
X_4-MA		-.195*	-.141*	-.144*	-.132*	-.103	-.103	-.132*
		(.055)	(.063)	(.054)	(.055)	(.053)	(.053)	(.046)
X_5-AP			-.127*	-.002	.030	.103	.103	.049
			(.063)	(.055)	(.063)	(.061)	(.061)	(.054)
X_6-XACT			-.144*	-.199*	-.185*	-.123*	-.122*	.013
			(.051)	(.043)	(.045)	(.044)	(.045)	(.041)
X_7-PA				-.519*	-.516*	-.530*	-.530*	-.424*
				(.047)	(.047)	(.045)	(.045)	(.041)
X_8-ED					-.067	.220*	.220*	.346*
					(.065)	(.080)	(.080)	(.071)
X_9-OCC						-.439*	-.439*	-.417*
						(.077)	(.077)	(.069)
X_{10}-INC							-.002	.115*
							(.044)	(.040)
X_{11}-SP								-.406*
								(.045)
X_{12}-SOC								-.129*
								(.038)
R^2	.140	.173	.203	.426	.428	.483	.483	.616

Correlation of residuals for X_{13} and X_{14} = .355.

[a]X_1 father's occupation; X_2 father's education; X_3 mother's education; X_4 mental ability; X_5 academic performance; X_6 extracurricular activities; X_7 personal adjustment; X_8 education; X_9 occupation; X_{10} income; X_{11} social participation; X_{12} socializing; X_{13} self-estrangement; X_{14} powerlessness

*Absolute value of coefficient is at least twice as large as the standard error and the relationship is defined as statistically significant.

is evident in that X_7—adolescent personal adjustment has a strong negative effect on both self-estrangement and powerlessness as revealed in the large increments to R^2 in Table 3 (compare for equations 3 and 4). Moreover, the total effects are largely unmediated by intervening processes, as given in equations 5 through 8. On the assumption that personal adjustment is an adolescent analogue to adult alienation, the beta coefficients are interpretable as stability coefficients, and we note that the psychogenic antecedent is apparently more stable than is the sociogenic behavior indexed by social integration.[19] Whether or not one prefers to interpret the relationship as we have, the effect of personal adjustment, which doubles the variance accounted for in powerlessness and nearly trebles that explained in self-estrangement, supports our thesis that models ignoring the psychogenesis of alienation are improperly specified.

The highly educated are not self-estranged, though the negative total effect of X_8—education is explained by occupational variation in alienation. The reduced-form estimates reveal no education effect on powerlessness; although, among men of comparable occupational status, the highly educated are more inclined to see themselves as powerless. The net effect runs counter to the sign of the zero-order correlation and the hypothesis that socioeconomic statuses are inversely related to alienation. Statistically controlling on X_{11}—social participation and X_{12}—socializing magnifies the positive effect. Higher levels of education (net of other socioeconomic statuses) either raise one's expectations for control or lower one's sense of control, net of the effects of mental ability and social integration. In either case, higher levels of education are accompanied by diminished sense of control.

Occupational socioeconomic status is negatively related to both forms of alienation, as predicted. Moreover the effect, unlike that of education, is primarily direct, not mediated by social integration. Higher status occupations are marks of success which have the independent effect of rewarding incumbents with a sense of fulfillment and a feeling of mastery (net of the negative education effect).

The predicted net negative effect of income on self-estrangement is only slightly mediated by adult social integration, which acts as a suppressor in the case of powerlessness. Within levels of ability, education and occupations, those with higher earnings feel more powerless. Perhaps those highest paid in each occupation shoulder heaviest responsibilities and face greater challenges, for which they perceive a higher probability of failure. Incorporating the socioeconomic effects increases the variances explained to R^2 = .270 and .483 for self-estrangement and powerlessness, respectively.

The adult social integration dimensions, X_{11}—social participation and X_{12}—socializing, have no total and direct effects of self-estrangement, although the zero-order correlations were negative and consistent with our expectation. Both dimensions have a negative effect on powerlessness and raise explained variance some 13 percent (R^2 = .616). The more one is socially integrated, the better he assimilates social expectations and aligns his aspirations with his resources. Adult social integration assists the individual in reconciling the real with the ideal with respect to the value of mastery and control, but it offers the individual little by way of finding meaning and a sense of fulfillment in his daily activities.

The correlation of residuals for self-estrangement and powerlessness is r = .355, which indicates that our model does reveal limited common antecedents—social and psychological—to the two alienation dimensions ($r_{13,14}$ = .401).

Summary and Discussion

We have estimated a structural equation model for identifying the antecedents of two variants of alienation, self-estrangement and powerlessness. The model incorporates socio- and psychogenic influences over a fifteen-year interval. The limited efficacy of the model is demonstrated by its predictive power in accounting for 27 percent of the variance in self-estrangement and 62 percent of the variance in powerlessness; it reduces the covariation between the two forms of

[19] The tentativeness of this observation is underscored by the fact that the comparison of standardized coefficients is made over different equations.

alienation from r = .401 to r = .355, the latter being the correlation of residuals.

We define alienation as a subjective phenomenon based on an individual's perceptions of his own realization of valued outcomes. Alienation is perceived discordance between the real and the ideal. We argue that alienation is a consequence of inadequate socialization and that whatever interferes or facilitates socialization processes, whether structural or psychological, has potential consequences for individual perceptions and self-reports of alienation. Those in higher socioeconomic statuses hold values consistent with the dominant subculture and have the advantage of resources with which to achieve that to which they aspire. The socially integrated have more access to the communication and interaction patterns of the larger society which provides the context for learning cultural expectations. Respondent's psychological characteristics have an independent effect on alienation inasmuch as they affect his perceptions and learning.

Our analysis identifies the socio- and psychogenic antecedents of two forms of alienation, provides evidence for the multidimensionality of the construct and partially accounts for the covariation between self-estrangement and powerlessness in common antecedents. The net effect of being reared in a family of higher socioeconomic status is to feel more in control of one's circumstances. Families of higher socioeconomic statuses have resources with which to indulge themselves. This sense of mastery is conveyed from father to son. Family background statuses (a social setting) differentially affect the two variants of alienation. Similarly, the net effect of mental ability (a psychological variable) is to contribute to self-estrangement on the one hand, but a sense of control, on the other. The competent appear to master their environment, but they also seem to hold out perhaps excessively high personal aspirations. The psychogenic effects of mental ability are complemented by the sociogenic effects of academic performance. Thus, the antecedents of self-estrangement are both social and psychological.

The psychogenesis of alienation thesis is clearly supported in the case of personal adjustment. The better adjusted the individual, the better he learns societal expectations and to accommodate his aspirations to his resources. Personal adjustment diminishes both variants of alienation. The socioeconomic indicators differentially affect the alienation variants and force a far more differentiated set of propositions about the relation of socioeconomic status to alienation than we have seen to date. The total negative affect of education on self-estrangement is absorbed by the centrality of the male's work role wherein he finds meaning. Moreover, males are socialized into believing that income is a quantifiable index of success; thus, earnings are positively associated with sense of fulfillment. With respect to powerlessness, however, the patterns differ. Higher prestige occupations require the manipulation of ideas and the productive use of resources, which foster a sense of mastery and control; but within occupations, education and income contribute to powerlessness. As one becomes schooled in the intricacies of international politics and economics, for example, he is increasingly awed by the complexity and indeterminacy of the multiple factors involved. Those with higher earnings within occupations and levels of education may be those who wrestle with the high risk challenges. Whatever sense of control develops within occupations is more than absorbed by education and income. Finally, as predicted, adult social integration effectively lowers sense of powerlessness. Social interaction exposes one to models and information about what constitutes realistic control as opposed to unrealistic aspirations, but it does not enable the individual to find personal meaning in life.

The fact that indicators of social integration differentially affect alienation repudiates the "logic of industrialism" thesis (see the critical review by Goldthorpe, 1969), which argues that as societies become increasingly industrialized, there is a tendency for crystallization of individuals and groups across stratification orders. One's sense of well-being is presumed to be dependent upon the congruence of his position across several orders of integration which form a tight nexus of placements in an overall stratification hierarchy. Our conception of integration is that society offers rewards other than prestige and income and that noneconomic rewards also function to bind the individual to the group. Our analysis of the effects of differentiated indicators supports the thesis

that integration may be achieved in a variety of substitutable ways (Hodge, 1970).

A methodological caveat is in order. As is often the case with sociological data, we were not able to reproduce perfectly the input matrix for all indicators from the factor matrix. Although the residuals are effectively zero in most instances, some differences do appear primarily with respect to the personal adjustment and alienation indicators. Most of the residuals are within the range of sampling error; but the chi square statistic (Jöreskog, 1970:241), significant at $p = .001$, indicates that the probability of the residuals appearing by chance is remote. Another factor may be required to account for the correlated error; alternatively, there is the possibility of an inflated chi square due to sample size.

Finally, we have not addressed the issue of the structure of alienation other than to assume the existence of correlated variants. Others may entertain alternative specifications of the relationship between the variants reasoning, for example, that powerlessness and self-estrangement are causally or reciprocally related. Unfortunately, there exists precious little direction from theorists instructing specification of the structure of alienation.

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THE EFFECTS OF INTERNATIONAL ECONOMIC DEPENDENCE ON DEVELOPMENT AND INEQUALITY: A CROSS-NATIONAL STUDY*

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The research reported in this paper studies the effects of a nation's dependent position in the world economy on its economic development and income inequality. Two kinds of international economic dependence are studied: investment dependence, the penetration of a country by foreign capital and debt dependence, the dependence of a government on foreign credit. The dependent variables studied include three measures of aggregate economic development: gross national product per capita, kilowatt hours of electricity consumed and the percentage of the male labor force not employed in agriculture. In addition, two control variables are included in the analysis: domestic capital formation and the extent to which the national economy is specialized in mining. The research design employed is panel regression analysis, which utilizes data at two points in time (1950 and 1970). The results indicate that both types of international economic dependence have overall negative effects on aggregate economic development. A cross-sectional estimate of the dependence effects on income inequality produces positive, though not statistically significant effects. It is concluded that dependency theories predict the effects of inputs from advanced nations to less-developed ones better than neo-classical international economic theories or sociological modernization theories.

Dependency theorists claim that international economic dependence produces the "development of underdevelopment" in peripheral areas of the world economy. At the same time, the core-periphery distinction has led to revisions in the theory of capitalist development which focus on the world-system, rather than national societies, as the relevant unit of analysis. In combination these two ideas imply that while Marx's law of uneven development and pauperization may have been wrong for the core nations, it is correct for the world economy as a whole and for the peripheral nations in particular. The growing gap between core and periphery and the gross inequalities within peripheral countries suggest that this is indeed the case.

However, the hypothesis that dependence retards development has not been adequately tested by formal comparative research. Case studies have found evidence both contradictory and supportive of this hypothesis, as have the few previous comparative studies which have been done.[1] The related hypothesis that dependence increases inequality within peripheral countries has not been subjected to comparative test. This paper reports the results of a longitudinal comparative study of the effects of two kinds of international economic dependence on economic development and a cross-sectional test of the effects of dependence on income inequality.

INTERNATIONAL ECONOMIC DEPENDENCE

International power-dependence relations range from direct military force, through

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[1] The previous comparative studies on dependence are: investment dependence–Szymanski, 1972; Evans, 1972; Kaufman et al., 1975; debt dependence–Szymanski, 1972; Griffin, 1970.

formal political subjugation (colonialism), to more subtle economic forms of power and influence such as foreign investment, foreign aid and trade relations based on a vertical division of labor. Although it is clear that these different forms are functionally interrelated, the focus of this research is on the more subtle neo-colonial types of economic dependence which predominated in the period studied—the *Pax Americana* from 1950 to 1970.

International economic dependence itself varies from direct penetration to indirect dependence resulting from location in a larger structure. The most direct economic penetration by core nations of peripheral areas is through private investment by transnational corporations which directly own and control the process of production. Less direct influence is exercised by foreign aid programs and credit agencies. Indirect economic dependence results from a nation's location in a restricted position in the world trade network or its specialization in a marginal role in the international division of labor.

This research examines the economic effects of two specific types of dependence: investment dependence and debt dependence. These two types of economic dependence are positively correlated (.63) and they may have similar effects on economic development and income inequality.

THEORETICAL APPROACHES TO DEPENDENCE AND DEVELOPMENT

Uneven development and international power-dependence relations

Myrdal (1957) points out that capitalist development unleashes market forces which create both "spread" effects by which growth in one area or sector creates development in other areas and "backwash" effects which drain resources out of the hinterland and concentrate them. In the developed nations (comprising what Wallerstein [1974a] calls the "core" of the world-system), class struggle and political processes have strengthened spread effects resulting in a more even distribution of development. In the periphery, however, backwash effects have been dominant, resulting in the concentration of development in the core and in enclaves attached to the core. At the level of the world-system as a whole, uneven development remains the dominant trend.

The operation of control structures between core-states and peripheral areas may reinforce this uneven development. Direct colonial control of peripheral areas has been replaced by the neo-colonial mechanisms of direct foreign investment and credit. It is my hypothesis that, other things equal, penetration by external control structures has a negative effect on economic development through the mechanisms reviewed below. The period studied in this research, 1950 to 1970, was a period of world economic expansion in which almost all national economies were growing (Meyer et al., 1975). The hypothesis is, then, that dependent nations grow less than others.

Before discussing theories which predict or imply hypotheses about dependence effects on development, it is necessary to clarify three possible misunderstandings. First, what many theorists understand to be simply resource flows between largely unconnected societies are considered by other theorists to be control structures which link superordinate to subordinate units in the same interactive system. This is especially true of the types of dependence studied here—dependence on foreign investment and foreign credit. These radically different ways of perceiving the same reality lead to completely different predictions about the consequences of such relations.

The second problem in discussing different theoretical approaches is that some theorists propose very long-run structural mechanisms by which dependence may affect development while others propose short-term effects. Both long-run and short-term hypotheses are reviewed because the research design used in this study is germane to both.

Third, some theories deal with the effects of international power-dependence relations in general, others deal specifically with investment and debt dependence. Again both types are discussed as the research is relevant to both, although more directly to the latter.

The Theory of Imperialism. Marx (1967) saw the historical relationship between the centers of capitalist development and the

hinterlands of the world in terms of the notion of primitive accumulation. Part of the impetus for the emergence and domination of the capitalist mode of production was the appropriation by European powers of the wealth of Africa, Asia and the Americas. Marx thought that the domination of the capitalist mode of production would spread throughout the world economy battering down all Chinese walls with the cheap prices of its commodities. Colonialism would destroy the pre-capitalist mode of production and each nation would develop the way England had.

Lenin (1965) introduced the concept of imperialism into the Marxist analysis of capitalist development. He pointed out that the exploitation of peripheral areas was an important dimension of capitalist development rather than a passing phase. Lenin saw monopoly capitalism as needing to export capital and to appropriate raw materials and markets in the periphery. He predicted inter-imperialist wars resulting from the division and redivision of the periphery by the advanced capitalist powers. Lenin expected that the original centers of capitalist development in Europe would become decadent and that the source of industrial strength would move to the periphery.

Dependency Theory. The persistence of underdevelopment in the periphery led to the reexamination of the theory of imperialism and monopoly capitalism by Baran (1956). Baran put forth the theory that penetration of the periphery by core-capitalism actually creates obstacles to development. Later Frank (1969) was to term this idea the "development of underdevelopment," implying that the poverty of the periphery is an ongoing modern process not a primordial state. The literature on the mechanisms by which interaction between core and periphery has retarded the latter has grown greatly in recent years. Dependency theory deals with three main topics: exploitation, structural distortion and suppression of autonomous policies.

a. Exploitation of the periphery by the core

Capitalist development on a world scale involves competition among different actors (capitals and states) for shares of the economic surplus. New resources are created, but at any point in time the amount of extant surplus is finite. Frank (1969) argues that penetration of the periphery by foreign investment drains surplus from the periphery to the core through the repatriation of profits and interest. This backwash effect accumulates capital in the core and underdevelops the periphery.[2] Amin (1974) contends that this "decapitalization" results from a continuing process of primitive accumulation in which political and military force backs up economic advantage in core-periphery relations.

Emmanuel (1972) contends that exploitation is hidden in the prices at which commodities from the periphery are exchanged for commodities from the core. This "unequal exchange" stems from the different wage structures in the core and the periphery. In the periphery, a worker must work, say, two hours to produce what is exchanged for a product which a core worker produces in one hour—and at the same level of productivity. Thus, the market prices of core-periphery exchange contain a transfer of value to the core.

Some theorists argue that unequal exchange and uneven development will occur in any system of interaction in which the distribution of power is unequal (Baumgartner et al., 1975). This happens because more powerful actors will use their power to determine outcomes to their advantage. This approach seems to apply to any situation in which the welfare of the less powerful is not heavily protected by social or political institutions. Certainly the world economy is such a system. If foreign investment and credit are control structures between superordinate and subordinate actors in a competitive system, they should cause uneven development.

Thus, exploitation of the periphery by the core is hypothesized to occur by means of decapitalization, unequal exchange and subordination to external controls in a competitive system. These mechanisms are thought to retard the development of the periphery.

[2] Pinto and Knakal (1973) show that, between 1960 and 1968, profit remittances to the United States from Latin America exceeded new investment by 6.7 billion dollars.

b. Structural distortion of the peripheral economy

A number of dependency theorists argue that the economic structure which emerges in the periphery as a result of the world division of labor is distorted in such a way as to create obstacles to development (Amin, 1974; DosSantos, 1970). An outward oriented economy specializing in the production of raw materials for export does not develop a differentiated internal structure. Frank (1969) observes that the infrastructure created by colonialism and foreign investment is oriented toward the ports of exit. Railroads, roads, telegraph lines, etc. all function to carry raw materials out of the country and return processed goods. This retards the integration of the national economy by linking the different areas and sectors of the peripheral countries with the external world rather than with one another (Ehrensaft, 1971). The multiplier effect, by which demand in one sector or area of a country creates demand in another, is weak because externally oriented linkages soon transfer demand to the international economy (Singer, 1971).

The peripheral areas have been integrated into the world economy as producers of raw materials for export to the industrial countries. Peripheral areas often specialize in a single raw material export, either agricultural or mineral. Galtung (1971) argues that an economy which is specialized in the production of raw materials will grow less than one in which production is more differentiated. Production which involves high levels of processing creates greater demand for inputs and greater opportunities for related economic activities on the output side of production (Hirschman, 1958).

Prebisch (1950) maintains that any national economy which remains undifferentiated will suffer from the vicissitudes of the international market, and that this is especially true of those specialized in the production of raw materials because the terms of trade for these commodities decline relative to manufactured goods and capital equipment produced in the core. Also, as there is more fluctuation in price for raw materials in the world market, instability makes economic planning (private or public) more difficult.[3]

In addition Griffin and Enos (1970) argue that dependence on foreign credit reduces the domestic marginal propensity to save and thus negatively affects economic growth by lowering domestic capital formation.

Beckford's (1971) analysis of the role of transnational corporations in plantation economies suggests that the short-term impact of these corporations increases national income and opens new areas of production, but the long-run institutional and structural effect is to distort the use of resources in the peripheral economy. Transnational corporations greatly affect land use, labor markets and the allocation of investments. Beckford maintains that the logic by which these corporations operate is derived from the scope of the enterprise as a whole, including processing and marketing operations in the core. Thus, they influence the allocation of resources in the periphery from this point of view, using market imperfections (monopoly power in sales and consumption) to guard against the risks of natural or political disaster. Their interest in maintaining relatively cheap labor, low taxes and the freedom to maneuver is often inconsistent with the balanced development of the peripheral country in which they produce. The distortion of resource use patterns creates what Beckford calls a state of dynamic underdevelopment. Thus dependence is hypothesized to distort the economic structure of the periphery in the following ways: specialization in raw material production (low differentiation); outward-oriented infrastructure (low integration); the creation of resource use patterns which retard economic development.

c. Suppression of autonomous policies in the periphery

Some dependency theorists have argued that dependence retards development

[3] Barratt-Brown (1974) has shown that the terms of trade alternately worsen and improve for Third World raw material producers over the last 100 years. Wallerstein (1974b) has argued that opportunities for upward mobility occur for some "semi-peripheral" nations during periods of contraction in the world economy. The recent economic downturn and the price rise for raw materials may indicate the return of such a period. Delacroix's (1975) cross-national study finds that specialization in raw materials has no effect on economic growth.

through its suppression of autonomous policies in government and business which would mobilize balanced development (Johnson, 1972). Baran (1956) contends that dependence distorts the development of a national bourgeoisie. Merchants, with their stake in the export of raw materials and the import of manufactured goods, combine with landed classes (which have similar interests) to prevent the emergence of a domestic manufacturing or industrial bourgeoisie. They do this by politically preventing the introduction of tariffs which would protect infant industries against the competition of already developed producers in the core-states. Frank (1969) sees this process in the history of Latin American development. Civil wars occurring around 1830 in most of the newly independent republics involved the issue of autonomous versus external orientation. In each case, the forces of "free trade" and external orientation triumphed. Wallerstein (1972) makes a similar case for the peripheralization of Poland in the sixteenth century European "world-economy."

Other theorists maintain that strong links between elites in the periphery and core elites form what Galtung (1971) calls a "bridgehead" of interests and connections. The external orientation of these "liaison elites" and the fact that their power and interests are based on connections with the core-states means that they suppress policies and leaders seeking to mobilize balanced autonomous development.[4] According to Hayter (1971), international and transnational agencies which lend to peripheral countries (development banks and private banks) stand behind the operations of transnational companies in maintaining "a good climate for foreign investment."

Sunkel (1973) claims that the connections of ruling groups in the dependent periphery with core-states and transnational corporations create a political structure which keeps wages low and concentrates development in the international sector. The links between the core and elites in the periphery increase income inequality by (1) raising the incomes of elites and (2) keeping the wages of workers low. The power of the elites in dependent peripheral countries is backed by their alliances with the core, so they are able to suppress demands for higher wages and income redistribution. Thus dependence creates a political situation which retards development by linking elites in the periphery to the interests of the core. This prevents the emergence of autonomous forces seeking to mobilize balanced development and maintains extreme inequalities in the periphery.

Theoretical Approaches Which Hypothesize Positive Effects of Dependence on Development

Most neo-classical economists and modernization theorists in sociology see that dependence as resource flows or the diffusion of modernity from advanced to backward societies. The notion that foreign investment and credit constitute a form of domination is seen as paranoid and reactive and as a remnant of the unfortunate colonial past (Viner, 1952). Occasionally the reality of foreign control is acknowledged, however, as more than a political misconception. The following quote from Lester Pearson (1969) compares debt dependence to dependence on foreign investment.

> Another common misconception is that loan financing leads to less foreign control over the host countries' economies than equity investment. Actually, fixed interest creditors are in a far stronger position with respect to national authorities than are foreign equity investors, who can often be influenced by fiscal, monetary, and other policies.

This comparison of the control aspects of debt and investment dependence is not seen by Pearson as inconsistent with the theories reviewed below which predict positive effects of dependence on the economic development of peripheral countries.

Neo-classical international economics. In neo-classical international economics, the flow of capital from advanced to less-developed nations is understood to be a

[4] Trimberger (1976), in a study of military bureaucrats and economic nationalism in Japan, Turkey, Egypt and Peru, argues that autonomous leadership can be effective in modifying the worst aspects of dependence.

main engine of economic growth. Capital is capital and its investment should lead to increased production in the enterprise into which it is channeled, as well as economic growth in other sectors due to increased demand. Unrestricted international flow of capital to areas where it will bring the highest return will result in the maximization of growth for the system as a whole and presumably also for the peripheral areas to which the capital flows. The benefits of foreign investment will spread due to incomes created by new employment and the "trickle-down" effect caused by increased demand for land, labor and materials (Schelling, 1958). The input of foreign aid should result in growth for the same reasons and also because it supplements local savings and makes greater investment possible (Chenery and Strout, 1966). The use of aid funds to build public works and infrastructure should have positive effects on later growth.

The theory of comparative advantage holds that specialization in the production of raw materials (one of the correlates of dependence) will not have negative effects on growth if it is more economical to exchange raw materials for manufactured goods than to produce the imported goods domestically. Ricardo (1933) put forward the theory of comparative advantage to show why it was mutually advantageous for nations to trade. The example he used was the exchange of English linen for Portuguese wine.[5] The theory holds that "whether or not one of two regions is absolutely more efficient in the production of every good than is the other, if each specializes in the products in which it has a comparative advantage (greatest relative efficiency), trade will be mutually profitable to both regions" (Samuelson, 1964). This is in direct contradiction to the idea of "structural distortion" discussed above.

Modernization theory. Most sociologists who study development and modernization focus almost exclusively on processes internal to national societies (Eisenstadt, 1966; Stanley, 1972; Smelser, 1963). The exceptions to this are those theorists who discuss borrowing or diffusion of modern traits from advanced to premodern societies. Bendix (1964) discusses the way imperial Meiji Japan modeled its constitution after that of imperial Germany. Parsons (1971) also focusing on diffusion, claims that

> ... the "imperialist" phase of Western society's relationship with the rest of the world was transitional. The trend toward modernization has now become worldwide. In particular, the elites of most nonmodern societies accept crucial aspects of the values of modernity, especially economic development, education, political independence, and some form of "democracy."

Modernization theorists also emphasize the transfer of advanced technology, modern rational organizational forms, labor habits complementary to industrial production and "modern" attitudes toward the self, the family and the society which facilitate economic development (Moore and Feldman, 1960). This approach implies that a country which is penetrated by direct foreign investment (has located within it subsidiaries of modern transnational corporations) should develop more than a country that is not so penetrated. Similarly, foreign aid which brings technical assistance and advice regarding fiscal and development policies should also facilitate economic growth.

Summary of Propositions

Dependency theorists predict a negative effect of dependence on economic growth and a positive effect on income inequality. Neo-classical economic theories and modernization theories predict a positive effect for economic growth and a negative effect on income inequality. The dependency literature reviewed above discusses mechanisms by which international power-dependence relations in general have effects on development. The following propositions relate specifically to the types of dependence studied in this research – investment and debt dependence.

[5] Sideri (1970) has made an historical investigation of the emergence of this exchange and its consequences for the development of the two countries. This exchange was imposed on the Portuguese by the British in exchange for protection of the Portuguese colonial relationship with Brazil. This division of labor is seen by Sideri as one cause of the Portuguese failure to industrialize.

Mechanisms by which investment and debt dependence negatively effect economic growth:

(a) Exploitation by the core drains resources from the periphery which are needed for its development. Profits on foreign investment and interest on credit transfer value from the periphery to the core and retard the development of the periphery.

(b) Externally oriented production and penetration by transnational corporations distort the economic structure of the periphery. Differentiation and integration of national economies are obstructed and a pattern of resource use is created which maintains a state of dynamic underdevelopment.

(c) Links between elites in the core and the dependent periphery act to suppress autonomous mobilization of national development.

Mechanisms by which foreign investment and credit positively effect economic growth:

(a) Foreign capital creates production directly in the enterprise in which it is invested and generates demand for other inputs which contribute to economic growth.

(b) Foreign credit finances public infrastructure needed for development.

(c) Diffusion of technology, work habits, modern organizational forms; modern attitudes and consumption preferences stimulate economic development.

Mechanisms by which investment and debt dependence affect income inequalities within dependent countries:

(a) Positive effect – In peripheral countries penetrated by external control structures, the ruling groups are able to obtain a large share of the national income and to prevent income redistribution because their power is backed up by alliances with the core.

(b) Negative effect – Foreign investment and credit expand the wage-earning working class and the salaried middle class which enlarges the middle of the income distribution and lowers overall inequality.

Let us turn now to a comparative test of dependence effects on economic development. This enables us to estimate whether aggregate effects are positive or negative, but does not differentiate between all the mechanisms hypothesized above. Any estimated aggregate effect may be the resultant of the simultaneous operation of different mechanisms, but findings which are consistently negative will direct further attention to dependency theory.

RESEARCH DESIGN

Panel Regression Analysis

Panel regression analysis is a flexible statistical method for testing causal propositions which allows the introduction of multiple independent variables and, because it employs data at different points in time, reduces the likelihood of false inferences due to reciprocal causality.[6] The dependent variable is measured at both the first and the second point in time (in this study, 1950 and 1970).[7] The independent variables are measured at the earlier time point. The dependent variable at the later time point (t) is regressed on itself at the early time point (t-1) and on the independent variables at t-1.

This provides an estimate of the effect of the independent variable which is "independent" of variance in the dependent

[6] Szymanski's finding of a positive relationship between investment dependence and economic growth in Latin America may have resulted from this type of error. He measured dependence in 1969 and the change in GNP per capita from 1960 to 1968. Thus the positive effect which GNP per capita has on investment dependence among poor nations combines with the negative effect of dependence on development to produce Szymanski's small positive result. Chase-Dunn (1975) shows that the level of development of a country influences the influx of foreign capital. In the periphery, foreign capital tends to flow where there is already greater economic activity and so economic development is positively correlated with investment dependence and increases it over time.

[7] The twenty-year time lag has been chosen because it is the longest period over which quantitative data are presently available. There is enough variation in economic development over this period to allow the effects of independent variables to show up in panel analysis. Events such as changes in independent variables in the period between measurements could alter the results, but the use of shorter lags reveals results which are smaller but in the same direction as those shown in this paper. Structural variables such as those under investigation require a substantial time lag to produce measurable effects.

variable at t-1. It is an estimate of the effect which the independent variable has on the dependent variable over the period of time between the two measurements. The length of time between the two measurements is referred to as the "lag," and the t-1 measurement of the dependent variable is called the "lagged dependent variable." The basic equation for this panel model is:

$$Y_t = a + bY_{t-1} + bX_{i(t-1)} + U_t$$

where Y_t = dependent variable
Y_{t-1} = lagged dependent variable
a = constant
b = unstandardized regression coefficient
$X_{i(t-1)}$ = measure of economic dependence
U_t = disturbances affecting Y_t but unrelated to other variables in the equation.[8]

If it is hypothesized that a variable not in the equation is causing a spurious estimate of the effect of the independent variable, it can be controlled by including it in the equation. Failure to include such a variable may lead to faulty inferences.

Weighting of Variables

Measures of dependence used in the tables reported below have been weighted by population size (per capita) rather than on economic size (per GNP) of the nation in order to make a conservative test of the hypothesis of a negative effect of dependence on development. Weighting with population size rather than economic size should bias the estimate toward the positive if there is an artifact due to ratio measures having common terms.[9]

Sample of Nations

The theoretical literature on dependence effects focuses on less-developed countries and so our sample of nations consists of those with less than $406 per capita GNP in 1955. This excludes the richest nations from the analysis while leaving enough cases to allow meaningful regression estimates. Thus we are comparing poor, dependent nations with poor independent or isolated nations. A parallel analysis including the rich nations is reported in Chase-Dunn (1975). The results are very similar to those presented below.

MEASUREMENT

The research design employed in this study requires that data be gathered at two points in time – 1950 and 1970. A great quantity of comparable data have been produced over the last twenty-five years by

[8] This panel model results in an upward-biased estimate of the autoregression term (the estimated effect of the lagged dependent variable on the dependent variable) because of autocorrelation of error terms associated with Y_t and Y_{t-1}. The measurement errors involved in measuring the dependent variable at both time points are correlated, thus violating one of the assumptions of the model–that error terms are uncorrelated. This results in an upward bias of the estimate of the autoregression and a downward bias of the estimates of those independent variables which are positively correlated with the lagged dependent variable. Since both measures of dependence are positively correlated with economic development, this should bias the estimated effect of dependence toward zero. Therefore the panel regression model should provide a conservative test of the hypothesis that dependence affects development.

[9] It is argued that using ratios as indicators may lead to faulty inferences, especially if the same terms appear in both independent and dependent variables. Thus a correlation between two indicators that are both weighted by population (such as GNP per capita and doctors per capita) may be artificially high because both indicators contain population in the denominator. Likewise, two measures that contain a similar term which is in the denominator of one and the numerator of the other will have an artificially negative correlation (Schuessler, 1974). The extent to which this is true depends on the variances of the terms (Fuguitt and Lieberson, 1973). In the case of estimating the effects of dependence on development, it is possible that negative effects such as those found by Evans (1972) are artifactual because dependence measures are standardized on GNP to control for economic size, while the dependent variable is GNP per capita. This is the case in which the same term is in the denominator of one measure and the numerator of the other. In order to provide a conservative test of the hypothesis that dependence has a negative effect on development, the dependence measures are weighted with population. This is because, according to the argument that an artifact results from a common term, the per capita measures should create a positive artifact.

international organizations. Secondary analysis of these data require that a fit be made between the information available and the theoretical variables of interest.

Measurement of International Economic Dependence

Investment dependence per capita – the penetration of a nation by direct private foreign capital investment – is measured by employing an item from the International Monetary Fund Balance of Payments Yearbook: (1950-55) "Debits on investment income" in U.S. dollars. It reports all profits made by foreign direct investment in the "host" country (regardless of whether or not they are repatriated). The average debits on investment income are computed for the period from 1950 to 1955 (to smooth out short term variation) and divided by the population. This variable, due to its badly skewed distribution, is converted to a logarithmic scale to make it suitable for use in linear regression analysis. Summary statistics on the measures of investment and debt dependence are contained in the Appendix.

Debt dependence per capita – the dependence of a nation on foreign credit – is measured by the total "external public debt," which is composed of loans to the government and government-guaranteed loans (IBRD, World Bank, 1971). The per capita distribution of this variable is not badly skewed, so it is unnecessary to convert it to a logarithmic scale. Since these data are not available before 1965, we are forced to use a shorter time lag for our panel regression estimates.

Measurement of Economic Development

We use three conventional measures of aggregate economic development:

(1) Log Gross National Product per capita for 1950 and 1970 (IBRD, 1971; 1973). This measure and the measure of kilowatt hours have been converted to a logarithmic scale because of the extreme right-skewness of the unlogged distributions.

(2) Log kilowatt hours of electricity consumed per capita for 1950 and 1965 (Taylor and Hudson, 1971).

(3) The percentage of the male labor force not employed in agriculture for 1950 and 1960. These data, based on national censuses, are reported by the International Labour Organization (I.L.O., 1971).

Measures of Other Causal Variables

There are two additional variables introduced into the equations estimating the effects of dependence on aggregate economic development. They are hypothesized to be related to both dependence and development and so they must be included in a model estimating the effect of dependence on development.

(1) Domestic Capital Formation – a measure of local savings, domestic capital formation as a percentage of Gross Domestic Product.[10] It is found for 1960 in Taylor and Hudson (1971).

(2) Specialization in mining – the percentage of Gross Domestic Product produced in the mining sector shows the extent to which a national economy is specialized in the production of mineral raw materials. It is found in data presented by the World Bank for 1955 (IBRD, 1971).

Measure of Income Inequality

Data on income inequality are not available for different time points so panel analysis which uses it as a dependent variable is not possible.

Individual and household income inequality–a Gini index computed from the distribution of national income to quintiles of the population, from the poorest 20% to the richest 5%. It has been gathered by Adelman and Morris (1971) and improved by Paukert (1973) resulting in 58 cases available for around 1965.

RESULTS

First, let us consider the cross-sectional correlations between debt dependence, investment dependence and economic development. The correlation between investment dependence and debt dependence is .63 indicating that they are somewhat different aspects of economic dependence.

[10] Gross Domestic Product is GNP plus net factor payments to abroad. It includes income which returns to non-nationals, including profit and interest on foreign investment and loans (O'Loughlin, 1971).

Gray (1969), however, claims that foreign aid and credit, rather than displacing foreign private investment, facilitate it by providing public infrastructure which is useful to the foreign enterprise. Hayter (1969) contends that international lending agencies work hand in hand with transnational corporations to maintain open conditions for the foreign investor. Both authors would agree that debt and investment dependence are complementary rather than functional alternatives.

Both investment dependence and debt dependence are positively correlated with measures of economic development in the sample of poor nations. This is because a number of very poor nations are not dependent on foreign investment and credit, that is, they are relatively isolated from neo-colonial control structures. Foreign capital tends to flow where there is already greater economic activity; thus the correlation between dependence and development is positive (see footnote 6). Similarly, debt dependence is higher among more developed peripheral countries because credit is extended on the basis of economic potential. Previous research has interpreted these positive cross-sectional correlations as evidence that dependence facilitates development. Panel analysis enables us to separate the reciprocal effects of dependence and development, that is, to estimate the effect of dependence on development separately from the effect of development on dependence.

Effects of Investment Dependence on Economic Development

Let us look now at the panel regression estimates of investment dependence effects on three measures of economic development in Table 1. The lags differ between dependent variables so it is difficult to compare the size of the effects. The direction (positive or negative) is the main question.

The regression coefficients estimating the effects of investment dependence are negative for all three measures of economic development (betas = -.22; -.12; -.02) indicating that investment dependence retards development. For GNP per capita and kilowatt hours of electricity per capita, the estimates are statistically significant at the .05 level. The estimated effect on the percentage of the labor force in non-agricultural production is small and not statistically significant, but this measure of occupational structure changes slowly and there is only a ten-year lag, as data are not yet available for 1970.

Beckford's (1971) contention that foreign investment has immediate positive effects, but long run negative effects on development is supported by the finding that longer lags with the same dependent variables produce larger negative estimates (Chase-Dunn, 1975).

The stability terms in the panel regressions (the estimates for Y_{t-1}) are always large and thus significance levels are not indicated for these coefficients in the tables. The amount of variance in the dependent variable accounted for by panel regression analysis is always very high (above .70), so no measure of explained variance is reported.

The estimated effect on Log Kilowatt Hours in Table 1 is for a fifteen-year lag (1950-1965). Data are available for 1970 on this variable, but for fewer cases. The estimate using the 1970 measure of Kilowatt Hours is also negative (beta = -.15).

The model examined in Table 1 assumes that other variables are not interacting with investment dependence and economic development. However, specialization in mining and capital formation may interact with investment dependence and economic development to change the effects shown in Table 1. Therefore, there is a need to introduce these two variables into the model.

First, we know that core-states need to acquire raw materials, so foreign investment is attracted to countries with substantial mineral resources. This means that the effect of having mineral resources is confounded with the effect of investment dependence in Table 1.

In Table 2 we introduce another independent variable, the specialization of a nation in mining, including the production of oil and other minerals. The correlation between investment dependence and specialization in mining is .40, which supports the idea that foreign investment is drawn to areas with mineral resources. Table 2 shows the estimated effects on three measures of

Table 1. Panel Regression Estimates of Effect of Investment Dependence on Economic Development

Y_t	Log GNP per capita 1970			Log Kilowatt Hours per capita 1965			%Non-agricultural Labor Force 1960		
	b	S.E.b	Beta	b	S.E.b	Beta	b	S.E.b	Beta
Constant	-.17			1.13			-6.7		
Y_{t-1}	1.32	.13	.99	.78	.05	.97	1.02	.03	.98
Investment Dependence 1950-55	-.097	.045	-.22*	-.087	.041	-.12*	-1.62	2.27	-.02
Cases	N = 38			N = 31			N = 46		

Y_t = Dependent Variable
Y_{t-1} = Lagged Dependent Variable, 1950

S.E.b = Standard Error of b
* = $P < .05$

Table 2. Panel Regression Estimates of Effects of Investment Dependence and Specialization in Mining on Economic Development

Y_t	Log GNP per capita 1970			Log Kilowatt Hours per capita 1965			%Non-agricultural Labor Force 1960		
	b	S.E.b	Beta	b	S.E.b	Beta	b	S.E.b	Beta
Constant	-.39			1.27			-13.7		
Y_{t-1}	1.50	.16	1.05	.76	.04	.98	1.07	.06	.99
% GDP in Mining 1950	.120	.046	.28**	.099	.039	.17**	.89	1.01	.05
Investment Dependence 1950-55	-.187	.059	-.41**	-.145	.044	-.22**	-2.27	1.15	-.12*
Cases		N = 25			N = 24			N = 25	

Y_t = Dependent Variable

Y_{t-1} = Lagged Dependent Variable, 1950

S.E.b. = Standard error of b

* = P < .05 ** = P < .01

economic development of both investment dependence and specialization in mining.

The estimates of investment dependence effects are all more negative and statistically significant in Table 2 than in Table 1. Thus, the hypothesis that investment dependence has negative effects on development is strongly supported. The effects of specialization in mining are positive on all three dependent variables and are statistically significant at the .01 level except for the effect on the percentage of the labor force in non-agricultural production. These results provide support for the contention that mineral resources facilitate economic development.

Second, it may be that investment dependence is related to the ability to generate local savings. If domestic savings for new investment are low, foreign capital may flow in to take up the available opportunities. At the same time the low propensity to save and invest may be related to other difficulties within a peripheral economy which retard economic development. If this were so it would produce a spurious negative estimate of the effect of investment dependence on economic development when domestic capital formation is not included in the model.

Table 3 shows the results of including a measure of domestic capital formation in an equation estimating the effect of investment dependence on development. Due to lack of data in 1950 on domestic capital formation, a shorter time lag for the dependent variable is used in Table 3. (The longer lag with fewer cases produces results very similar to those shown in Table 3.) The shorter time lag reduces the size of the estimated effect of investment dependence, but Table 3 presents the estimate both with and without domestic capital formation. It can be seen by comparing the estimates that the inclusion of domestic capital formation reduces the negative estimate of the investment dependence effect only slightly. This means that investment dependence has negative effects independent of its relationship with domestic capital formation.

The effect of domestic capital formation is positive on both GNP per capita and Kilowatt Hours per capita, although only the first estimate is statistically significant. The betas for investment dependence and domestic capital formation are strikingly similar in both equations, except for opposite signs. This is partly an artifact of the sample variances for the distribution of each measure, but it implies that variance in two types of economic input can, given different structural and institutional conditions, have opposite effects on economic development.

Effects of Debt Dependence on Economic Development

Data on external public debt are not available for most poor countries before 1965. A short-lag panel analysis can be made between 1965 and 1970, however.

Table 4 contains the results of an analysis which includes debt dependence and domestic capital formation as independent variables. Domestic capital formation is included because it may be related to the tendency to contract foreign debt and because it is known to effect economic development. The estimated effects of debt dependence are both negative, but only the effect on Kilowatt Hours is statistically significant. This is weak evidence that debt dependence has negative effects, but fairly strong evidence that it does not have positive effects.

The estimated effect of domestic capital formation, which was significant with the longer lag in Table 3, is not significant in Table 4 for either dependent variable.

Effects of Debt and Investment Dependence on Inequality

Dependence is thought to affect development in part through its effects on the class structure of the peripheral countries. It is hypothesized that liaison elites in dependent countries are supported by external power and thus the distribution of resources is very unequal. Only cross-sectional data on income inequality are available and they are not gathered at exactly the same time point for each case (Paukert, 1973). Even so, given the theoretical relevance of the proposition that dependence effects on development are connected with effects on inequality, a preliminary test of the effect on inequality is justified. Table 5 contains the results of the cross-sectional

Table 3. Panel Regression Estimates of Effects of Investment Dependence and Domestic Capital Formation on Economic Development

A. Without Domestic Capital Formation in the equation:

Y_t	Log GNP per capita 1970			Log Kilowatt Hours per capita 1965		
	b	S.E.b	Beta	b	S.E.b	Beta
Constant	-24			.19		
Y_{t-1}	1.18	.07	.97	.95	.02	.99
Investment Dependence 1956-60	-.052	.027	-.12*	.034	.018	-.04

B. With Domestic Capital Formation in the equation:

Y_t	Log GNP per capita 1970			Log Kilowatt Hours per capita 1965		
	b	S.E.b	Beta	b	S.E.b	Beta
Constant	-.16			.19		
Y_{t-1}	1.09	.08	.89	.94	.03	.98
Domestic Capital Formation 1960	.89	.35	.16**	.25	.29	.03
Investment Dependence 1956-60	-.044	-.024	-.11*	-.031	.019	-.04
Cases		N = 28			N = 28	

Y_t = Dependent Variable
Y_{t-1} = Lagged Dependent Variable, 1960
S.E.b = Standard Error of b
* = P < .05 ** = P < .01

Cases are those for which data are available on both Domestic Capital Formation and Investment Dependence.

analysis of the effects of investment and debt dependence on income inequality in peripheral countries. Included as dependent variables in both equations are specialization in mining and economic development—both thought to be related to dependence and inequality.

The estimates of the dependence effects are both positive but not statistically significant (although nearly so at the .05 level). This is weak support for the hypothesis that dependence maintains income inequality in peripheral countries.[11]

Summary of Results

The hypothesis that investment dependence retards economic development is

[11] The possibility that this result is due to reciprocal causation—a positive effect of inequality on investment dependence due to low wages or the policies of liaison elites—is partly ruled out by a panel analysis which includes income inequality as an independent variable. An estimate of the effect of inequality on investment dependence was computed by regressing investment dependence in 1966 on income inequality in 1965 while controlling for investment dependence in 1955. This revealed no effect of inequality on investment dependence, although this is not a conclusive result because of the time at which inequality is measured. If it may be assumed that inequality is fairly stable over this time period, this result rules out the hypothesis of a reciprocal effect which would account for the cross-sectional relationship shown in Table 5. This is additional support for the hypothesis that dependence causes inequality.

Table 4. Panel Regression Estimates of Effects of Debt Dependence and Domestic Capital Formation on Economic Development

Y_t	Log GNP per capita 1970			Log Kilowatt Hours per capita 1970		
	b	S.E.b	Beta	b	S.E.b	Beta
Constant	-.03			-.06		
Y_{t-1}	1.09	.07	.99	1.19	.14	1.03
Domestic Capital Formation 1960	.004	.27	.001	-.49	1.09	-.05
Debt Dependence 1965	-.007	.007	-.05	-.059	.028	-.22*
Cases	N = 33			N = 26		

Y_t = Dependent Variable

Y_{t-1} = Lagged Dependent Variable, 1965

* = P < .05

S.E.b = Standard Error of b

strongly supported by Tables 1, 2 and 3. Further investigation of dependence effects on growth in different economic sectors indicates that there are negative effects on production in agriculture and manufacturing, but that investment dependence has a *positive* effect on production in mining (Chase-Dunn, 1975). This suggests that de-

Table 5. Cross-sectional Regression Estimates of the Effects of Investment and Debt Dependence, Economic Development and Specialization in Mining On Income Inequality

A. Investment Dependence

Y	Gini Index of inequality of distribution of national income among individuals and households		
	b	S.E.b	Beta
Constant	38.59		
Log GNP per capita	-3.28	7.6	-.09
Specialization in Mining	.014	.018	.17
Investment Dependence	6.11	3.84	.44
	N = 31	r^2 = .27	

B. Debt Dependence

Y	Income inequality		
	b	S.E.b	Beta
Constant	45.62		
Log GNP per capita	-1.19	7.12	-.04
Specialization in Mining	.021	.018	.22
Debt	.011	.008	.31
	N = 30	r^2 = .18	

pendence effects are not uniform and that further research should be done to determine the loci of effects on development.

It is shown that debt dependence definitely does not facilitate economic development, and there is weak evidence that it retards it. It may require a longer time lag for the negative effects of debt dependence to become measurable by cross-national research. This question can be settled as more complete data are collected.

Table 5 provides some support for the hypothesis that dependence causes the unequal distribution of income. It should be added that the estimated effect of investment dependence on the income share received by the top five percent of the population is positive, while effects on the income received by the bottom three quintiles of the population are negative. This is further evidence that dependence provides support for elites in the periphery and keeps wages low relative to the income of elites.

THEORETICAL IMPLICATIONS

The findings reported above indicate that dependency theory must be taken seriously as an explanation for uneven development in the world economy. Power-dependence relations between core and periphery operate to reproduce the inequalities between national societies. In a competitive world economy, subjection to external controls is a disadvantage which retards relative development. This is shown to be true for direct penetration by investment dependence and is also true of indirect forms of dependence resulting from restricted location in the international trade network.[12] This effect may become larger in the period of world economic contraction which began in 1967. Peripheral nations which are more subject to core control may be prevented from taking advantage of opportunities for development which emerge in such a period.

The finding of a negative effect of investment dependence on economic development means that foreign capital must be seen as a form of control as well as a flow of resources. The structural and institutional context in which this economic flow is imbedded produce effects which are opposite those of domestic capital investment. The proposed mechanisms by which dependence retards development are not directly tested herein, and further research focusing specifically on intervening variables needs to be done. The most plausible explanation for the short-term effects of dependence is that proposed by Beckford (1971). Nations which are subjected to external controls cannot appropriate their own surplus capital for investment in balanced development. Transnational corporations operate to further their own growth, but not the development of the countries in which they are located. These corporations use their political and economic influence to keep labor costs and taxes low and to maintain the conditions for their continued profitable operation.

The preliminary finding that dependence maintains income inequalities suggests that one way in which dependence retards development is by linking national elites in the periphery to the interests of the transnational corporations and the international economy. Thus political and economic forces which attempt to mobilize balanced national development are suppressed.

We know that a great deal of foreign investment in peripheral countries goes into the extraction of mineral raw materials. Therefore the finding that investment dependence increases production in the mining sector (Chase-Dunn, 1975) suggests that foreign investment has positive effects on the enterprise in which it is invested. In combination with the finding of negative effects on aggregate economic development, this suggests that foreign investment combines positive direct effects on the enterprise in which it is invested with negative effects on the rest of the national economy, resulting in overall negative effects.[13]

[12] Export partner concentration, or the extent to which a nation's exports go to a single other nation, is also found to retard economic development (Chase-Dunn, 1975).

[13] Further support for this interpretation is provided by a cross-sectional analysis which estimates the effect of investment dependence on inequality of production in economic sectors. This is measured by the Gini index of the distribution of product per worker across eight economic sectors. The beta

In the light of these findings, oppositional movements and national policies in the periphery which stress self-reliance and careful control over inputs from the core must be seen as other than reactive ignorance. Beckford (1971) points out that independence is a necessary but not a sufficient condition for development in the periphery. Autarchy combined with political mobilization for development has been successful only in large states which control a big internal market, such as China and the Soviet Union. Small nations that cut themselves off from the world economy risk isolation and stagnation, e.g., Burma. Peripheral countries that try to break their dependency on the core also risk subversion or invasion, e.g., Chile.

One solution to the problem of dependence would appear to be the control of inputs from the core to assure compatibility with balanced development (Morley, 1975). A number of nations have made laws regulating foreign investment in an attempt to gain the benefits without the costs, e.g., Japan, Mexico, India and Yugoslavia. This strategy is difficult in a competitive world in which the transnational corporations have the upper hand. The larger scale on which transnational corporations operate gives them the advantage over small peripheral countries (Moran, 1973). The Balkanization of the periphery which is the legacy of the colonial empires makes solidarity among peripheral nations difficult. Competition for foreign investment, rather than concerted regulation of it, has been the main tendency.

Regional agreements to regulate core inputs such as the Andean group may be a good strategy for the periphery (Girvan, 1973). The example of the Organization of Petroleum Exporting Countries indicates that effective coordination of policies between peripheral states is possible, and this alternative may be more realizable with the economic downturn which began in 1967. A world-wide contraction of production with increasing competition between core-states has compromised the hegemony which the United States enjoyed during the *Pax Americana*. Increasing economic competition and political pluralism in the world-system, with more of a balance between competing core-states, may create opportunities for peripheral solidarity which have not heretofore existed.[14]

We have already seen manifestations of such solidarity in the OPEC and other producers' combinations, the growth of regional trade, investment and development agreements and the rise of economic nationalism in many peripheral nations. This growing awareness of the core-periphery contradiction and the effects of dependence may be the beginning of a political process which eventually will modify the grossly uneven development of the world economy.

coefficient estimating the investment dependence effect on sectoral product inequality is .78 and is significant at the .001 level for a sample of 26 poor nations (controlled for economic development and specialization in mining).

APPENDIX

Summary Statistics on Measures of Economic Dependence (for nations with less than $406 per capita GNP in 1955)

Log investment dependence per capita 1950-55:
Mean= 2.06 Standard deviation= .75
Skewness= -.08 Maximum= 3.26
Minimum= .68 N= 38

Debt Dependence per capita 1965:
Mean= $318 Standard Deviation=$261
Skewness= 1.2 Maximum= $1080
Minimum= $6 N= 55

[14] In earlier periods of contraction and conflict in the world economy, peripheral nations individually have moved toward political and economic independence (Frank, 1969; Ness and Ness, 1972).

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CULTURAL CONCEPTIONS, MENTAL DISORDERS AND SOCIAL ROLES: A COMPARISON OF GERMANY AND AMERICA*

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Matched samples of German and American high school students and mental hospital staffs completed a 60-item questionnaire on conceptions of mental illness. The conceptions of these groups showed greater differences between countries than between popular and professional views within each country. Furthermore, comparing these conceptions with a previous report on mental patients' conceptions in Germany and America, the conceptions of mental patients were found to parallel those of the public and hospital staff in their respective countries. These conceptions appeared to influence patients' coping behavior within the hospital. These results supported the application of labeling theory to mental disorders. But the results did not support the view that specific stereotypes of the mentally ill structure specific symptomatology. Stereotypes were not prominent in the responses of patients, staff or students in either country, and differences in the public's and professionals' responses to stereotypes within each country were greater than differences between the two countries.

INTRODUCTION

Scheff (1966) proposes that popular stereotypes of mental disorders are primary determinants of symptomatology. According to this theory, both before and after public labeling, the popular conceptions of mental illness, which have been learned and culturally reinforced since childhood, govern the expectations of the rule-breaker and of those around him and force his behavior into ever-increasing conformity with popular conceptions. The ultimate products of this process are the stereotyped roles of insanity.

Scheff's theory thus proposes that a culture's conceptions of mental disorders largely determine the process of defining someone as mentally ill. This definitional process acts as a self-fulfilling prophecy, i.e., due to inadvertent reinforcement from the social environment, the deviant's symptomatology ultimately comes to resemble the popular stereotypes. In this process, the stereotypes function as a "guideline for action," both for the deviant and for the laymen and professionals who react to him. Critical to any test of this theory, then, is information on the conceptions of mental disorders held by mental health professionals and laymen.

It is an established fact that cultures vary in the way they view mental disorders (Benedict, 1934; Linton, 1956; Wallace, 1972). Relating Scheff's theory to this cross-cultural variation, one would predict that two different cultures with different conceptions of mental disorders would: (1) show differences in professional conceptions of mental disorders, which correspond to the differences in the two cultures' popular conceptions; (2) show differences in how patients view mental disorders which correspond to the differences in popular conceptions; (3) show actual symptom formation which corresponds to the differences in popular conceptions. This paper attempts to validate the first two propositions in a

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controlled comparison of Germany and America. The third proposition will be discussed with reference to previously published data on mental patients (Townsend, 1975; Rosenhan, 1973; Rokeach, 1964; Goffman, 1961).

Germany and America were chosen for this comparison because previous research had suggested that professional conceptions of mental disorders did parallel popular conceptions in those two countries (Nunnally, 1961; Jaeckel and Wieser, 1970).

PURPOSE

The purpose of this investigation was to test the following hypotheses:

Hypothesis 1. Intercultural differences in popular and professional conceptions of mental disorders exceed intracultural differences. That is, professionals and laymen in each country should resemble each other more in their conceptions of mental disorders than they resemble their status-equivalents in the other country.

Hypothesis 2. Regarding intercultural differences, the German and American professional groups should, as a result of professional socialization, resemble each other more than the German and American laymen resemble each other. Thus, based upon degree of expected similarities in conceptions of mental illness for the present samples, the following ranking of pairs is predicted:

(1) Intracultural differences (students versus professionals) approximately the same in either country (smallest differences);
(2) German staff and American staff (greater differences);
(3) German students and American students (greatest differences).

Hypothesis 3. The conceptions of mental disorders held by German and American mental patients resemble the conceptions of their own professionals and laymen more than they resemble each other. In combination, then, Hypotheses 1, 2 and 3 posit that intercultural variation exceeds intracultural variation in the conceptions of mental disorders held by professionals, laymen and mental patients in Germany and America.

METHODS

Samples

Students. To measure popular conceptions of mental illness in Germany and America, samples of high school students were chosen in the areas of Seattle, Washington (N = 728) and Frankfurt am Main (N = 552). These samples were chosen so as to be representative of academic achievement levels and family income for high school students in those urban areas. In addition, the German sample was stratified according to the national percentages attending each type of high school in Germany: *Gymnasium* (college-prep); *Berufschule* (professional-occupational); *Realschule* (practical school). To some extent, this stratification correlates with socioeconomic class—the lower classes tending to choose practical and occupational schools, the upper classes, college-preparatory schools. Because it is difficult in Germany to learn a respondent's approximate income, socioeconomic classification is generally accomplished according to occupation. Consequently, the two samples were matched, as closely as possible, on the basis of the father's (or principal breadwinner's) occupation. Table A (Appendix) indicates that the two student samples do generally approximate each other in terms of reported occupation for the principal income-earner. The reported family incomes of the American students are also consistent with their reported occuptions: eighteen percent of the American group had fathers (or mothers) who were officials or licensed professionals and twenty-two percent of the sample had incomes of over $17,000. Participation was made voluntary in each case, but returns were close to one hundred percent once a school principal had decided to cooperate. More complete demographic characteristics of the two student samples are given in Table A (see Appendix).[1]

[1] The American student sample does lack ethnic and religious diversity (see Table A) and in this sense it is not representative of American populations. It was reasoned, however, that inclusion of ethnic and religious minorities could introduce the possibility of subcultural traits acting as confounding variables. Moreover, insofar as the majority of the American students were white and Christian, they were representative of what has traditionally been described as the dominant ethos of the United

Mental hospital staff. To represent German professional conceptions of mental illness, samples of mental hospital staff were chosen in Frankfurt, Koeppern, Duesseldorf, Hannover and Bremen. With the exception of the deep south, this selection offers a fairly complete regional representation of Germany. Since all prospective psychiatrists and neurologists must complete a residency term in a university clinic, and older, more traditional psychiatrists are often found in state mental hospitals, our sample of both university clinics and state hospitals offers a good representation of the "old guard" and the new German psychiatry. Cooperation was more easily obtained from the physicians although the nursing staff did participate to some extent. A total of one hundred two questionnaires was obtained. Approximately seventy-five percent of the staff members who were contacted returned completed questionnaires.

The American staff sample was chosen so as to match, as closely as possible, the German sample in terms of training, experience and work setting. To obtain some geographic diversity, samples of hospital staff were obtained in southern California and the upstate New York Area. More complete descriptions of the two staff samples are given in Tables B and C (Appendix). Examination of these tables reveals that, compared to the German sample, the American sample is slightly biased in favor of older physicians working in state hospitals. Regarding the present hypothesis, however, this is probably not a serious bias.[2] Participation was voluntary in both countries and returns were approximately seventy-five percent in Germany and eighty percent in America. A total of seventy-nine questionnaires was obtained in America.

INSTRUMENTS

Nunnally's 60-item Information Questionnaire (1961:259–64) was utilized to assess conceptions of mental illness in Germany and America. This instrument was chosen in order to facilitate comparisons with Nunnally's data and because the questionnaire items had been derived from a broad spectrum of popular and professional conceptions and then subjected to a considerable array of validation procedures.[3] The questionnaire consisted of 60 statements about mental health or illness followed by a seven-step "Disagree-Agree" scale. Thus a mean of "1.0" would indicate a group's total disagreement with an item, while a mean of "7.0" would indicate total agreement.[4] The 60-item questionnaire was translated with the aid of German colleagues who were experienced in this type of research

States. It was for these reasons that school districts containing large concentrations of ethnic and religious minorities were not included in the sample.

[2] This is probably not a serious bias because the German university hospital which provided most of the physicians (Frankfurt/Main) functioned as a municipal clinic and therefore more closely resembled non-university facilities in the American sample than it would resemble the prototype university clinic. The age bias, though slight, could be important. However, as represented by older physicians, traditional German psychiatry is almost exclusively biological. Consequently, any bias resulting from the greater number of young physicians in the German sample should only serve to *decrease* the intercultural differences which were found. Similarly, it is also quite probable that the "volunteer bias" tended to select for more "avant-garde" or progressive physicians in both countries. More conservative physicians tend to be skeptical of such research and, in the experience of this writer, are less likely to cooperate. In any event, there is no reason to suppose that volunteer bias would operate differently among mental health professionals in the two countries.

[3] Over three thousand statements concerning mental illness were gathered from 22 public-information pamphlets, from professional publications and from over two hundred personal interviews with the general public. The statements were then made randomly positive or negative and were pre-tested for clarity and for bias in phrasing. A final pre-test on 350 persons was conducted to further refine the instrument.

[4] Nunnally interpreted the close parallel between public and experts as a lack of *misinformation*. In constructing the questionnaire, however, Nunnally had eliminated all statements which elicited almost unanimous agreement or disagreement. This had the effect of eliminating most statements which were unequivocally true or untrue, and rendering the remaining statements somewhat ambiguous. Because of this ambiguity, most of the questions cannot be answered definitely on the basis of factual information. This questionnaire therefore tests information less than it tests *opinion*: a person's responses reveal what he believes to be true, not whether he is "informed" or "misinformed." In this sense, the responses of both public and experts to this questionnaire are "popular conceptions," and as such are properly included in the study of the sociology of knowledge.

and who had some command of English. This group consisted of two sociologists, two psychiatrists and one psychologist. This translation was then submitted to a professional translator of German-English psychiatric literature. He acted as a consultant in editing the final version.[5]

In order to isolate conceptual dimensions which were common to both German and American groups, the results of the questionnaire were factor analyzed using the varimax rotation method and including all subjects in the study (N = 1461). Mean factor scores were computed for the two staff groups and the two student groups, and these mean factor scores were subjected to four one-way analyses of variance.

RESULTS: FACTOR 1

Three factors were derived whose Eigen values exceeded the traditional cutoff point (i.e., 1.0). The first factor had an Eigen value of 6.1 and explained fifty percent of the total variance. Factors 2 and 3 had Eigen values of 2.3 and 1.8, respectively, and they explained nineteen percent and fifteen percent of the total variance, respectively. Factor 1, then, was by far the strongest and most general factor. Factor 2 was more specific and will be discussed separately. Factor 3 was not directly pertinent to the present hypotheses and has been described elsewhere (Townsend, Forthcoming).

Factor 1: Environmental Forces and Personal Effort. This factor contained items which expressed broad, general conceptions of mental disorders. Items with high loadings on Factor 1 stress the importance of environmental factors in causing and curing mental disorders. Thus, providing a person with a vacation, change of scene or the proper guidance or financial support can help prevent or cure mental disorders. Because mental disorders are transitory and mutable, a person must "try" to get well; he can "learn good emotional habits," should avoid worry and should read "books on peace of mind." Table 1 depicts the items comprising Factor 1 and their loadings.

As stated above, it was predicted that intercultural differences (i.e., staff versus staff and students versus students) would exceed intracultural differences (i.e., staff versus students within each country). It was further predicted that, due to professional socialization, the two staff groups would resemble each other more than the two student groups. The results of the four one-way analyses of variance tend to confirm both these predictions. Table 2 depicts the results of these comparisons and shows that, relative to the American groups, the German groups tended to reject the items comprising this factor. The intercultural differences were highly significant and tended to exceed the intracultural differences.

It should be noted that the differences depicted in Table 2 are relative, not absolute. The range of the groups' means (simple

Table 1. Factor 1: Environmental Forces

Item		Loading
11.	Mental illness can usually be helped by a vacation or change of scene.	.55
32.	Helping the mentally ill person with his financial and social problems often improves his condition.	.49
55.	Books on "peace of mind" prevent many people from developing nervous breakdowns.	.46
45.	Good emotional habits can be taught to children in school as easily as spelling can.	.40

[5] During the translation of the questionnaire, it was discovered that many of the expected conceptual differences were encoded in terminological differences. For example, the term "mental illness" is probably best translated by *psychisch-krank*, but this term is definitely not a part of middle-class vernacular. The common German word *geisteskrank*, is more akin to our word "insane" with its concommitant legal connotations. German psychiatrists also generally interpret the word *geisteskrank* as implying schizophrenia (endogenous and virtually incurable in their view). This means that the word *geisteskrank* is commonly used where we would use "insane" or "psychotic," but it is also used where we might employ the more euphemistic "mentally ill" and the inadequacy of *geisteskrank* as a translation reflects the conceptual differences the statements were designed to study.

Table 2. Analyses of Variance of Factor 1 Scores for German and American Staff and Student Groups*

	Intracultural Comparisons	F	df	P<
1.	American staff ($\bar{X}$ = .56) and students ($\bar{X}$ = .51)	.45	1/805	---
2.	German staff ($\bar{X}$ = -.84) and students ($\bar{X}$ = -.60)	8.66	1/652	.005
	Intercultural Comparisons			
1.	American and German staffs	171.87	1/179	.0001
2.	American and German students	856.88	1/1278	.0001

*American staff (N = 79); American students (N = 728); German staff (N = 102); German students (N = 552). See Tables 1-3 and text for description of samples and instrument.

means, not factor scores) on the items was quite narrow (1.5 – 5.9). This narrow range probably results from the ambiguity of questionnaire items. In constructing the questionnaire, Nunnally had eliminated all statements which elicited almost unanimous agreement or disagreement. This had the effect of eliminating most statements which were unequivocal. The statements which remained were therefore somewhat ambiguous; i.e., they could not be answered definitely on the basis of factual information. Subjects may have reacted to this ambiguity by indicating mild disagreement with an item (marking 2 – 4). Although they did not know whether a statement was absolutely false, they did know it was too general to be *always* true. Despite the limited range of the means, differences between groups tended to be highly significant. Standard deviations tended to be small enough that a difference of only 0.40 between two groups' item means could be significant at the 0.001 level. Standard deviations ranged from 0.727 to 1.23.

DISCUSSION: FACTOR 1

The results of Factor 1 directly support Hypotheses 1 and 2. Mental health professionals in Germany and America resembled their lay compatriots in their conceptions of mental disorders more than they resembled each other. Americans, compared to Germans, tended to endorse the notions that mental disorders are environmentally induced and can be influenced by an individual's personal effort and will power. Other sources support these findings. Jaeckel and Wieser (1970) found that the German layman tends to differentiate two separate orders of illness. *Gemütskrankheit* (emotional disturbance; melancholia) is a psychological disturbance caused by an external event. It is thus reactive in nature, can be merely transitory and is subject to therapeutic influence. *Geisteskrankheit* (mental illness; insanity) is inherited and is therefore a result of a "physical disease process"; it runs a fixed course and is not subject to therapeutic influence. Jaeckel and Wieser (1970:42) point out that this popular dichotomous taxonomy directly parallels the dichotomous taxonomy of mainstream German psychiatry (Hoff and Arnold, 1961:62).

Similarly, Nunnally (1961) found that popular conceptions of mental illness in America closely paralleled professional conceptions. Compared to the dichotomous taxonomy of the Germans, American popular conceptions appear to be unstructured and inconsistent; the American public apparently does not harbor a coherent folk taxonomy (Nunnally, 1961:23). American professionals manifest similar uncertainty and lack of consensus. Even basic premises are hotly debated among mental health specialists in America (Buss and Buss, 1969; Robbins, 1966; Torrey, 1974). In spite of their lack of consensus, however, both American laymen and professionals tend to emphasize environmental factors and will power compared to their German counterparts.

Conceptions of German and American Patients

A study of the conceptions of German and American mental patients tended to support Hypothesis 3. These results have been reported elsewhere (Townsend, 1975) and will only be summarized here.

Townsend (1975) interviewed matched samples of mental patients in Germany (N = 112) and America (N = 110). The interviews consisted of 24 open-ended and six forced-choice questions, a Twenty Statements Test and five semantic differentials. Several questions in the interview explored patients' conceptions of mental illness and patients' perceptions of proper coping tactics within the hospital. Only these questions pertain to the present discussion. These questions were coded and tested with Chi-square.

Curability. Two questions tested curability: "Is mental illness curable or not?" and "What are the chances of curing someone who is mentally ill?" On both questions Germans considered mental illness less curable than did Americans ($P < .001$, $P < .01$, respectively).

Stereotypes of Insanity. Americans differed significantly from Germans ($P < .001$) in response to the question, "How can you recognize a mentally ill person?" Americans tended to cite stereotyped bizarre behaviors and appearance as "diagnostic" criteria. In contrast, the German patients more frequently favored "internal" criteria; i.e., they tended to cite disturbances of mental functions, cognition and judgment as characterizing mental illness.

Purposes of Confinement. Germans and Americans differed markedly in their response to the question, "Why are the mentally ill admitted to the hospital?" Although the number of responses in some categories was too small to permit statistical analysis, the percentages of responses for each group show striking differences. For example, forty-six percent of German patients mentioned "improvement of health" as the reason that patients were committed; only twenty-four percent of the American group gave this response. In contrast, only two percent of German patients stated "because they behaved in a 'crazy' [in the stereotyped sense] manner," while over twelve percent of the Americans cited this as the reason for commitment.

Patients' Perceptions of Coping Tactics. The patient groups' conceptions of mental illness were reflected in their perceptions of appropriate coping tactics within the hospital. For the German patients (79%), only the doctor could decide when a patient should be released; there was nothing a patient could do to alter this medical decision. Only forty-nine percent of the American patients offered this opinion. In contrast, the Americans emphasized behavior and voluntary acts as crucial to the patient's discharge. In this regard, they stressed compliance with hospital regulations and staff directives (47%). Only five percent of the German patient sample offered this opinion.

The results of the patient study are thus consistent with the results of Factor 1. German patients generally agree with their staff and public that mental illness is a virtually incurable, biological malady. In contrast, patients, staff and public in America agree (relative to Germans) that mental illness resides in environmentally induced behavioral deviance rather than in an innate, organic condition. In the American view, this deviance can be eliminated with the right combination of motivation, skill and will power on the part of the patient and physician. In combination, then, the results of Factor 1 and the patient study tended to support Hypotheses 1–3. Insofar as these results support Hypotheses 1–3, they are, as explained in the Introduction, also consistent with an application of labeling theory to mental disorders.

RESULTS: FACTOR 2

Factor 2: Negative Sterotypes. This factor contained much more specific conceptions of mental disorders than Factor 1. Items comprising this factor portrayed the mentally ill as recognizably different from "normal people." The mentally ill do not pay attention to personal appearance, they have glassy eyes, are incurable and laugh more than normal people. Table 3 presents the items comprising Factor 2 and their loadings. The mean factor scores for the American and German student groups were −.025 and .162, respectively ($F = 15.10$, $df = 1/1278$, $P < .0001$). The American and German staff groups tended to reject this factor more strongly with mean factor scores of −.457 and −.345, respectively. Alhtough there was no significant difference

Table 3. Factor 2: Negative Stereotypes of the Mentally Ill

Item		Loading
14.	The insane laugh more than normal people.	.51
37.	You can tell a person who is mentally ill from his appearance.	.50
46.	The eyes of the insane are glassy.	.46
22.	There is not much that can be done for a person who develops a mental disorder.	.46

between the two staff groups, they did differ significantly from their student groups in both countries ($P < .0001$). Table 4 presents the results of the four paired comparisons and indicates that for this factor intracultural differences exceeded intercultural differences.

DISCUSSION: FACTOR 2

Factor 2 (Negative Stereotypes) distinguished between staff and public groups and between the student groups but it did not significantly differentiate the two staff groups, who tended to reject this factor more strongly than their publics. Factor 2, then, did not support Hypothesis 1. This finding is probably partially explicable in terms of professional socialization. Hypothesis 2 posits that the common professional experiences of the two staff groups will tend to diminish differences between them. By the same token, this same shared experience could cause the staff groups to differ from the student groups in their responses to certain items. This seems to explain the group's responses to Factor 2. Factor 2 contains extremely negative, stereotyped statements about the mentally ill: the insane laugh more than normal people, have glassy eyes, speak unintelligibly and pay little attention to personal appearance. These are images which many mental health specialists decry as derogatory sterotypes held by an ignorant public. The staff in a mental hospital know as a fact of experience that most mental patients do not laugh more than non-patients (indeed, except for manic patients, they may laugh less); the staff also know that most patients do not have "glassy eyes," nor do they speak in unintelligible words. It is thus probably due to their actual experience (as well as to their desire to appear enlightened) that the hospital staff groups in both countries tended to reject this factor as compared to the student groups. Thus, although these findings do not support Hypothesis 1, they do tend to support Hypothesis 2: professional socialization tends to diminish intercultural differences.

Patient Stereotypes of Insanity

As reported in the discussion of Factor 1, patients' interview responses tended to sup-

Table 4. Analyses of Variance of Factor 2 Scores for German and American Staff and Student Groups*

	Intracultural Comparisons	F	df	P<
1.	American staff ($\bar{X}$ = -.46) and students ($\bar{X}$ = -.025)	18.83	1/805	.0001
2.	German staff ($\bar{X}$ = -.35) and students ($\bar{X}$ = .16)	31.16	1/652	.0001
	Intercultural Comparisons			
1.	American and German staffs	0.92	1/179	---
2.	American and German students	15.10	1/1278	.0005

*American staff (N = 79); American students (N = 728); German staff (N = 102); German students (N = 552). See Tables 1-3 and text for description of samples and instrument.

port Hypotheses 1–3. However, the patients' statements concerning negative stereotypes of the mentally ill showed the same pattern as the staff and student groups' responses to Factor 2 (Negative Stereotypes). For example, in response to the question, "How can you recognize a mentally ill person?", only a minority of patients in either group mentioned stereotyped images of insanity. Similarly, in answer to the question, "Why are the mentally ill admitted to the hospital?", less than a third of the patients cited negative stereotypes of insanity. Moreover, a majority of patients in both groups denied that they themselves were mentally ill, and a majority failed to identify with patients who they thought were mentally ill and who therefore belonged in the hospital. In short, a majority of patients declined to use stereotyped images of insanity, and a majority of patients denied they were mentally ill and failed to identify with patients they thought were mentally ill (Townsend, 1975).

The patients' relative rejection of the stereotypes of insanity and their disinclination to use these images in descriptions of themselves and others are thus consistent with the staff and student groups' responses to Factor 2. *All* of the groups tended to reject such imagery compared to their relative endorsement of the more general conceptions embodied in Factor 1. The results of Factor 1 and the patient data, then, are consistent with a general application of labeling theory to mental disorders: general cultural conceptions appear to influence patient behavior (at least, the conceptions appear to influence patient conceptions and perceptions of proper behavior). In contrast, the results of Factor 2 and the patient data do *not* support Scheff's application (1966) of labeling theory to mental disorders: stereotypes of insanity (of the type discussed by Scheff) did not appear frequently in patients' conceptions or in their perceptions of proper coping behavior. To interpret this finding, it is useful to recapitulate the logic of this investigation and to examine some further evidence which bears on Scheff's theory and the present results.

Conceptions versus Stereotypes

In the Introduction it was proposed that, according to Scheff's theory, cultures with different conceptions of mental disorders would: (1) show differences in professional conceptions of mental disorders which correspond to the differences in popular conceptions; (2) show differences in patients' conceptions which correspond to the differences in popular conceptions; (3) show actual symptom formation which corresponds to the differences in popular conceptions. Propositions (1) and (2) were generally supported by Factor 1 but not by Factor 2. Proposition (3) was not directly investigated in this study; i.e., no study was made of symptoms per se. Interviews with patients did, however, elicit statements about mental illness and about "symptomatic" behavior. In making these statements, most patients did *not* use stereotyped imagery. Rather, as with the other interview questions, the patients tended to use the conceptions embodied in Factor 1.

These results suggest that a clear distinction should be made between popular *stereotypes* and popular *conceptions*. The type of stereotypes discussed by Scheff are those extreme images of insanity popularly depicted in the mass media. These are essentially the same images embodied in Factor 2 and they were not strongly endorsed by any of the groups. It is thus quite possible that the stereotypes do not structure symptomatology in the manner Scheff proposed. On the other hand, cultural *conceptions* of mental disorders, as embodied in Factor 1, did appear in patient conceptions and in their descriptions of proper coping tactics. This suggests that cultural conceptions, or beliefs about mental illness, do influence patient perceptions and coping tactics. It is therefore quite possible that these conceptions affect overt symptomatology as well.

The results cited above do not support the theory that *stereotypes* effectively mold the symptoms of mental patients. In addition to these results, several lines of evidence suggest that the stereotypes of insanity do not play such a role. First, Scheff's theory suggests that those with the power to label have internalized stereotyped images of insanity which they then inadvertently reinforce in the behavior of patients. There is no direct evidence that this is true of mental hospital staff. The most common stereotype, that of wild, uncontrollable, dangerous behavior, is certainly discouraged and often directly punished by the staff. It is true that bizarre

behaviors are frequently reinforced inadvertently by the staff (usually because that is the only way the patient receives staff attention), but these behaviors are seldom those contained in popular stereotypes. More often than not, they are passive behaviors: sitting and staring, aimless walking, rocking (Rosenhan, 1973; Goffman, 1961). Furthermore, it is clear from the results of this study that staff and public conceptions differ significantly, so any serious attempt to validate labeling theories of mental disorders would have to make detailed comparisons between the conceptions of staff and public and the symptomatic behaviors *each* tends to reinforce.

A second line of evidence concerns the "closeness of fit" between stereotypes and symptomatology. According to the theory, the stereotypes act as a guideline ultimately producing roles which resemble the stereotypes. Paradoxically however, most symptoms do *not* resemble the stereotypes and Scheff himself admits this in one part of his analysis (1966:67–79).[6] Two of the most common stereotypes are: uncontrolled violence and delusional personalities (e.g., "I'm Napoleon." "I'm Jesus Christ."). Regarding violence, Scheff cites a statistical study which showed that the incidence of violent crime (or of any crime) is much lower among former mental patients than in the general population (1966:72). Regarding delusional personalities, among 25,000 patients in state mental hospitals in Michigan, Rokeach (1964) found only a handful of such people; only three patients without brain damage consistently believed they were Christ and there were no Napoleons, Caesars, Krushchevs or Eisenhowers. Thus, although stereotypes may influence symptoms, they do not seem to structure them effectively in the manner proposed.

The third line of evidence concerns the apparent lack of a coherent folk-model of mental illness in urban populations. No empirical study has been able to demonstrate such a model. To be sure, Germany appears to have a somewhat more systematic folk-taxonomy than America (Jaeckel and Wieser, 1970; Nunnally, 1961), but even in Germany the public's answers showed considerable variation and disagreement. Moreover, to warrant the term "model," folk conceptions of illness generally contain some notions of diagnostic criteria, etiology, prognoses and treatment arranged in some reasonably coherent framework (e.g., Wallace, 1972). No such "models" for mental disorders have been demonstrated in lay populations of industrialized nations. Compared to large nation-states, the societies where folk models have been described are consensual communities. In such communities, virtually everyone subscribes to the same supernatural beliefs and their theories of illness are generally an integral part of these beliefs. Such theories, then, have the force of powerful sanctions behind them and, because there is virtual consensus on these theories, those persons surrounding the deviant are probably more consistent in reinforcing or punishing various behaviors. It is probably due to this consensus and consistency that folk-conceptions appear to be more effective in structuring illness behavior in small primitive societies than in large nation-states.

The evidence marshalled above is thus consistent with the results of the present study: stereotypes (of the extreme type discussed by Scheff) do not effectively structure symptomatology in the manner proposed. The present findings suggest, however, that more general cultural conceptions may influence symptomatology in the manner proposed by labeling theory. These findings thus raise the question of what role, if any, stereotypes play in the societal reaction to deviance. In the following paragraphs, I would like to depart somewhat from my empirical findings and elaborate on

[6] Scheff fails to define both "stereotypes" and the behavior they allegedly shape, namely, the "symptoms" of mental illness. This is unfortunate because "stereotype" is an extremely nebulous term: no general statement can be made about the degree of distortion implied by the term "stereotype" (Gould and Kolb, 1964:694). Consequently, the reader never knows in what sense the stereotypes are "stereotypes"; i.e., how they deviate from reality. The problem of defining stereotypes is thus central to Scheff's theory. To define rigorously the "stereotypes of insanity," Scheff would have had to describe the degree of their correspondence to reality. In doing so, he would have automatically commented on the efficacy of the stereotypes as the shapers of the roles of mental illness. Scheff seems unaware of this problem as he alternates between presenting the stereotypes as distortions of reality (1966:67–79) and depicting them as shapers of that reality (1966:82–96).

Jaeckel and Wieser's analysis (1970) of the stereotypes of insanity. To be sure, the functions for stereotypes which will be proposed below do not derive directly from the results presented in this paper. Nevertheless, the proposed functions are entirely consistent with the present results and with other empirical studies to be discussed.

FUNCTIONS OF STEREOTYPES

One of the most consistent elements in society's reaction to mental illness is fear. This is reflected in empirical studies (Cumming and Cumming, 1956; Nunnally, 1961; Jaeckel and Wieser, 1970) and in the laws society has erected to protect itself. Jaeckel and Wieser argue that, although the public fears physical threats from the insane, the real threat is to the background understandings and expectations which make social intercourse possible. Unlike other deviants, the mentally ill appear to the lay public to have incomprehensible motives and to be impervious to threats of punishment and other sanctions. They thus appear both unpredictable and uncontrollable. It is this fantasy of uncontrollability which provokes such intense fear.

This scheme receives support from two sources. First, empirical studies have isolated "unpredictability" and "incomprehensibility" as consistent components in the images urban populations have of the mentally ill (Nunnally, 1961; Townsend, 1972; Jaeckel and Wieser, 1970). Secondly, it appears that in small communities, where people are more familiar with a deviant individual, they tend to find his behavior more comprehensible and predictable and are thus more likely to tolerate certain eccentricities (Foucault, 1965; Eaton and Weil, 1967). Scheff (1966), for example, found that rural courts were more careful about commitments and generally more tolerant of eccentric behavior than urban courts.

In line with the preceding argument, three functions for the stereotypes of insanity may be proposed. First, they may serve as a warning, a contrast conception, to those who would breach social expectations. Phrases like "That's crazy!", or "Better watch out. They'll be calling the men in white coats!", though often used facetiously, nevertheless protest against unexpected rule violations; they tell a person his behavior is not quite acceptable. Second, the stereotypes may function as diagnostic criteria for the layman. For the man in the street, stereotypes are his only guide for detecting insane behavior. As was previously pointed out, however, the stereotypes are *not* accurate depictions of symptomatology. One would therefore expect the public's recognition threshold to be high; i.e., initial symptoms which did not match the stereotypes would not be perceived as mental illness. This appears to be the case. Yarrow et al. (1955) found that husbands had to breach their wives' expectations repeatedly before the wives would finally define the behavior as mental illness. Similarly, Star (1955) found that when presented with classic textbook examples of various mental disorders, the public designated only the most extreme case, paranoid schizophrenia, as mental illness. Evidently, these studies tend to support the notion that the public's recognition threshold remains high partially because their images of mental illness are so distorted. Once the recognition threshold is crossed, however, the stereotypes serve to justify the extreme, dichotomous treatment of the mentally ill. This is a third possible function of the stereotypes. The distorted images of the stereotypes then justify total isolation of the deviant and the suspension of his rights and duties. This occurs even though a person's behavior may have indicated that less extreme measures were in order (because, in fact, his behavior did not conform to the stereotypes). Even when this is the case—as it usually is—the fear is always present that his behavior *might* conform to the stereotypes in the future.

Although the functions proposed above for the stereotypes of insanity are not directly supported by the results of this study, they are nevertheless consistent with them and with other empirical works. Consistent with the present findings, then, is the notion that the stereotypes are not frequently realized in symptomatology, but they may well serve as warnings, as folk diagnostic criteria and as rationalizations for the extreme treatment of the mentally ill.

CONCLUSION

The results of the present study suggest that broad, cultural conceptions of mental disorders differ in Germany and America and the conceptions of patients, mental hospital staff and laymen within these countries parallel

each other quite closely. Thus, intercultural differences exceeded intracultural differences. In contrast, cultural stereotypes of insanity were not consistently endorsed by patients, staff or students in either country. Stereotypes were also lacking in patients' self-conceptions and their perceptions of proper behavior within the hospital. These findings thus provide some support for a general application of labeling theory to mental disorders: cultural conceptions do appear to influence patients' perceptions. The present results do not, however, support Scheff's specific application (1966) of labeling theory; i.e., that the stereotypes of insanity act as a guideline for symptom formation. The present findings thus establish a definite link between cultural conceptions of mental illness and patient views of appropriate coping tactics. It is not yet clear, however, how cultural conceptions may affect a wider range of patient behavior. The exact relationships between patient behavior, different conceptions of mental illness and the formal requirements of institutional life have yet to be delineated. This seems a promising topic for future study.

Table A. Demographic Characteristics of German Student Sample (N = 552) and American Student Sample (N = 728)

Characteristic	% of German Students	% of American Students
Sex		
Male	53	51
Female	47	49
Age		
14-15	11.1	20
16-17	59.6	52
18-19	25.4	22
20-22	3.8	6
Ethnic Group		
Chicano	---	0.7
Black	---	0.6
White	98.6	93.6
Indian	---	1.9
Foreign Student	1.3	0.2
Asian American	---	1.0
Other	---	1.9
Approximate Family Income		
Under $2,000	Information not available for German Students	2.6
$2.200-5,600		12.7
$5,600-7,000		5.9
$7,000-11,800		25.8
$11,800-17,000		30.4
$17,000-20,000		10.1
Over $20,000		12.5
Marital Status		
Married		3.0
Single		97.0
Religion		
Protestant	56.8	30.4
Catholic	35.3	25.9
Jewish	0.2	0.6
Agnostic	---	5.6
No Preference	1.8	14.9
Other	5.9	22.5
Occupation (of father or of principal earner)		
Laborer (manual)	25.4	35.2
White collar employee	31.2	26.5
Civic official (Beamte), or licensed professional	11.4	18.1
Entrepreneur	20.7	5.4
Farmer	0.2	1.4
Retired	0.7	1.4
Unemployed	---	4.4
Unknown	10.1	7.6

Table B. Demographic Characteristics of German Hospital Staff Sample (N = 102)

Characteristic	Percent
Sex	
Male	71.8
Female	28.2
Age	
15-20	1.9
21-30	33.0
31-40	40.8
41-50	18.4
51-60	5.8
Marital Status	
Single	35.9
Married	56.3
Divorced	6.8
Widowed	1.0
Years of Education	
6-8 (Volkschule)	1.9
9-11 (Mittelschule)	2.9
12-15 (Oberschule, Fachschule or Berufschule)	20.3
15-20 (Hochschule)	74.8
Present Position	
Intern	7.8
Resident	31.1
Psychiatrist	27.2
Medical Admin.	4.9
Social Worker	2.9
Psychologist	1.9
Therapist	1.9
Attendant (Male)	8.7
Nurse	13.6
Length of Service in Mental Hospitals	
0-3 months	15.5
4-6 months	5.8

Table B. (continued)

Characteristic	Percent
7 mos.-2 years	23.3
3-5 years	22.3
6-10 years	15.5
11-20 years	15.5
Over 20 years	1.9
Hospital	
Bremen Municipal Psychiatric Clinic	21.1
Hanover University Psychiatric Clinic	16.7
Düsseldorf State Mental Hospital	24.4
Frankfurt University Psychiatric Clinic	36.7
Koeppern State Mental Hospital	1.1

Table C. Demographic Characteristics of American Hospital Staff Sample (N = 79)

Characteristic	Percent
Sex	
Male	66.2
Female	33.7
Age	
15-20	1.2
21-25	6.3
26-30	17.5
31-35	12.5
36-40	7.5
41-50	26.2
51-60	20.0
Over 60	8.7
Years of Education	
12-13	17.5
14-15	12.5
16-17	5.0
18-19	5.0

Table C. (continued)

Characteristic	Percent
20-21	45.0
Over 21	15.0
Marital Status	
Single	8.7
Married	75.0
Divorced	11.0
Separated	1.2
Widowed	2.5
Religion	
Protestant	45.0
Catholic	17.5
Jewish	6.3
Latter Day Saints	13.7
No Preference	13.7
Other	2.5
Unknown	1.2
Length of Service in Mental Hospital or Psychiatric Ward	
0-3 months	6.3
4-6 months	5.0
7 mos.-2 years	22.5
3-5 years	13.7
6-10 years	18.8
11-20 years	23.7
Over 20 years	10.0
Present Position	
Psychiatric Technician	25.0
Nurse	3.7
Psychiatrist	37.5
Resident	22.5
Intern	3.7
Medical Admin.	1.3
Social Worker	2.5
Psychologist	1.2
Student Aide	2.5
Type of Hospital	
State Mental Hospital	68.7
University	21.2
General (Psychiatric Ward)	1.3
Veterans Administration	6.2
Combinations of above	2.5

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SOCIOECONOMIC STATUS AND CRIMINAL SENTENCING: AN EMPIRICAL ASSESSMENT OF A CONFLICT PROPOSITION*

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The "conflict perspective" has generated considerable critical interest in the mechanisms of criminal sanctioning. Among the hypotheses generated by conflict criminology is the proposition that "when sanctions are imposed, the most severe sanctions will be imposed on persons in the lowest social class" (Chambliss and Seidman, 1971:475). This paper affords an empirical test of that proposition, which has been widely accepted as true despite the absence of substantial supporting evidence. Prison sentences received by 10,488 inmates for a total of seventeen specific offenses, within three southeastern states are examined. Product-moment correlations between a defendant's SES and the sentence received provide no support for the conflict proposition, and this conclusion is further strengthened when controls are introduced for "prior record" and demographic characteristics of the defendant. Multiple correlation and beta coefficients are also examined with similar conclusions. Implications for the explanation of criminal sanctioning institutions by conflict mechanisms are suggested.

Among criminologists, the promise of a "conflict" perspective for the study of crime and social control has been widely proclaimed in recent years (Chambliss, 1969; Chambliss and Seidman, 1971; Horton, 1966; Quinney, 1970; 1972; 1973; 1975; Turk, 1966; 1969). Indeed, enthusiasm for this perspective has prompted the identification of a "new criminology" (Taylor et al., 1973), as well as the judgment that criminology is now "both a reflection of and a force behind" what Chambliss (1973:1) contends is a "paradigm revolution" occurring in western sociology.

While the concept of "paradigm revolution" (Kuhn, 1970) may be ill applied to what is happening in sociology,[1] it *is* clear that criminology is no longer insulated from the dominant currents of sociological theory (Mills, 1943; Taylor et al., 1973), and is actively engaged in the renewed critique of functionalist conceptions of social order (Gouldner, 1970; Fredrichs, 1970; Horton, 1966; Chambliss, 1973; Taylor et al., 1973; Thio, 1973). Whether or not this critique warrants description as "... a gigantic struggle between the previously dominant functional paradigm (Durkheim, 1949; Parsons, 1937; 1949; 1951; Merton, 1957) and the

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[1] To characterize the renaissance of interest in conflict models of society as a "paradigm revolution" may depart from the meaning of that concept as eventually clarified by Kuhn (1970:174–210). While the scope and impact of "functional" and "conflict" models of social order may warrant their designation as "exemplars" within the discipline of sociology, it may overstate the case for functionalism, and understate the case for conflict and other perspectives, to speak of a "revolution" among them. In particular, it is doubtful that functional sociology ever so dominated the field that sociology was engaged in the simple puzzle-solving of "normal science" (Kuhn, 1970:23–41). Indeed, the virulence of competing exemplars (social action, symbolic interaction, conflict, exchange, etc.) within sociology probably justifies its designation as a "pre-paradigm" field of study (Kuhn, 1970:10–22). Further, if we allow Chambliss' (1973:2) characterization of Durkheim as "the well-spring for much contemporary functional analysis," and we acknowledge that Marx's writings antedate Durkheim's by approximately fifty years, then it is even more difficult to conceive of recent interest in conflict as constituting a "revolution."

emergent conflict paradigm" (Chambliss, 1973:1), it is certain that conflict criminology begins with a set of domain assumptions that are radically different from those of functional criminology (Horton, 1966; Chambliss, 1973). Those assumptions, which consider the bases of legal order as centrally problematic, have shifted criminological attention away from the criminal actor and his behavior toward "variations in legislative, judicial and enforcement behavior" which, according to Turk (1966:340), "have seldom been treated other than as sources of error in estimating rates of deviance."[2]

Though current versions of conflict criminology in the United States range from the formal propositions of Turk's (1966; 1969) "theory of criminalization" to Quinney's (1973; 1974) "critical philosophy," the central issue – power, and its potentially oppressive use in matters of law and criminal justice – receives consistent attention from most conflict criminologists. With specific allowances made for such contingencies as the size and degree of threat posed by various groups and interests, this perspective has generally argued that the less powerful a group, the more likely will its behaviors be designated as crime and its members designated as criminals. However persuasive the logic or appealing the ideology of such an argument, its validity and scientific utility require that it be formulated as testable research hypotheses which can direct the accumulation of relevant empirical evidence. It is on the basis of such evidence that the acceptability of a "conflict perspective" as an alternative world view in criminology specifically, and sociology more generally, will ultimately be determined; and it is toward the accumulation of such evidence that the research reported herein is directed.

Several conflict theorists (Chambliss and Seidman, 1971; Quinney, 1970; Turk, 1966; 1969) have generated propositions concerning the *formulation* of criminal definitions (laws) and Quinney's (1970:16) may be considered representative: "Criminal definitions describe behaviors that conflict with the interests of the segments of society that have the power to shape public policy." However, analyses of the legislative process which support, or even address such a proposition have not been extensive.[3]

Concern with the *application* of existing criminal sanctions has led to propositions such as Turk's (1966:349): "In general, the greater the power difference in favor of the authorities, the greater the probability of criminalization of the opposition." Though empirical evidence addressing such a proposition remains inconclusive at best (Bordua, 1969; Black, 1971; Hagan, 1974) social scientists have, as noted by Thornberry (1973), readily asserted the truth of its implications.[4] An example of such an assertion, made without specific reference to research evidence, is provided by Chambliss (1969:86) who argues that:

> The lower-class person is (1) more likely to be scrutinized and therefore to be observed in any violation of the law, (2) more likely to be arrested if discovered under suspicious circumstances, (3) more likely to spend the time between arrest and trial in jail, (4) more likely to come to trial, (5)

[2] In fact, primary credit for redirecting criminological attention to the processes and institutions of social control must rest with the "labelling" perspective, which has been described by Scheff (1974) as serving a "sensitizing" role in the study of deviance. Labeling, of course, is rooted in the organizing premises of symbolic interaction (Blumer, 1969) which posit the centrality of all processes whereby meaning is assigned to objects – including behaviors and people who may be designated as "criminal" by the legal process.

[3] See, for example: William J. Chambliss (1964), "A Sociological Analysis of the Law of Vagrancy," *Social Problems* 12 (Summer): 66–77; James M. Graham (1972), "Amphetamine Politics on Capitol Hill," *Transaction* (January): 14–72; Jerome Hall (1952), Theft, Law and Society, Indianapolis: Bobbs-Merrill Co.; Gabriel Kolko (1965), Railroads and Regulations, Princeton: Princeton University Press; Alfred R. Lindesmith (1959), "Federal Law and Drug Addiction," *Social Problems* 7 (Summer): 48–57; Pamela Roby (1969), "Politics and Criminal Law: Revision of the New York State Penal Law on Prostitution," *Social Problems* 17 (Summer): 83–109; Andrew Sinclair (1964), Era of Excess: A Social History of the Prohibition Movement, New York: Harper and Row.

[4] Thornberry cites instances from Lemert (1951) and Clinard (1963), as well as Sutherland and Cressey (1963), wherein the authors assert, without reference to data, that minority group members are substantially disfavored in the processes of criminal justice. A similar theme is developed by Terry (1967).

more likely to be found guilty, and (6) *if found guilty, more likely to receive harsh punishment than his middle or upper-class counterpart* [emphasis added].

The present study considers Chambliss' assertions to be empirical questions requiring careful research attention. Specifically, we have examined the relationship between a defendant's socioeconomic status and the severity of prison sentence received upon conviction. Thus, the present data speak to the final assertion by Chambliss noted above, as well as to the formal proposition developed by Chambliss and Seidman (1971:475) in their "theory of the legal process in complex societies" which holds that:

> When sanction are imposed, the most severe sanctions will be imposed on persons in the lowest social class.[5]

Such a proposition appears to have been prompted less by the availability of empirical evidence[6] than by the recognition that statutory provisions for sentencing accord extraordinary discretionary latitude to the judiciary.[7] Indeed, given the expanded use of indeterminate sentences and the emergence of a conflict perspective in criminology, it is surprising that more researchers have not generated evidence that would address the foregoing theoretical propositions as well as the pragmatic questions raised by Chambliss and Seidman (1971:462–3), who ask:

> What are the consequences of permitting basically middle-class judges, socialized into a profession in which most of its functions serve a middle-class clientele, working in a bureaucratic framework where efficiency and smooth functioning are seen as critically important . . . to have unfettered discretion in the sentencing of criminals who are mainly from the poor and minority groups?

PRIOR RESEARCH

The available evidence relative to criminal sentencing is poorly suited for the task of testing Chambliss and Seidman's proposition. As noted by Hagan (1974) in his comprehensive review of studies which relate sentencing outcome to extra-legal characteristics of defendants, there have been few attempts to ascertain the relevance of socioeconomic status for sentencing dispositions,[8] and the results of these studies must be regarded as inconclusive.

Hugo Bedau (1964; 1965) examined the "final disposition" of death sentences imposed in the state of New Jersey, for the period of 1907–1960 and in the state of Oregon, for the period of 1903–1964. Bedau's indicator of socioeconomic status (though he did not term it as such) was the defendant's occupation, which was classified according to the traditionally broad census categories, which range from "professional and technical workers" to "laborers" and "service workers." For New Jersey, Bedau (1964:27) observed that while only 9.7% of all males in the state (between 1940 and 1950) could be classified as "laborers," 29.7% of those sentenced to die in that state were "laborers." Similarly for Oregon, Bedau (1965) noted that 13.1% of the adult males were classified as "laborers," while 36.9% of those receiving the death sentence were similarly employed. Although

[5] It should be emphasized that Chambliss and Seidman (1971:473–5) actually specify a total of twenty-three "propositions" in their theory, a number of which actually serve as necessary assumptions for the development of subsequent propositions. Thus, the present research is limited to a single proposition within the larger set, but it is clearly addressing one of the two or three most important propositions in Chambliss and Seidman's theory.

[6] In fact, the evidence which Chambliss and Seidman (1971) cite as support for their proposition concerning sentencing appears limited to a discussion of Green's (1961) data pertaining to race, as well as Mary Owen Cameron's (1964) findings relative to the disposition of shoplifting cases, and Wolfgang, Kelly and Nolde's (1962) concerning racial disparities in commutations and executions of condemned criminals.

[7] Chambliss and Seidman (1971) develop this theme in Chapter 21 of their book, *Law, Order and Power*.

[8] Hagan (1974) lists a total of six studies that are presumably concerned with the relationship between SES and sentencing dispositions. However, one of these (Forslund, 1969) does not employ sentencing as the dependent variable, but is concerned instead, with the likelihood of conviction. A second study (Martin, 1934) reviewed by Hagan, is unavailable to the present authors; however, our review includes two studies with certain relevance for sentencing (Terry, 1967; Thornberry, 1973) that were not examined by Hagan.

Bedau (1964:27) considered such evidence to be "... further confirmation to the familiar charge that the death penalty is more likely to be applied to the ignorant, the poor, and the friendless," one must observe that Bedau's data reveal nothing about the characteristics of defendants in capital cases who *did not* receive the death sentence. Quite possibly, the disproportionate representation of "laborers" among those sentenced to die is matched by the representation of "laborers" among all persons brought to trial for capital cases, and on that account, among persons who do not receive the death penalty.

A somewhat more sophisticated analysis of sentences determined by California juries in 238 first-degree murder cases between 1958 and 1966 was reported by Judson[9] et al. (1969) with more definitive conclusions than those allowed by Bedau's data. Again, the defendant's socioeconomic status was measured in occupational terms and for most analyses was dichotomized into "blue-collar" and "white-collar" categories, although the researchers were also concerned with "job stability" and "employment status" at the time of the offense. Judson et al. (1969:1377–9) report that 42.1% of the "blue-collar" defendants (N = 157) as opposed to 4.8% of the "white-collar" defendants (N = 21) received the death sentence upon conviction for first-degree murder. Having supported such findings with product-moment and partial correlations, the authors conclude that "the simplest hypothesis to explain this powerful association is that juries are lenient toward white-collar defendants on the basis of their occupational status alone" (Judson et al., 1969:1379).

Stuart Nagel (1969) has examined the sentencing outcome for a sample of state cases involving grand larceny (N = 1,103) and aggravated assault (N = 846) for 1962, as well as the total population of assault (N = 196) and interstate larceny (N = 785) cases tried at the federal level in 1963. Nagel's indicator of "economic class" is defined in a "legalistic, rather than a sociological" manner, with the primary distinction involving "indigents" versus "non-indigents" for the state data and "assigned counsel" versus "retained counsel" for the federal data.[10] With regard to sentencing disposition, Nagel employed two dependent variables: the percentage of defendants receiving a prison sentence (as opposed to probation or a suspended sentence) and the percentage of those sentenced to prison who received a term exceeding one year. Nagel found similar patterns at both the state and federal levels. For assault, "indigents" or those with "assigned counsel" were more likely to receive a prison sentence and were more likely to have that sentence exceed one year. For larceny, the poorer defendants were more likely to receive a prison sentence, but were less likely to have that term exceed one year (Nagel, 1969:90–2). Though Nagel did not report on the significance of these differences, Hagan (1974:372) has shown that most were, in fact, statistically significant. Nagel controlled for the defendant's prior criminal record (none/some) among federal larceny cases and determined that "the positive disparity between being indigent and not being recommended for or being granted probation or a suspended sentence is still large even when the presence of prior record is held constant" (1969:91). However, Hagan's (1974:372) subsequent computations showed that the significant zero-order differences between indigents and non-indigents in sentences received at the federal level were rendered non-significant with controls introduced for prior record.

Two additional studies of the administration of juvenile justice were not considered in Hagan's (1974) review, but seem relevant for present considerations. Robert Terry (1967) examined all cases of delinquency that came to the attention of the police in a "heavily industrialized Midwestern city of slightly less than 100,000" for the years 1958 through 1962. From a total universe of 9,023 juvenile offenses which were recorded at the police level, only 246 were eventually referred to the juvenile court. Socioeconomic status was determined by father's occupation and was essentially unrelated (tau = −.09) to the severity of juvenile court dispositions (pro-

[9] Judson and his associates (1969) examined only those cases for which the same jury determined *both* the guilt of the defendant and the sentence received.

[10] Nagel (1969) reasoned that indigency and having a court appointed counsel amounted to much the same thing, and both were indicators of a relatively impoverished status.

bation versus institutionalization). Thornberry (1973), like Terry (1967), examined a variety of discretionary dispositions at the police and court levels, utilizing data collected for 3,475 boys who were born in 1945, resided in Philadelpha between their 10th and 17th birthdays and had at least one official delinquency recorded by the police.[11] The primary sentencing alternatives employed by juvenile court judges were probation or institutionalization. The measure of SES employed was the median income level of all persons living within the census tract containing a child's last known address. Thornberry (1973:96) reports that lower status juveniles are more likely (39.5%) to receive the more severe sentencing disposition of institutionalization than higher status juveniles (28.6%), and that this pattern is generally maintained when controls are simultaneously introduced for the seriousness of offense and the prior record of the juvenile.

The empirical evidence relative to socioeconomic status and sentencing which has been published to date is limited in several respects, and these limitations render the validity of the conflict proposition all the more problematic. First, it can be observed that the range of law violations considered in these research efforts is either extremely narrow or undefined. Three of the studies (Bedau, 1964; 1965; Judson et al., 1969) are concerned with the single, atypical offense of murder; another (Nagel, 1969) deals with the two crimes of assault and larceny; while two more (Terry, 1967; Thornberry, 1973) involve unspecified offenses, inasmuch as they include *all* delinquencies known to criminal justice authorities. Thus, what we "know" about this dimension of sentencing is limited to murder, assault and larceny and a host of undefined juvenile delinquencies.

A second limitation of available research is reflected in the fact that in every instance the dependent variable – sentencing outcome – has been operationally defined in dichotomous terms. The several studies concerned with murder (Bedau, 1964; 1965; Judson et al., 1969) considered only whether the defendant received the death penalty or not. Nagel (1969) considered two dichotomous possibilities: whether or not the defendant received a prison term and whether or not that term exceeded one year. The two studies dealing with juvenile justice (Terry, 1967; Thornberry, 1973) examined the likelihood of a child receiving probation or being sent to an institution. Clearly, such operational definitions overlook a great deal of variation in the sentencing disposition (even among those convicted of murder but not receiving the death penalty) by not considering the continuous nature of sentences received.

A third shortcoming of existing research evidence is established by the fact that relatively unsophisticated methodologies were employed by the researchers. Specifically, only Judson et al. (1969) and Terry (1967) report any measures of association with their data and, for the remainder, it is difficult to determine how strongly socioeconomic status and sentencing outcome are related. In addition, the four studies (Bedau, 1964; 1965; Nagel, 1969; Thornberry, 1973) that limit their analyses to percentage comparisons offer no indication of the significance of differences observed between "indigents" and "non-indigents" or "high" and "low" status defendants. For three of the studies (Bedau, 1964; 1965; Terry, 1967) the relationship between SES and sentencing was not examined with controls introduced for obviously important variables such as the prior record of the defendant. Nagel (1969) employed prior record as a control, but only for federal larceny cases – ignoring the assault and larceny cases at the state level and assault cases at the federal level. In short, the methodologies heretofore employed make it difficult to ascertain the strength, the significance and the non-spuriousness of any presumed relationships between SES and sentencing.

Finally, the conclusions to be drawn from existing research are not altogether clear. Only one of the studies (Terry, 1967) claims to have found *no support* for the proposition that lower status defendants will receive harsher dispositions, and his conclusion is based upon a relatively weak (tau = −.09) correlation coefficient. The remaining studies (Bedau, 1964; 1965; Judson et al., 1969; Nagel, 1969; Thornberry, 1973) all claim some support for the proposition that

[11] Thornberry (1973) has employed the cohort data collected by Wolfgang, Figlio and Sellin (1972) and reported in their research monograph, *Delinquency in a Birth Cohort.*

discrimination in sentencing occurs. Quite apart from the methodological shortcomings articulated above, the apparent unanimity of "support" for the conflict proposition can be seriously questioned. Hagan (1974:372–3) reanalyzed the data reported by Bedau (1964; 1965), Judson et al. (1969) and Nagel (1969), computing chi-squared values and levels of significance for reported percentage differences, as well as measures of association (tau-b) for the relationship between sentencing outcome and SES. While several of the chi-squared values proved statistically significant (Nagel, 1969; Judson et al., 1969), the more important finding appears to be that not one of the computed correlations was as high as tau-b = .05. A similar conclusion was reached by Wellford (1975) who analyzed Thornberry's (1973) data and found that correlations between SES and sentencing disposition did not exceed –.10.

RESEARCH METHODS

Though not without its own shortcomings, the present research has attempted to overcome some of the limitations of previous research which have been described above. The data were derived from admissions summaries provided by the adult correctional agencies for North Carolina, South Carolina and Florida. Included for study are all felon inmates received by the prison systems of those states during the following time periods: North Carolina, January 1, 1969 to April 30, 1973; South Carolina, January 1, 1969 to June 30, 1971; and Florida, June 1, 1969 to May 30, 1970. To reduce random error and chance fluctuations, only those crimes were examined for which a minimum of twenty offenders were received by any one state in the specified period of time. This procedure yielded a total of 10,488 inmates sentenced for seventeen specific criminal offenses. For all admissions, data were available concerning criminal offense, length of sentence received and for the computation of SES. However, additional data with regard to demographic and prior record variables were consistently available only for Florida inmates and these "control" factors were examined only for that state.

The dependent variable – sentence length – has been recorded in months. For those receiving an indeterminate sentence, the only certain sentence is the "minimum" number of months received and it was used in conjunction with the full term (in months) given with determinate sentences. Life sentences, as well as death sentences, though rarely encountered, were recorded as 480 months.[12] First-degree murder has been omitted because virtually all penalties received are life or death sentences, providing minimal variation for purposes of analysis. It must be recognized that a more complete assessment of the sentencing process would include those sentences received by probationers as well as by those remanded to prison. However, these data are unavailable at this time and the present research must be regarded as but a partial test of the Chambliss-Seidman proposition.

Each inmate's socioeconomic status was measured with techniques developed by Nam and Powers (1968) and the U.S. Bureau of the Census (1963), which compare the subject's income, education and occupation to national distributions of these status criteria.[13] Given the backgrounds of most persons who filter through the criminal justice process to the prison level, it makes little sense to speak of "high," "middle" and "low" status in traditional terms. In fact, 96.6% of the offenders in this study had scores assigned which were less than 70 on a 100-point status scale.[14] Thus, our analysis has not attempted

[12] Since first-degree murder was excluded from the study, this eliminated most of the "life" or "death" sentences. This total of forty years was chosen as a continuous variable substitute for the nominal categories of "life" or "death" sentences when they did occur, because at the time the data were collected, it was conventionally true that most state systems employed this length of sentence whenever a "life" sentence was commuted to a term of years.

[13] The status scores for an inmate's income and education levels were those published by the U. S. Bureau of Census (1963), and those employed for occupations were those published by Nam and Powers (1968), who helped to develop the Bureau of Census scores and utilized the same 1960 census data. The socioeconomic status score assigned to inmates within the present sample, represents a simple arithmetic mean of the three standardized scores for income, education and occupation and is identical to the method for determining socioeconomic status levels for the general population which was employed by the Bureau of the Census (1963) and by Nam and Powers (1965; 1968).

[14] It should be recognized that despite the robustness of the product-moment correlation, the skewed SES distribution is a potential limitation of

to designate status groups or levels, and the SES scores are treated as a continuous variable to be correlated with sentences received by inmates.

Additional variables have been employed for the purpose of elaborating the basic relationship between socioeconomic status and sentence length. Several of these, such as an offender's race and prior criminal record, have been central to many previous sentencing studies. Others, such as an offender's age and the rural/urban character[15] of the sentencing county, have been introduced with the presumption that they might be reasonably expected to influence the criminal sanctioning process.[16] Because the present analysis examines sentencing disparities *within* offense categories, another significant variable, seriousness of offense, is effectively controlled by the research design.

FINDINGS

Table 1 shows the product-moment correlation (Pearsonian r) between the socioeconomic status level of defendants and the sentences received for seventeen specific

the present study as well as any other research that uses an incarcerated population. Unfortunately, since this is the nature of the "real world," this is the kind of population the researcher is forced to use if sentence length is to be the dependent variable. Because of the distribution of SES scores, however, a question might be raised concerning the sufficiency of variation of the SES variable to permit a relationship to occur. One might respond to this problem in several ways. First, it could be argued that the issue is inappropriately raised since the total covariance that *could* exist between these two variables is permitted to operate because all possible values of a population are being used, and not a restricted portion of a larger continuum of values. Second, one could indicate that while the $\bar{X}$ scores on SES reflect the skewness of the curve, the standard deviations indicate that a sufficient degree of variation *is* present so that a relationship between SES and sentence length would not be suppressed due to insufficient variation in SES scores. The scores for the three states are: Florida: $\bar{X}$ = 32.89, S.D. = 16.36; South Carolina: $\bar{X}$ = 31.86, S.D. = 15.96; North Carolina: $\bar{X}$ = 36.59, S.D. = 17.77. The SES distribution differs slightly for separate crime categories; however, for each crime there would appear to be sufficient variation to warrant the computation of product-moment correlations. For example $\bar{X}$'s for SES ranged from a low of 23.99 with S.D. = 16.94 for second-degree murder in Florida to a high of 49.71 with S.D. = 18.81 for drug offenses in North Carolina. Third, if the variation in the SES measures unduly restricted the correlations, one might expect to find the highest correlations for those crime categories having the least variation. This pattern does not occur, however, since some of the crime categories having the least variation in SES produced some of the largest correlations and vice versa. A Spearman's rank-order correlation was computed between the coefficient of relative variation (CRV) for SES and the r between SES and sentence length across the Florida crime categories, which yielded an $r_s = -.01$. Thus, there would appear to be little relationship between the relative variation in SES and the magnitude of r between SES and sentence length. Fourth, one might suggest that a log transformation of the SES variable be made in an attempt to normalize the distribution. Although the extent of skewness was not as great as that which might call for such a transformation (Schuessler, 1971:396, 417), a log transformation was made for the Florida data. Only minimal change occurs in the transformed correlations and none of these correlations are statistically significant. The largest negative correlation among the transformed correlations is –.09 for receiving stolen property (as compared to –.08 in the original correlation). The greatest changes are for second-degree murder where the transformed r = .07 as compared to .14; aggravated assault with a transformed r = .06 as compared to –.05; and forcible rape (which produced the largest negative correlation in the Florida data) is reduced to –.08 as compared to the original r = –.12. None of the other transformed correlations produced a difference greater than .02 when compared with the original correlations. Finally, one might consider the transformation of the dependent variable (sentence length) as well as the independent variable (SES). This was also examined for the Florida data and none of the correlations with both variables transformed were statistically significant. Only three crime categories were changed to any noticeable extent from the original correlations. Second-degree murder changes from .14 to –.10, aggravated assault from –.05 to .07 and receiving stolen property from –.08 to –.15. The other transformed correlations varied only slightly from the original correlations, with no pattern presented concerning the direction of the signs. It would appear that a log transformation of either (or both) the independent and/or dependent variables fails to alter in any meaningful manner the findings presented in this paper.

[15] Degree of "urbanness" for counties was estimated as follows: all counties were ranked in terms of population density, total employed in non-agricultural labor and total dollar value of all sales conducted within the county. The average of the three ranks was designated as the county's index of urbanization. Thus, the counties with the lowest average rank were considered the most urban.

[16] Among those studies which have considered age to be relevant for understanding some aspect of the sentencing process are those reported by Bedau (1964; 1965), Chiricos, Jackson and Waldo (1972), Green (1961; 1964), Judson et al. (1969) and Nagel (1969) and Terry (1967). With regard to the degree of urbanization, Bullock (1961) found "short" sentences more common in more urbanized areas.

Table 1. Product-Moment Correlations between Socioeconomic Status and Sentence Length for Specific Crime Categories in Three States

	Florida			South Carolina			North Carolina		
	r	r^2	N	r	r^2	N	r	r^2	N
2nd Degree Murder	.14	.02	68	--	--	--	--	--	--
Vol. Manslaughter	-.01	.00	98	.05	.00	69	.03	.00	422
Invol. Manslaughter	--	--	--	--	--	--	-.09	.01	252
Forcible Rape	-.12	.03	77	.19	.04	30	.19[a]	.04	115
Statutory Rape	--	--	--	--	--	--	.04	.00	340
Aggravated Assault	-.05	.00	199	.10	.01	241	.12[b]	.02	477
Armed Robbery	-.04	.00	143	-.05	.00	205	.12[c]	.01	769
Unarmed Robbery	-.03	.00	294	.17	.03	107	-.01	.00	180
Burglary	.09[a]	.01	606	-.03	.00	119	.04	.00	1879
Larceny	.09	.01	235	.01	.00	440	-.06	.00	291
Recv. Stolen Prop.	-.08	.01	67	--	--	--	.09	.01	114
Auto Theft	.03	.00	111	--	--	--	.04	.00	153
Embezzlement	--	--	--	--	--	--	.08	.01	83
Forgery	-.02	.00	239	-.07	.01	149	.03	.00	666
Drug Offenses	-.05	.00	253	-.13	.02	149	.01	.00	686
Escape	.10	.01	96	--	--	--	--	--	--
Arson	--	--	--	--	--	--	.17	.03	66

a = $p < .05$; b = $p < .01$; c = $p < .001$.

crimes within the three states. Also shown is the proportion of variance (r^2) in one variable that is explained by the other, as well as the sample size (N) for each crime category. Levels of statistical significance for computed correlation coefficients are shown where relevant.

As noted above, the conflict perspective generally, and the proposition of Chambliss and Seidman (1971) specifically, lead us to expect negative correlations between SES and sentence length.[17] The data in Table 1, however, yield only fourteen out of thirty-seven correlation coefficients that are negative and none of these is statistically significant. Further, only two of these negative relationships is greater than −.10 and the strongest is only −.13 (for drug offenses in South Carolina), which means that for the hypothesized negative relationships SES explains no more than two percent of the variance in sentence length. It should be noted that no crime yields consistently negative correlations across all three states and there is no discernible sentencing pattern when broad types of crime such as "personal" and "property" are considered.

One may also observe that five of the

[17]The observation of harsher sentences for lower status offenders could be predicted by other perspectives besides conflict. For example, labeling might argue that stereotyping of lower-class people as "criminal types" could generate harsher penalties, and functionalism might argue that since the greatest disruption to necessary conditions of orderly social life was provided by lower status criminals, harsher penalties for them are functionally necessary.

positive correlations are stronger than the largest negative correlation, with four of these significant at $p < .05$ or lower. Three of the four statistically significant positive correlations are found in North Carolina and appear to be more a function of large N's than of theoretically meaningful correlations. Despite a pattern of stronger positive than negative correlations, the largest positive correlation is only .19 and SES can be said to account for no more than four percent of the variance in sentence length for any crime. It should be noted in passing that the three offenses yielding statistically significant positive correlations in North Carolina (forcible rape, aggravated assault and armed robbery) produced insignificant but negative correlations in Florida. In general, it would appear that the correlations presented in Table 1 are minimal and fluctuate in a random fashion. Such findings run directly counter to Chambliss and Seidman's conflict proposition, to expectations generally raised by conflict criminologists and to common assertions made in sociology and criminology textbooks.

In an effort to understand the lack of negative correlations and the moderate positive correlations that were obtained, the basic relationship between SES and criminal sentencing in Florida was re-examined within categories of several "prior record" variables (Table 2) and several "demographic" variables (Table 3). As noted above, these data were not consistently available in South Carolina and North Carolina and the subsequent analysis pertains only to Florida. Inasmuch as the zero-order relationship between SES and sentencing was more often negative in Florida than in South Carolina or North Carolina, any bias introduced by excluding these states would appear to favor the conflict hypothesis.

Three measures of a defendant's prior criminal record have been dichotomized for purposes of the present analysis, and these include whether one has ever: (a) been arrested; (b) been incarcerated in a juvenile institution; (c) been convicted of a felony. The data from Table 2 tend to confirm the earlier findings and provide little support for conflict expectations. Specifically, of the seventy-two correlations examined, thirty-four are negative and thirty-eight are positive. Four of the correlations are statistically significant but three of these are positive. Only eight of the correlations exceed .20 and five of these are positive. It should be noted, however, that for the crime of auto theft the correlation between SES and sentencing is statistically significant with an $r = -.36$ among those who had been in a juvenile institution. This represents the strongest association in the table. Overall, however, prior record does not seem to affect the SES/sentencing correlation, despite the fact that there are more negative correlations among those with no prior record (19) than are found among those with some prior record (15). Thus, we must conclude that whatever difference "prior record" makes for the relationship between SES and criminal sentencing, it does little to increase our confidence in the conflict hypothesis.

Controlling for demographic characteristics of the defendant does not appreciably alter the pattern of zero-order correlations and provides little support for the conflict hypothesis. As noted in Table 3, there are no statistically significant negative relationships between SES and sentence length within any of the demographic control categories. The three significant correlations are positive and involve the single offense of burglary. Overall, thirty-nine correlations are negative and thirty-seven are positive, but only eight correlations exceed .20. Of these, five are negative and three positive. For the crime "receiving stolen property" the correlation is -.30 for those under age 25, and while this is the strongest correlation in the table, it is not statistically significant.

Table 4 presents the step-wise multiple correlations between sentence length and seven independent variables for the thirteen crimes. In this phase of the analysis, we are particularly interested in how early in the process SES enters the equation for each crime and, further, how much of an increment that variable makes to the proportion of variance explained in the dependent variable – sentence length. Table 4 shows the R and R^2 values at each step in the process. Immediately below these values is listed the variable which was added at that step. For second-degree murderers, for example, race was the variable most highly correlated with the sentence length and it shows up in the zero-order column with an $R = .26$. In the first-order column urbanization had the strongest relationship with sentence length after the effects of race were partialled out, so it was added to race and produces an $R = .35$.

Table 2. Product-Moment Correlations between SES and Sentence Length within Florida Crime Categories Controlling for Defendant's Prior Criminal Record

		Number of Arrests				Ever in a Juvenile Institution				Any Felony Convictions			
		≤5		>5		No		Yes		No		Yes	
	Zero-Order	r	(N)	r	(N)	r	(N)	r	(N)	r	(N)	r	(N)
2nd Degree Murder	.14	.20	45	.22	21	.14	60	--	--	.15	50	--	--
Vol. Manslaughter	-.01	.01	70	-.07	27	.02	86	--	--	-.01	71	.01	26
Forcible Rape	-.12	-.10	53	-.06	23	-.19	62	--	--	-.03	56	--	--
Aggravated Assault	.05	.02	106	.09	93	.06	160	-.06	37	.04	119	.10	72
Armed Robbery	-.04	.00	82	-.09	61	-.01	105	-.05	36	.01	94	-.09	49
Unarmed Robbery	-.03	-.03	187	-.01	107	.02	202	-.17	87	.09	169	-.10	107
Burglary	.09[a]	.09	312	.07	287	.07	400	.13	186	.12[a]	324	.09	254
Larceny	.09	-.05	118	.21[a]	115	.10	177	-.05	56	-.01	125	.15	103
Recv. Stolen Prop.	-.08	-.22	30	.08	37	-.13	41	.05	20	-.27	37	.15	27
Auto Theft	.03	-.13	62	.12	49	.25[a]	73	-.36[a]	35	-.04	62	.12	47
Forgery	-.02	.06	90	-.06	147	-.02	193	-.00	37	-.07	115	.02	116
Drug Offenses	-.05	-.01	159	-.08	91	-.06	218	.02	31	-.06	197	-.04	42
Escape	.10	-.07	42	.21	53	.05	60	.13	30	--	--	.14	79

a = $p < .05$.

Table 3. Product-Moment Correlations between SES and Sentence Length within Florida Crime Categories Controlling for Defendant's Race, Age and Sentencing County

		Race				Age				County of Sentencing			
		White		Black		<25		25+		Urban		Non-Urban	
	Zero-Order	r	(N)	r	(N)	r	(N)	r	(N)	r	(N)	r	(N)
2nd Degree Murder	.14	.10	23	-.00	45	.24	20	.12	48	.21	26	.03	42
Vol. Manslaughter	-.01	.01	27	.03	71	.06	32	-.03	65	-.29	33	.18	65
Forcible Rape	-.12	--	--	-.16	61	-.17	52	-.06	25	-.24	36	-.07	41
Aggravated Assault	.05	.12	55	.02	144	-.10	79	.18	119	-.03	81	.12	118
Armed Robbery	-.04	.14	52	-.11	91	-.02	95	-.08	47	-.05	74	-.05	69
Unarmed Robbery	-.03	-.08	109	.03	185	-.04	182	-.06	111	-.10	173	.08	121
Burglary	.09[a]	.12[a]	353	.06	253	.02	373	.13[a]	232	.05	237	.11[a]	369
Larceny	.09	.16	136	-.07	99	-.03	110	.11	124	.02	92	.14	143
Recv. Stolen Prop.	-.08	-.02	43	-.21	24	-.30	30	.05	37	-.26	42	.09	25
Auto Theft	.03	.04	74	-.01	37	-.11	71	.12	40	-.03	36	.07	75
Forgery	-.02	.04	157	-.18	82	.07	82	-.08	157	.02	99	-.04	140
Drug Offenses	-.05	-.05	214	-.04	39	-.07	180	-.03	70	-.11	143	-.02	110
Escape	.10	.11	74	-.01	22	-.05	57	.21	39	--	--	.09	84

a = $p < .05$.

Table 4. Step-Wise Multiple Correlation with Sentence Length as Dependent Variable and Seven Independent Variables for Florida Crime Categories

	0-Order		1-Order		2-Order		3-Order		4-Order		5-Order		6-Order	
	R	R^2	R	R^2	R	R^2	R	R^2	R	R^2	R	R^2	R	R^2
2nd Degree Murder (N = 64)	.26	.07	.35	.12	.36	.13	.37	.13	.37	.14	.37	.14		
	Race		Urbani-zation		SES		Prior Convictions		Juvenile Institution		Arrests			
Vol. Manslaughter (N = 95)	.22	.05	.27	.07	.31	.09	.32	.11	.34	.12	.35	.13	.36	.13
	Prior Convictions		Juvenile Institution		Urbani-zation		Race		Age		Arrests		SES	
Forcible Rape (N = 73)	.13	.02	.17	.03	.21	.04	.25	.06	.27	.07	.28	.08	.28	.08
	Race		Urbani-zation		SES		Age		Arrests		Prior Convictions		Juvenile Institution	
Aggravated Assault (N = 188)	.15	.02	.20	.04	.20	.04	.21	.04	.21	.04	.21	.04		
	Age		Prior Convictions		Race		Urbani-zation		SES		Arrests			
Armed Robbery (N = 140)	.17	.03	.20	.04	.22	.05	.22	.05	.22	.05	.22	.05	.23	.05
	Prior Convictions		Urbani-zation		Race		Arrests		Age		SES		Juvenile Institution	
Unarmed Robbery (N = 270)	.16	.03	.20	.04	.21	.04	.22	.05	.23	.05	.23	.05	.23	.05
	Age		Urbani-zation		Race		Arrests		Prior Convictions		SES		Juvenile Institution	
Burglary (N = 557)	.18	.03	.21	.04	.22	.05	.23	.05	.23	.05	.23	.06	.23	.06
	Prior Convictions		SES		Age		Juvenile Institution		Urbani-zation		Arrests		Race	
Larceny (N = 225)	.20	.04	.25	.06	.29	.08	.30	.09	.31	.09	.31	.09	.31	.09
	Prior Convictions		Juvenile Institution		Urbani-zation		Age		SES		Arrests		Race	

Table 4. (continued)

	0-Order		1-Order		2-Order		3-Order		4-Order		5-Order		6-Order	
	R	R^2	R	R^2	R	R^2	R	R^2	R	R^2	R	R^2	R	R^2
Recv. Stolen Prop. (N = 58)	.23	.05	.27	.07	.31	.10	.32	.10	.33	.11	.33	.11	.33	.11
	Prior Convictions		Juvenile Institution		Age		SES		Urbani-zation		Race		Arrests	
Auto Theft (N = 106)	.27	.07	.34	.12	.41	.17	.42	.18	.43	.18	.43	.19	.44	.19
	Urbani-zation		Arrests		Juvenile Institution		Age		SES		Prior Convictions		Race	
Forgery (N = 221)	.19	.04	.23	.05	.24	.06	.24	.06	.24	.06	.24	.06	.24	.06
	Juvenile Institution		Prior Convictions		Arrests		Age		Race		SES		Urbani-zation	
Drug Offenses (N = 233)	.20	.04	.26	.07	.27	.07	.28	.08	.29	.09	.29	.09	.29	.09
	Urbani-zation		Age		Arrests		Race		SES		Prior Convictions		Juvenile Institution	
Escape (N = 88)	.18	.03	.21	.04	.23	.05	.25	.06	.26	.07	.26	.07	.26	.07
	Age		Urbani-zation		Arrests		SES		Prior Convictions		Race		Juvenile Institution	

By the fifth-order, SES, prior convictions, juvenile institution and arrests have been added, providing an R = .37 with six independent variables in the equation.

An overview of the table indicates that for all of the crimes the zero-order correlation ranges between .13 and .27 and by the sixth-order the range in R is from .23 to .44, indicating that our ability to predict sentence length is not greatly improved by the addition of these variables. The largest R^2 is .19 for auto theft, indicating that only nineteen percent of the variance in sentence length has been accounted for by these variables. Overall, the R^2 exceeds .10 for only four crime categories. For aggravated assault, with an R = .21 and R^2 = .04, only four percent of the variance is explained. It is readily apparent that we are able to explain a minimal amount of the variance in sentence length for any of the crime categories on the basis of the variables included in this study.

The reader should also note that after the third-order correlation, the value of R is virtually unaffected by the inclusion of additional variables. The largest increase in R between the third-order and sixth-order is .04 (.32 to .36 for voluntary manslaughter). In fact, for most of the crime categories, there is little gained after the first-order relationship. The largest increase in R between the first-order and the sixth is for rape and the difference in R is only .11 (.17 to .28).

In the zero-order relationship, the first variable included for five of the thirteen crime categories is "prior convictions." Age enters first for three crimes, race and urbanization for two and juvenile institution for one. SES does not enter at the zero-order for any crime category, enters at the first-order only for burglary and at the second-order only for second-degree murder and forcible rape. Overall, the inclusion of SES does not improve our prediction appreciably for any crime, with the greatest increase in R^2 being .01 (forcible rape) when SES is added.[18]

[18] Technically, it could be argued that given the obvious relationship between SES and such factors as prior-record and race, their increment to R^2 is partly a function of SES. However, while it is true that "forcing" SES into the equation at an early step would increase its impact on variance explained, that impact will never exceed .04, which is the largest r^2 in Table 1 (zero-order).

The beta coefficients for the final step in the step-wise multiple regression are presented in Table 5 for the various independent variables. This coefficient indicates the amount of change in sentence length which is produced by a standardized change in an independent variable when the other independent variables are controlled. The sign attached to the coefficient also indicates the direction of the relationship. As noted in Table 5 none of the beta coefficients yields a negative value for SES that is statistically significant. The only statistically significant beta for SES is for burglary (.093) and it is positive. The examination of the beta coefficients adds confidence to the interpretation given the zero-order correlations in Table 1. In fact, after the effects of the other variables are controlled in the beta coefficients, two of the offenses which had yielded negative zero-order correlations in Table 1 (voluntary manslaughter and aggravated assault) produce positive beta coefficients in Table 5.

In examining the beta coefficients for the other independent variables, "prior convictions" is the only variable that provides a consistent pattern across the crime categories. Eleven of the thirteen offenses yield positive beta coefficients for "prior convictions" and seven of them are statistically significant. The other variables either provide few statistically significant beta coefficients or they have different signs for different crimes. Urbanization, for example, has six beta coefficients that are significant – three positive and three negative.

SUMMARY AND CONCLUSIONS

What the foregoing data suggest, rather conclusively, is that the socioeconomic status of convicted criminal offenders is unrelated to the severity of the state's official sanction, as reflected in the length of prison terms assigned by the courts. Such a conclusion, which strongly contradicts common folklore, the general expectations of conflict criminology and the specific proposition of Chambliss and Seidman (1971), is given added credence by the fact that it is true for a total of seventeen different criminal offenses and for three separate states. Furthermore, that conclusion is sustained (for Florida) regardless of the age, race, number of prior arrests, felony convictions or juvenile commitments

Table 5. Beta Coefficients for Seven Independent Variables with Sentence Length as the Dependent Variable, at the Final Step of Multiple Regression for Florida Crime Categories

	SES	Prior Convictions	Arrests	Urbanization	Juvenile Institution	Age	Race
2nd Degree Murder	.093	.077	.020	-.240[b]	.072	--	-.175
Vol. Manslaughter	.064	.256[c]	-.113	-.153[a]	-.220[c]	.148	.130
Forcible Rape	-.170	-.105	.141	-.154	-.024	-.205	.060
Aggravated Assault	.029	.148[b]	-.024	.041	--	-.173[c]	.057
Armed Robbery	-.021	.187[b]	.045	.105	.020	-.033	.081
Unarmed Robbery	-.038	.094	-.099	.123[c]	.029	.162[c]	.068
Burglary	.093[c]	.127[c]	-.047	-.038	-.072[b]	.130[c]	-.005
Larceny	.057	.290[c]	-.036	-.145[c]	.175[c]	-.097	-.018
Recv. Stolen Prop.	-.074	.330[b]	.023	.070	.185	-.209	.025
Auto Theft	.092	-.110	.263[b]	.179[b]	-.271[c]	.148	.066
Forgery	-.008	.132[a]	.064	-.007	-.157[c]	-.051	-.010
Drug Offenses	-.066	.035	.086	.207[c]	-.008	.156[c]	-.093
Escape	.090	.077	-.188	-.132	-.015	.281[a]	-.038

a = $p < .05$; b = $p < .01$; c = $p < .001$.

of the defendant and regardless of the "urban" or "non-urban" character of the sentencing county. In fact, of a total of 185 zero-order correlations computed between SES and sentence length, only *one* was statistically significant in the direction hypothesized by the conflict perspective; and surprisingly, ten of the *positive* correlations proved to be statistically significant.

The implications of such findings can be understood at several levels of generality. Most specifically, it would appear that Chambliss and Seidman's (1971:475) proposition that "when sanctions are imposed, the most severe sanctions will be imposed on persons in the lowest social class," will have to be abandoned, or at least modified. As presently formulated, that proposition constitutes a "sufficient" proposition (If X, then Y, regardless), and as noted by Zetterberg (1963:16): "sufficient propositions are rare in sociology and contingent propositions [If X, then Y, given Z] are the rule." Thus, conflict criminologists may wish to salvage the proposition relative to sentencing by stating it in contingent terms that would stipulate its truth only under certain conditions – which obviously were not met within the parameters of the present research.

For example, since the present findings apply only to adult felons sentenced to state prisons, one might still hypothesize that class or status related sentencing bias could be shown for juveniles, or for misdeameanants or for those placed on probation. Though neither Chambliss and Seidman (1971) nor other conflict criminologists offer a specific rationale for expecting the influence of SES to be greater in the sentencing of *less* serious offenders (juveniles, misdemeanants, probationers) the basis for such an expectation seems reasonable. Specifically, one could argue that both the defendant *and* his crime constitute more or less of a threat to the established order of society – whether that order is conceived in functional or conflict terms. One might further argue that lower status defendants constitute more of a "symbolic" threat to the interests of the powerful than do higher status defendants, who, by virtue of their higher status obviously share important segments of a symbolic universe (Berger and Luckmann, 1966; Lofland, 1969) with those who impose the sanctions of the state. Thus, when the crime is most serious and the threat to established interests most clear, there may be less margin for tolerance and sanctions may be relatively consistently applied – for all status groups. However, when the offense poses less of a generic threat, the sanction imposed may be more responsive to the symbolic threat generated by the defendant – thereby allowing for greater variance in sentencing by SES.[19]

However plausible such an argument, the existing evidence relative to sentencing of juveniles, probationers and misdemeanants does not offer much support for even this contingent conflict proposition. As noted above, neither Terry (1967) nor Thornberry (1973) could find negative correlations between SES and juvenile sentencing that exceeded -.10; and with regard to probationers, the relevant evidence is even more limited. Chiricos, Jackson and Waldo (1972) did show that the likelihood of adjudication being withheld for those adults sentenced to probation, was little affected by the defendant's employment status (full-time, part-time, unemployed) or level of occupational skill (professional, skilled, unskilled); however, indigents and defendants with little

[19] With the discussion of symbolic threats, it may appear that we have strayed from the conflict perspective which is central to this paper. Such is not the case, however, for this discussion simply explores one of several plausible arguments for *how and why* the relationship between SES and sentencing could exist. It seems clear that a variety of sociological perspectives – including interactionist and functionalist (see footnote 17) could account for an SES/sentencing relationship, but the concept of threat – with all of its psychological connotations – may facilitate an explanation of the hypothesized SES/sentencing relationship that is most consistent with conflict criminology. For example, if one were to ask a conflict criminologist *why* a conflict of interests is likely to lead to criminal sanctions (Quinney, 1970) it would be surprising if his answer did not express or imply a consideration of the necessity to repress the threat to established interests posed by those who are sanctioned. Similarly, if one asked *why* lower status offenders are expected to receive harsher sanctions (Chambliss and Seidman, 1971), he could plausibly reply that such offenders represent more of a threat to the class interests of those who generate and administer the law. Indeed, the concept of threat, which is central to Lofland's (1969) work on deviance, is one which conflict theorists might fruitfully employ, regardless of whether they conceptualize conflict at the level of individuals, interests, classes or nations.

education were more likely to be adjudicated than those with private counsel or more education. The sentencing of misdemeanants has not generated data appropriate for the proposition in question, and this dimension of the issue remains unresolved.

Specifying the contingencies within which negative correlations between SES and sentencing could be hypothesized is a relatively boundless task. For example, it may be expected to hold true—though for no apparent theoretical or empirical reasons—in northern states, but not southern states; for females, but not males, given certain victim–offender relationships, but not others; or for victimless crimes, but not for those with a consensus of community approbation. The latter possibility, though not offered with any serious intention, receives some minimal support within the data presented above. Specifically, one can observe that the greatest consistency of hypothesized negative correlations (thirteen of fifteen) was found for the category of "drug offenses," a category which, unfortunately, encompasses a broad and varied mixture of specific criminal activities. In any event, future considerations of the SES/sentencing relationship, both at a theoretical and empirical level, may wish to introduce whatever limiting contingencies seem reasonable, and the acceptability of this conflict proposition would seem to require such specification.

At a more general level, the data presented herein speak to the accuracy of that alternative "world view" which conflict theorists have proposed as a substitute for other perspectives—particularly the functionalist perspective—in the study of crime and social control. That "world view," and its underlying domain assumptions, generally purports to use such concepts as "interests," "power" and "conflict" to account for what has been previously (and presumably mistakenly) understood in terms of "pathology," "dysfunction" and "disorganization." The merit of such an alternative model will be determined, in part, by its ability to make sense of the observations which are commonly made by students of crime.

Certainly, there has been no more persistent and reliable observation made by criminologists than that which concludes that lower status persons are the most likely to be designated as official criminals or delinquents. Traditional sociological theory in criminology, operating within a strongly positivistic orientation (Jeffery, 1960; Matza, 1964; 1969), ignored the variables of criminal justice and process, took the correlation between low SES and high rates of crime as both given and real, and attempted to explain such correlations in terms of various personal and environmental characteristics of the *offenders*. Several (Merton, 1938; Cohen, 1955; Cloward and Ohlin, 1960) actually tried to specify the unique strains that were experienced by lower status Americans which would account for their more frequent criminal or delinquent *behavior*. Subsequent research, however, (Nye et al., 1958; Dentler and Monroe, 1961; Akers, 1964; Erickson and Empey, 1963; Voss, 1966; Williams and Gold, 1972) has strongly suggested that the assumption of more frequent law violating *behavior*, at least among lower status *juveniles*, may not be warranted. Such findings, of course, make all the more attractive any perspective such as "labeling" or "conflict" which directs our attention to variation in legal process, rather than criminal behavior.

As noted above, awareness of conflict and its attendant consequences have led a variety of criminologists to generate propositions which *could* account for class or status distributions of official criminals in terms that emphasize the general working of the legal process. However, the evidence—including that presented in this paper—consistently fails to support the expectations of conflict criminologists with regard to the state's sanctioning machinery. This conclusion appears warranted both at the police level and at the level of the courts. In short, the capacity of conflict criminology to account for variations in criminal process must be seriously questioned at this point and it is certain that conflict does no better than traditional perspectives in criminology when it comes to explaining—in empirically sustained terms—the class and racial characteristics of convicted and sentenced criminals. In fact, it remains an abiding challenge to criminologists of all theoretical persuasions to come up with an explanation that *can* make sense of this basic and reliable empirical observation.

Finally, since it has been argued (Chambliss, 1973:1) that the work of conflict criminologists is at the heart of a "gigantic struggle" between conflict and functional

paradigms within sociology, it is appropriate to consider the implications of the present work for that "struggle." Indeed, it would appear that the present findings, and all of those findings relating to bias in the legal process, are limited in their general implication. This limitation would appear to have two sources. First and most obviously, findings relative to criminal sentencing or to other dimensions of legal process, speak to but one of a variety of issues that centrally concern conflict theory in sociology. While it is true that legal institutions and their deliberate control by specific limited interests within the state represent key considerations for some conflict theorists, it is also true that the fullness of conflict theory in sociology encompasses a host of issues (e.g., alienation, capitalism, social change, ideological development, etc.) that transcend the limited matters of criminal justice. Second and perhaps more important, empirical findings that address the specific propositions of conflict criminologists, such as Chambliss and Seidman (1971), Quinney (1970) or Turk (1966; 1969), do not directly address the more general propositions of conflict sociologists because the requisite links between the more general and the more specific concepts and propositions have not been made. In short, the conflict criminologists have generally failed to establish precise links between their criminological propositions and the more general sociological propositions or concepts which they presumably reflect. For example, Turk's (1966; 1969) propositions dealing with conflicts between "authorities" and "subjects" are developed without mention, much less discussion of Dahrendorf's (1959) conflict model involving authorities and subjects, while the Chambliss and Seidman (1971) volume makes but three brief references to Marx. Indeed, the relevance of criminological conflict models—particularly those of Turk and Quinney—for more general conflict issues have been more carefully developed by one of their critics (Taylor et al., 1973:237-67) than by the authors themselves.[20]

At this stage in the development of a conflict perspective in criminology, two lines of inquiry would appear to hold the greatest promise. The first would involve the careful derivation of propositions concerning the legal process from the concepts and propositions of more general conflict sociologists. As noted above, the work of Taylor et al. (1973) and Quinney (1973) represents an important first step toward this kind of theoretical integration, but they have been more concerned with general theoretical implications than with the derivation of testable propositions. The second line of inquiry would involve the wider accumulation of empirical evidence relative to existing conflict propositions than has heretofore been accomplished. It is only with the accumulation of such evidence that the viability of conflict as an alternative "world view" can be established.

[20] Chambliss' (1973) recent discussion of functional and conflict theories of crime represents the kind of effort needed before conflict criminology and conflict theory generally will become reasonably well integrated. However, even that work, which focuses primarily on Marx, fails to draw specific implications from the consideration of Marx for existing or possible propositions in conflict criminology.

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COMPONENTS OF SUSTENANCE ORGANIZATION AND NONMETROPOLITAN POPULATION CHANGE: A HUMAN ECOLOGICAL INVESTIGATION

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An assumption basic to human ecological theory, as developed by Gibbs, Hawley, Martin and others, states that a significant relationship exists between sustenance organization and population growth or decline. The present investigation transforms this assumption into an empirically verifiable hypothesis. Several components of sustenance organization are operationalized and found to account for a substantial proportion of the variation in relative population change in the nonmetropolitan counties of the United States between 1960 and 1970. The efficacy of sustenance activities as an explanation of population change is tested against a number of competing hypotheses focusing on age and racial composition, economic opportunities and proximity to metropolitan areas. Collectively these alternative explanations add little to the amount of variation in population change accounted for by components of sustenance organization. The implications of these findings for ecological theory are discussed in some detail.

INTRODUCTION

A fundamental tenet of human ecology asserts that a population will tend to redistribute itself through the vital processes (fertility and mortality) and through migration (far and away the most efficient mechanism) so as to achieve balance or equilibrium between population size and life chances (Hawley, 1968:331). A recent analysis of black male migration from 253 Southern cotton-belt counties suggests that population redistribution may be viewed as a direct "demographic response to *differences in sustenance organization*..." (Sly, 1972:615, emphasis added). Although the study provided a test of the hypothesis that ecological organization exercises a direct influence on migration while mediating the effects of technology and the environment, there was no assessment of the assumed differential impact of various types of sustenance activities.

The primary objective of the present research is to transform an assumption basic to most studies in human ecology, *viz.*, that a significant interrelationship exists between sustenance organization and population growth or decline, into an empirically verifiable hypothesis applicable to population change in general rather than to migration in particular. A second goal is to determine the nature of the relationships between *various components* of sustenance activity and population change. Several dimensions of sustenance organization are identified and operationalized. These components are then related to relative population change in the nonmetropolitan counties of the United States between 1960 and 1970.

Population change in the nonmetropolitan United States is a phenomenon which has received rather limited attention in recent years. Quite understandably, demographic and ecological research has focused on metropolitan areas where approximately two-thirds of the nation's population is located. As a consequence, we know comparatively little about population dynamics and patterns outside of metropolitan centers.[1] This is an

[1] The word "comparatively" is used advisedly here in order to contrast the amount of attention given metropolitan, as opposed to nonmetropolitan, studies. The work of Beale (e.g., 1964; 1969), Fuguitt (e.g., 1965a; 1965b; 1965c; 1971), Tarver (1972) and others, that has added substantially to our understanding of nonmetropolitan population dynamics is explicitly and gratefully acknowledged.

unfortunate situation, for taken in either absolute or relative terms, the remaining one-third of the population represents a significant number of persons. Moreover, popular misconceptions notwithstanding, not all nonmetropolitan counties are characterized by declining population. Of the 2623 nonmetropolitan counties (which comprise 84% of all U.S. counties) for which comparable demographic data are available, half (1312) gained population between 1960 and 1970.

Beale (1972) has reminded us of the considerable diversity observable in the structure and trends of nonmetropolitan population change. These differences must be recognized "if intelligent policies toward population distribution... are to be developed" (Beale, 1972:676). Of course, one explanation for the growth of nonmetropolitan counties may simply be that many of these units are not really "nonmetropolitan." That is, although they are not official components of Standard Metropolitan Statistical Areas (SMSA's), the location of a number of nonmetropolitan counties may place them within the sphere of dominance of a large urban center, thus permitting the outlying population to benefit from economic opportunities associated with the metropolis[2] (cf. Fuguitt, 1965; 1971; Hawley, 1971:219-40). We endeavor to go beyond this form of explanation and examine the relationships between specific components of sustenance organization and nonmetropolitan population change. The nature of these relationships is not well understood by human ecologists, despite the key role that sustenance organization assumes in ecological theory (e.g., Gibbs and Martin, 1959:30). Nonmetropolitan areas thus provide the context for a confrontation of important theoretical issues.

THEORETICAL PERSPECTIVE

General Conceptual Framework

Before proceeding further, it will be useful to specify theoretical antecedents, since even the most rigorous data analyses contribute to the cumulation of knowledge only to the extent that empirical results can be placed in the context of a broader conceptual framework. The orientation of this research is that of human ecology. In the most basic sense, human ecology "is concerned with the general problem of organization as an attribute of population" (Hawley, 1968:329). Specifically, "man survives through collective organization...[but] not all populations organize themselves... in exactly the same manner. It is in this variability in the characteristics of sustenance organization that human ecology finds its fundamental problem" (Gibbs and Martin, 1959:30).

A sustenance activity may be identified as any "expenditure of human energy in the direct pursuit of food or in the production of some good or service" (Browning and Gibbs, 1971:233-4). However, only the most elementary society will be characterized by a single and isolated sustenance activity. Most are characterized by complex configurations of sustenance functions, and it is precisely these components of sustenance organization in the nonmetropolitan United States that provide the focal point for this paper.

Our investigation has its roots in two related, yet distinct, research traditions in human ecology. One tradition has been characterized by its concentration on the division of labor, i.e., the "differences among members of a population in their sustenance activities and the related functional interdependence" (Gibbs and Poston, 1975). In a series of empirical inquiries, Gibbs and his colleagues have specified the number of sustenance activities engaged in by a population and the extent to which members of a population are evenly (or unevenly) distributed in these activites (cf. Gibbs and Martin, 1958; 1962; Gibbs and Browning, 1966; Browning and Gibbs, 1971). However, seldom has such research been directly concerned with identifying the specific substantive components of sustenance organization.

Another major interest pursued by human ecologists has focused attention on urban and metropolitan centers in an effort to discover the principal sustenance activity, or set of activites, in each area. Having identified the key function (or functions), an attempt has then been made to demonstrate the manner in which these sustenance activites are integrated into a system of urban and metropolitan centers (cf. Duncan et al., 1960; Galle, 1963;

[2] The effect of proximity to SMSA's is explored in more detail in a later section.

Duncan and Lieberson, 1970).

The present paper recognizes the central contributions of these earlier analyses. In order to develop a thorough comprehension of the ecological structure characteristic of a population, it is necessary to pay attention to the number of sustenance activities, one of the concerns of Gibbs and his colleagues in their analyses of the division of labor. Similarly, identification of the key sustenance function, one of the interests of the Duncans and their colleagues, is central to human ecological research. Our concern with ascertaining the specific elements of sustenance organization follows somewhat from both traditions. Moreover, since we intend to relate differences in sustenance organization to population change, it becomes even more important to distinguish among the components of sustenance organization, for it is inconceivable that all sustenance activites relate in the same way to population change.

Previous research has identified several distinct configurations of sustenance activities in the nonmetropolitan counties of the United States (Frisbie and Poston, 1974). Among the sustenance activities previously delineated are two types of agricultural production (large-scale commercial agriculture and the more traditional, small-scale agricultural enterprise), mineral industry, manufacturing and four types of service functions (retail, wholesale, educational and public administration services). But the extent to which sustenance organization in general, and its various components in particular, are related to population growth or decline in nonmetropolitan counties has not been adequately investigated.

Specific Implications

In this section, we concentrate specifically on the nature of the relationships between the various types of sustenance activities and population change. Counties dependent on primary or extractive sustenance activities are likely to experience population loss. As extractive industry becomes increasingly mechanized, productive and capital intensive, there is less demand for labor (Jones, 1967:403-6; Hawley, 1971:154-5). Such developments require that a population adjust its size, principally through out-migration, and population decline is the consequence. Because migration is selective of young adults, the age distribution of the population left behind tends to become skewed in the direction of the older age categories. This in turn leads to fewer births and more deaths.

On the other hand, capital intensive agricultural production need not necessarily culminate in a dimunition of employment opportunities. Areas in which commercial agriculture is pursued successfully may enjoy expansion of job opportunities (and population growth) as a complex of ancillary agribusiness establishments develops. Farmers with extensive land holdings and high volume production are more apt to require, and to be able to afford, labor involved in the actual production of commodities. To illustrate, it is possible for a farmer with, say, two or three hundred acres, to manage his operation more or less singlehandedly with a certain degree of mechanization and the occasional use of day-laborers; but the operator with 1000 or more acres will almost certainly require the services of additional workers on a more or less regular basis. Even though highly mechanized, the latter enterprise often will be too large for one individual or one family to manage.

The situation with respect to manufacturing is difficult to unravel, partially because most research has taken towns or cities rather than counties as the unit of analysis. However, the literature does provide information on the nature of the relationship between manufacturing and changes in population size. From their study of metropolitan and nonmetropolitan communities, Duncan and Reiss (1956) concluded that the proportion of the labor force engaged in manufacturing tends to vary inversely with population growth. More recently, Tarver (1972:53-72) has demonstrated that the industrial structure at the beginning of a decade has a pronounced influence upon population change in the ensuing decade, with places classified as "multiple-specialty" towns having the highest rate of population increase and "one-specialty" towns the lowest rates of increase, or in some cases, population decline. (See also Groth, 1975, for further evidence bearing on the relationship between functional specialization and population change.) It would seem therefore that manufacturing might be either positively or negatively related to population change, depending on its diversity or com-

plexity as a sustenance function.

It is anticipated that the probability of population increase will be greatest in nonmetropolitan counties where services constitute the key function. The rationale for this expectation recognizes the fact that "the balance of employment in the United States has shifted dramatically . . . in favor of the service industries" (Fuchs, 1968:2). There has been a rapid growth in the demand for wholesale, retail and other distributive services, as well as for the performance of financial, educational and administrative functions (Fuchs, 1968; Bell, 1973).

In summary, the foregoing suggests the following working hypotheses regarding population change in nonmetropolitan counties: (1) areas heavily dependent on primary industry such as mining or agriculture (with the possible exception of large-scale commercial agriculture) are likely to be population-decline areas; (2) areas where services constitute the most significant form of sustenance activity are likely to be characterized by growing populations; (3) areas dependent on transformative industry are expected to be intermediate in terms of growth potential. But since expansion of the size of formal organizations implies an increase in complexity and thus at least some increase in personnel required for coordination (Blau, 1970:201-18; Kasarda, 1974: 19-28), it seems reasonable to predict a positive relationship between growth of manufacturing and population change. Variables employed in assessing the tenability of these hypotheses are operationalized and more precise predictions are made in a subsequent section.

METHOD

Data

The percentage change in the population of 2623 nonmetropolitan counties of the 48 contiguous states between 1960 and 1970 was calculated from figures recently tabulated by the Census (U.S. Bureau of the Census, 1971).[3] Utilizing *circa* 1960 data from the *County and City Data Book, 1962*, measures of eight components of sustenance organization were developed. Relative population change scores were then regressed on these eight measures to determine the impact that the various configurations of sustenance activity at the beginning of the decade had on 1960-1970 population change.

Index Construction

As alluded to above, previous research indicates that the sustenance organization of nonmetropolitan counties may be defined in terms of one or more of eight specific activities: general, small-scale agriculture; large-scale, commercial agriculture; mineral industry; manufacturing; retail, wholesale, education and public administration services. Indexes for the first six dimensions were constructed, while educational and administrative services were measured by a single indicator.

The variables included in the development of the measures were selected according to the following rationale. The kinds of measures desired were those relating more or less directly to the manner in which a population sustains itself. Sustenance activities, whatever their form, are broad in scope and will not always admit to measurement with a limited number of variables. However, we would agree with Hauser (1956:486) that one of the most fruitful ways of ascertaining the sustenance functions of a community is through an examination of the industrial and occupational features of the area's labor force. We would further hold that any activity in modern society which yields money may be classified as a sustenance activity (Sly, 1972:616). The variables chosen thus reflect the industrial composition of the area's labor force, as well as the kinds and amounts of capital produced or brought into the area. Typical of the variables included are percent of the labor force employed in agriculture; labor expenses per farm; manufacturing payroll per capita; wholesale receipts per capita and so on (see Table 1).

[3] As in other studies of U.S. counties (e.g., Bonjean et al., 1969), Alaskan election districts, certain independent cities in Virginia, the District of Columbia and National Park counties are excluded from the analysis. Also excluded are the counties of Hawaii (three of which are nonmetropolitan). Included are all other nonmetropolitan counties for which the necessary 1960 and 1970 data are available.

Table 1. Item Composition and Reliability of Sustenance Organization Indexes*

Index	Reliability Coefficient (Ω)	Items Included in Index
General Agriculture	.860	% employed in agriculture, 1960 % of all land in farms, 1959 % of farms operated by tenants, 1959 % of farms with full-time operator, 1959 % of population with rural farm residence, 1960
Commercial Agriculture	.894	Labor expenses per farm, 1959 Average total value of farms, 1959 Value of products sold per farm, 1959 % of farms that are large commercial, 1959 % of farms of 1000 acres or more, 1959
Manufacturing	.877	% employed in manufacturing, 1960 Value added by manufacturing, 1958; per capita, 1960 % employed in large manufacturing establishments (100 or more employees), 1958 New manufacturing capital expenditures, 1958 Manufacturing payroll, 1958; per capita, 1960
Minerals	.848	% employed in mineral industry, 1960 Mineral receipts per establishment, 1958 Mineral industry payroll, 1958; per capita, 1960
Wholesale Services	.923	% employed in wholesale trade, 1960 Wholesale receipts, 1958; per capita, 1960 Wholesale services payroll, 1958; per capita, 1960
Retail Services	.920	% employed in retail trade, 1960 Retail receipts, 1958; per capita, 1960 % employed in financial, insurance and real estate services, 1960 Retail services payroll, 1958; per capita, 1960

*Source: U. S. Bureau of the Census, County and City Data Book, 1962. Washington, D. C.: U. S. Government Printing Office, 1962.

A factor analysis (principal axes with iteration) was performed for each set of variables, i.e., for each sustenance activity. In every case except one, a single factor solution was obtained. An interesting aspect of the analysis was the instance where a one-factor solution was not forthcoming. It had been assumed *a priori* that retail and wholesale functions would be so highly interrelated that they would constitute a single dimension. As it turned out, two distinct factors emerged—one obviously a "retail" dimension, the other clearly a "wholesale activities" dimension. Accordingly, separate indexes were created and a single-factor solution was obtained for retail services, on the one hand, and for wholesale services on the other.[4]

[4] Included as one item in the retail services index is the percentage of the labor force employed in financial, insurance and real estate industries. Although this variable does not reflect the

The six indexes are made up of linear combinations of the standard scores of from three to five items. Within-index, inter-item correlations are strong, ranging from .4 to approximately .9. The reliability of each index was determined by means of Heise and Bohrnstedt's omega coefficient (Heise and Bohrnstedt, 1970). The items (i.e., variables) included in each index, along with the reliability coefficients (which proved to be quite satisfactory, varying in magnitude from .848 to .923), appear in Table 1.

The seventh and eighth sustenance activities are operationalized in terms of the percentage of the labor force (1960) employed in educational services and public administration, respectively.

Table 2. Standardized Regression Coefficients between Population Change and Eight Components of Sustenance Organization: 2623 Counties of the United States, 1960-1970

Variables	Standardized Regression Coefficients
General Agriculture	-.38702
Mineral Industry	-.19421
Retail Services	.16972
Wholesale Services	-.11984
Commercial Agriculture	.11615
Educational Services	.07098
Manufacturing	.05921
Public Administration Services	.03692

Multiple Correlation Coefficient (R) = .48.

Multiple Coefficient of Determination (R^2) = .23.

performance of retail services per se, it does correlate highly with other items of the index (.5 or more) and emerges as one element of the single-factor solution.

RESULTS

Predicated on the theoretical statement outlined above, an inverse relationship was expected between the general agriculture and mineral industry indexes and percentage change in population size. A positive association was predicted between relative population change and scores on each of the other sustenance variables (commercial agriculture, manufacturing, and retail, wholesale, educational and public administrative services). The findings, for the most part, are as predicted. Table 2 presents the results of the regression of relative population change for 2623 nonmetropolitan counties on the eight components of sustenance organization. The general agriculture and mineral industry indexes are inversely related to population change, as is the wholesale services index. All other predictor variables have positive coefficients. The variation in population change accounted for by sustenance activities is substantial, though far from overwhelming (R = .48; R^2 = .23). Not surprisingly, given the limited viability and consequent sharp decline of small agricultural enterprise (Beale, 1964; Jones, 1967:401-12), the negative impact of general agriculture is the single most significant effect.

The reasons for the unexpected negative relationship between wholesale services and population change are far from clear.[1] A plausible *post hoc* explanation is that, paralleling the situation in many other industries, smaller wholesalers are unable to compete with their larger counterparts. More to the point perhaps, the effect in terms of diminished employment opportunities may be much more acute in wholesaling than in the other service categories. To illustrate, few, if any, *jobs* may be lost (though certain individuals may find themselves unemployed) when small retailers succumb to the pressure engendered by the economies of scale enjoyed by their larger competitors. That is, as larger retail outlets gain clientele who previously patronized small businesses, the former will no doubt employ additional clerks and other personnel to meet the demands of new customers. By the very nature of the business conducted, retail, educational and public administrative services tend to require individual, personalized contact with clients or customers. By contrast, wholesalers may be

able to absorb expanding business by the addition of a comparatively few employees, accompanied by a much greater degree of mechanization.[5]

Alternative Hypotheses

Briefly, the question of critical importance for this research is the extent to which the variation in population growth or decline may be accounted for in terms of a concept central to human ecological theory, *viz*., components of sustenance organization. It has been demonstrated that eight sustenance variables explain a significant proportion of the variation in relative population change in nonmetropolitan counties. Nevertheless, it is palpable that there will always remain an indefinite number of competing, alternative hypotheses which may provide the "real" explanation of the phenomenon observed. Thus, it is incumbent upon us to consider some of the more prominent alternative explanations (cf. Blalock, 1972:112-4). It may be that, when certain other factors are controlled, the relationship between population change and sustenance organization will be much less important than it presently appears.

Other factors that have been demonstrated to be related to changes in population size include: *age structure* (Duncan and Reiss, 1956; Chapter 3; Beale, 1964:264-72), *racial composition* (Beale, 1964), economic opportunities as evidenced by *average income* and *employment levels* (Duncan and Reiss, 1956:195-6 and Chapter 9) and *proximity to metropolitan areas* (i.e., to SMSA's; cf. Beale, 1962; Fuguitt and Thomas, 1966).

Age Structure

The age structure of a population may be assumed to be inversely related to population change. *Ceteris paribus*, an older population is less likely than a younger one to be growing rapidly (or to be growing at all). Hence one would expect median age to correlate negatively with population change.[6] A young population will contain a fairly large number of children and young adults so that relatively fewer deaths obtain as compared to populations with a heavy concentration of older persons. Furthermore, migration is selective by age and young adults in particular are prime candidates for migration (Hawley, 1950:340-1; Bogue, 1969). Out-migration of young people not only skews the age distribution at place of origin toward older age groups, but also may cause a decline in the rate of natural increase due to a deficit of persons in the prime child-bearing years.

Racial Composition

A second variable warranting consideration is the racial composition of an area, here operationalized as the proportion of the county population that is nonwhite.[7] Since 1930, birth rates for blacks have exceeded those for whites (Hamilton, 1964;273; Ryder and Westoff, 1971). Beale (1964) has called attention to high black fertility as a possible explanation for the fact that a number of U.S. counties with a farm-based economy have recorded an increase in population rather than the expected decline. We would therefore expect to find a positive relationship between relative population change and proportion of the county population that is nonwhite.

Employment and Income

If population redistribution functions as a mechanism to maintain a balance between size and economic opportunities, it is reasonable

[5] To use a rather homely example, an increase in the volume of business that would create a need for several additional employees in a retail outlet, such as a department store, restaurant or supermarket, might be met at the wholesale level by the purchase of a forklift.

[6] Of course, other things are never equal. For example, there are a number of counties in the United States which are growing rapidly even though they have a disproportionately large number of elderly residents. These are aptly termed "retirement counties" since, for various reasons, they consistently receive a heavy influx of in-migrants of relatively advanced age. But by and large, the expectation of an inverse correlation between median age and relative population change appears to be a plausible one.

[7] It has become conventional to employ percent nonwhite as a surrogate for the proportional representation of blacks in the population. That no great distortion of reality is introduced by such a procedure is apparent when it is noted that of all nonwhites in the population, approximately 90 percent are black (U.S. Bureau of the Census, 1973:iii).

to suppose that the probability of population gain is greater in counties with high levels of employment and income (as reflected by the proportion of the labor force employed and by median family income). Empirical evidence may be adduced in support of this proposition. For example, Duncan and Reiss (1956:203) observed that high-growth areas are characterized by high employment rates. They also found a slight tendency for median family income and population growth to vary together, but suggested that the tenuous nature of the relationship might be explained by the differential effects of race and other variables such as family status (p. 208).[8]

Proximity to Metropolitan Areas

Perhaps the factor most frequently demonstrated to be related to changes in the number of inhabitants of counties, as well as cities, is that of propinquity to large urban centers (e.g., Fuguitt and Thomas, 1966:513-27; Fuguitt, 1971;456-7; DeAre and Poston, 1973:1-6; Frisbie and Martin, 1973:288-96). Nonmetropolitan counties at the periphery of SMSA's may be especially attractive locales in that they combine the advantages of access to opportunities for employment and consumption of goods and services available in the metropolitan area with those of residence in a place relatively free of a number of urban problems and where land values and taxes are not burdensomely high. It seems clear that both size and distance are important variables in this regard (Duncan and Reiss, 1956:151). That is, the larger the SMSA and the closer it is in relation to nonmetropolitan areas within its sphere of dominance, the more the attractiveness of the nonmetropolitan area is enhanced. Accordingly, an index of proximity to SMSA's, weighted for both size and distance, has been included in the analysis (cf. Hathaway et al., 1968; Frisbie and Poston, 1974).[9]

The substantive implications of the foregoing discussion can be summarized as follows: a positive association is expected between the percentage change in nonmetropolitan county size from 1960 to 1970 and (1) the proportion of the population nonwhite, (2) employment rates, (3) median family income and (4) an index of proximity to Standard Metropolitan Statistical Areas (weighted for size and distance). The relationship between median age and population change is expected to be inverse.[10]

In order to explore the relative efficacy of the set of alternative hypothesis, the regression equation which originally consisted of population change and eight components of sustenance organization was expanded to include the five variables just discussed. Inspection of the standardized partial regression coefficients in Table 3 discloses that all relationships for the five additional variables are as predicted, while the direction of the relationships involving the eight original predictors remains exactly the same in every instance. In fact, even the magnitude of the coefficients for the set of sustenance organization variables is not greatly altered.[11] It is also apparent from Table 3 that the increment in explanatory power afforded by the addition of the five alternative variables is slight; the coefficient of determination increases from .23 to .25 (and R from .48 to .50). This is not surprising, for when population change is regressed *only* on the five alternate variables, the multiple correlation is quite low ($R = .06$ and $R^2 < .01$).

The important question now concerns the extent to which variation in population change is accounted for by sustenance organization *over and above* variation attributable to the five alternative predictors. A

[8] The empirical evidence cited is admittedly inferential in that the Duncan and Reiss research focused on communities, not counties.

[9] This measure, adopted from Hathaway et al., places each county on a scale from 0 to 20 with the larger values assigned to counties which are located close to the larger SMSA's. "Counties having values of less than 10 can be assumed to be the most rural," while those with values of more than 10 can be assumed to be more "urban," i.e., more under the influence of large metropolitan areas (Hathaway et al., 1968:12-8).

[10] All data utilized in measuring the five "alternative variables" are for 1960.

[11] It can be stated with some confidence that multicollinearity does not confound efforts to specify the direct effects of the indicators employed. Of the (N) $(N - 1)/2 = 78$ possible pairs of the 13 independent variables, 60 (77%) correlate .2 or less (in absolute terms) while 71 (91%) correlate .35 or less (in absolute terms). The highest single correlation is that between the retail and wholesale indexes where $r = .61$.

Table 3. Standardized Regression Coefficients between Population Change, Eight Components of Sustenance Organization and Five Control Variables: 2623 Non-Metropolitan Counties of the United States, 1960-1970

Variables	Standardized Regression Coefficients
General Agriculture*	-.46447
Mineral Industry*	-.20426
Employment Rate	.15869
Retail Services*	.14223
Wholesale Services*	-.11597
Commercial Agriculture*	.09289
Educational Services*	.06476
Median Family Income	.04782
Proximity to Nearest SMSA (Weighted Size/ Distance Index)	.03664
Median Age	-.03517
Public Administration Services*	.03067
Percent Population Nonwhite	.01154
Manufacturing*	.00912

Multiple Correlation Coefficient (R) = .50.

Multiple Coefficient of Determination (R^2) = .25.

Multiple Partial Correlation Coefficient $r^2_{i\,(jk\cdots n)\cdot t_u\cdots w}$ = .20.

*Sustenance organization variables.

technique singularly appropriate for making the necessary distinction is the multiple-partial correlation coefficient. The multiple-partial represents the proportion of variation explained by independent variables of interest *after* allowing "all control variables to operate first" (Blalock, 1972:458-9). The value of the multiple-partial (.20) indicates that the variation in the dependent variable associated with sustenance organization is virtually unchanged when the five alternate influences (the control variables) are permitted to operate and then are statistically controlled.

SUMMARY AND DISCUSSION

It has been shown that components of sustenance organization account for a substantial proportion of the variation in changes in population size of nonmetropolitan counties of the United States, and that the explanatory power of these indicators exceeds, by several orders of magnitude, that of a number of alternative predictor variables. It will be recalled that when measures of five variables often conceived of as determinants of population change were added to the regression equation, the increase in explained variation was negligible. Further, of the seven largest standardized regression coefficients linking population change to the predictor variables, six reflect different types of sustenance functions. The most significant effect was exerted by the general agricultural dimension which alone accounted for over half of the total variation (r^2 = .15). By comparison, the remaining sustenance variables contributed similar, albeit rather small increments.

Three theoretical issues in particular deserve further discussion: the nature of sustenance organization, the relationship between sustenance organization and population change and the viability of an ecological explanation. In earlier sections of this paper, considerable attention was given to the central role assumed by sustenance organization in human ecological theory. We noted, however, that despite the important work of Gibbs, the Duncans, and their colleagues, there have been few empirical treatments of the specific characteristics or components of sustenance organization and of the variability in each of these components among population aggregates.

The present analysis has operationalized sustenance organization in terms of eight specific components or activities. Each component has been examined individually as it relates to nonmetropolitan population change. These relationships with the dependent variable have not been identical in magnitude or direction. Clearly, sustenance organization involves a complex constellation of activities

as Gibbs, Martin, Hawley and others have argued. Our investigation contributes empirical support for these earlier formulations and warns against analyses of sustenance organization which would treat the phenomenon as unidimensional.

A second issue revolves about the question of the significance of the relationship between sustenance organization and population change. Human ecologists have long contended that sustenance organization plays a major role in population change (Hawley, 1950:101-2, 104, 122-3; Gibbs and Martin, 1959:33-4). Our demonstration that the sustenance activities examined in this paper account for almost one-quarter of the variability in nonmetropolitan population change supports these earlier discussions. It is tempting to go even further and interpret these results as evidence that changes in the size of nonmetropolitan county populations represent a demographic *response* to variation in sustenance organization. We would suggest that this temptation be resisted, however, for perception of the direction of causality in ecological systems is surely a function of the point of analytical departure. Alterations in sustenance organization may indeed necessitate adjustments in population size, but technological and/or environmental changes may require organizational adaptations if system equilibrium is to be maintained. The process is probably best viewed as one of continuous modification.

While it is important to obtain empirical evidence of the assumed interrelationship between sustenance organization and relative population change, and clearly permissible (indeed unavoidable) for analytical purposes to designate a certain variable or set of variables as independent, we do not find full justification for the unqualified conclusion that demographic changes are merely responses to organizational stimuli. Further, the complexity of the relationship as represented by the uneven impact of the different sustenance activities on population change is evidence against such a simple evaluation.

Third, one of the most compelling conclusions emanating from this study concerns the viability of an explanation based on sustenance organization. This explanation was sustained under the pressure of five competing hypotheses. Alternative hypotheses, focusing on age and racial composition, economic opportunities and proximity to metropolitan areas, collectively do not add more than two percentage points to the amount of variation in population change accounted for by the components of sustenance organization. Of even greater interest is the fact that when the competing factors are introduced as control variables, the variation in population change associated with sustenance organization is virtually unchanged. To the extent that this analysis comprises a comparison of competing explanations of population change, the interpretation involving sustenance organization must be adjudged superior.

Gibbs and Martin (1959:33) have contended that a key goal of human ecology is the investigation of the "consequences of the presence or absence of particular characteristics of sustenance organization in human populations." This paper has provided evidence of a clear, though complex, relationship between sustenance activities and population change in nonmetropolitan areas. Future research might appropriately introduce one or more of the remaining axes of the ecological system (technology and the environment), with the awareness that relationships stemming from these added dimensions will likely be as complex, if not more so, than the ones examined here. While the viability of the ecological approach has been demonstrated, considerable work still remains before any claim can be made to an adequate understanding of the dynamics of nonmetropolitan population change.

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SOCIOECONOMIC STATUS AND RELIGIOUS PARTICIPATION*

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The relationship between socioeconomic status and frequency of religious participation is examined for a 1970 U.S. sample of males and females. Although some support is found for the frequently observed positive relationship between these two variables, the data require that such a generalization be qualified. The zero-order relationship generally is stronger for males than females and is positive and weak for Protestants, but is essentially zero for Catholics and negative in sign for Jews and unaffiliated whites. Where the relationship is positive, it is not entirely explainable by the positive relationship of our measure of general social participation with both SES and religious participation. In addition, the examination of interactions with marital status and the presence of children under age 16 indicated that the SES-religious participation relationship is strongest for those who are married and responsible for young children. Even with these significant variations by relevant subpopulations, we conclude that the explanatory power of socioeconomic status in predicting religious participation is small both in absolute terms and in comparison with other possible determinants examined.

Much of the influential theory, research effort and continuing controversy in the sociology of religion centers on the relationship between religious involvement and socioeconomic status. The pervasive interest in this matter is indicated by the theoretical importance attached to both of the possible causal directions of the religion-socioeconomic status relationship. That is, religious involvement has been hypothesized to be both a determinant of, and determined by, socioeconomic position. Both of these causal orderings can be linked to the writings of Max Weber, the former pertaining to Weber's account of the economic consequences of the Protestant Ethic and the latter stemming from "church-sect theory." In each case, there has been considerable attention devoted to the appropriate interpretation of Weber's contentions as well as the applicability of each model to contemporary settings.[1] Despite these debates, however, both the Protestant Ethic and Church-Sect hypotheses have generated a substantial amount of empirical work in which religious involvement is examined either as an independent or dependent variable in relation to socioeconomic variables. And in each area, there has been considerable argument about (1) whether the relationship exists and, if it does, (2) whether it persists after appropriate controls and, if it does, (3) whether it is to be regarded as theoretically important.

In recent years, studies of religious affiliation as a predictor of socioeconomic level have solved many of the logical and methodological problems associated with earlier re-

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[1] For example, it is arguable that the class-religious participation relationship, like other Weberian hypotheses, may be historical, not contemporary. Similarly, it may be argued that Weber did not clearly differentiate the presumed effects of class or occupational status on types of religious organizations as opposed to religious participation (cf. Weber, 1963).

search on the Protestant Ethic hypothesis. Although theoretical problems remain (Bouma, 1970; 1973), several studies, since Lenski (1961) reopened the matter, have brought a heretofore absent order to this area (for example, Gockel, 1969; Jackson et al., 1970; Fox and Jackson, 1973). Path analytic approaches to this topic have been especially informative (Warren, 1970; Finney, 1971; Featherman, 1971). However, the church-sect influenced studies of religious involvement as a dependent variable have not shown comparable development. Although this line of inquiry is associated with a variety of highly regarded contributions (Niebuhr, 1929; Pope, 1942; Yinger, 1946; Wilson, 1959; Johnson, 1963; Festinger et al., 1965; Demerath, 1965; Lofland, 1966; Schwartz, 1970), major issues remain unresolved. As with earlier Protestant Ethic research, researchers in this area have experienced difficulty in explaining the relationship by controlling on theoretically relevant variables and specifying the conditions under which the relationship is strongest. These problems constitute the basis for most of the debate in this area (Lazerwitz, 1961; Dillingham, 1965; 1967; Demerath, 1965; 1967a; 1967b; Goode, 1966; 1967a; 1967b; Estus and Overington, 1970).

The purpose of this paper is to return to this latter research area by examining the socioeconomic status-religious participation relationship with recently collected national sample data and to analyze it with procedures appropriate to addressing the unresolved issues. In particular, our objectives are to: (1) examine the relationship with controls for religious affiliation and gender; (2) determine if the more general "nonreligious participation," which varies by socioeconomic position, explains the relationship observed and (3) determine if the direction and magnitude of the socioeconomic status-religious participation relationship is contingent on two other theoretically relevant variables—marital status and presence of young children in the family.

UNRESOLVED ISSUES

It is now generally accepted that the relationship between socioeconomic status and religious involvement is either negative, positive, curvilinear or random, depending upon which dimension of religious involvement is examined. Thus, "religiosity is apparently not a single dimension. It has various facets which are given various emphases by various social classes" (Demerath, 1965: 26). Studies of socioeconomic status (SES) and religious participation (ritual involvement), however, generally are interpreted to show a positive relationship—that higher status persons are more frequent participants than lower status persons.[2] There are, however, some unresolved issues which pertain to the nature of the relationship, its interpretation and, therefore, its theoretical significance.

One such issue pertains to whether the generally obtained zero-order positive relationship between SES and religious participation holds only for members of organized religious groups or also for the general population of members and nonmembers. Regarding Protestants, Dillingham's (1965; 1967) review and reanalysis of other studies (Bultena, 1949; Burchinal, 1959; Lazerwitz, 1961; Goode, 1966) concludes that among church members, there is no relationship between SES and religious participation. Dillingham's analysis indicates that the positive relationship is obtained for the general population only because lower status persons are less likely to have religious affiliations and, therefore, are less likely to participate than higher status persons.[3].

Data presented by Estus and Overington (1970) indicate that among Protestant church members, participation is positively associated with occupational level but unrelated to educational level. Using their own "social class scale," they find that church participation is unrelated to class among members, so these data may be interpreted to support Dillingham's contention. Other data, however, indicate that the positive relationship holds for members as well as for the general population

[2] Since we frequently refer to socioeconomic status and religious participation, we have adopted the conventional "SES" abbreviation for the former and have simply used "SES-participation relationship" in many instances to refer to the relationship of interest.

[3] Dillingham found that when the data are grouped by denomination, there is a positive relationship between social class and attendance within denominations, but the mean denomination status is negatively associated with mean denominational attendance rates. From this, Dillingham deduces that the two different directions cancel each other out; therefore, the essentially near-zero relationship is found when nonmembers are excluded.

(Stark, 1972). From these findings, then, one would expect to find a positive relationship between SES and religious participation in the general population. However, the relationship for persons with religious affiliations, including Catholics and Jews, is at issue.

A second unresolved issue pertains to the interpretation of these differences in religious participation by socioeconomic position. Much of the early theoretical writing emphasized the problem-solving function of religious involvement for the dispossessed. This interpretation, which often honors Freud, Weber, Nietzche and Marx as the intellectual precedent, took the view that "religion has its greatest relevance to the points of maximum strain and tension in human life" (Parsons, 1957). A body of literature emerged which was interpreted to support the contention that religious involvement functioned to assuage the problems associated with isolation and loneliness (Herberg, 1957; Yinger, 1957), physical or mental illness (James, 1958; Argyle, 1959; Moberg, 1962), fear of death (Malinowski, 1948; Stouffer et al., 1949), economic deprivation (Holt, 1940; Pope, 1942; 1948; Goldschmidt, 1944; Boisen, 1955) as well as institutional alienation (Catton, 1957) and status discrepancy (Demerath, 1965).

Research on the relation between SES and religious participation, however, generally showed that lower status persons were least, not most, involved in organized religion.[4] This pattern suggested to some an alternative interpretation of the SES-participation relationship: that lower status persons are less likely to participate in all organizational activity, religious *and* nonreligious (Lenski, 1953). The appropriate test of this contention, examining the SES-religious participation relationship while controlling for nonreligious participation has, however, produced contradictory findings. Stark's (1964) analysis of 1960 national survey data shows that SES differences in religious participation persist after controlling for nonreligious participation. But Goode's (1966) analysis of Protestants finds that the SES-participation relationship is greatly attenuated with such a control. Goode concluded that the greater church participation of higher status persons is not indicative of a greater religious commitment or interest, but reflects a greater involvement in voluntary organizations generally. Estus and Overington (1970) present data which support this interpretation.

One other theoretical position is relevant to the interpretation issue, one which merges the motivational elements of the dispossession hypothesis with the presumed irreligious character of much church participation. This position, most recently identified with Stark (1964), Demerath (1965) and Marx (1967), views religious involvement as responsive not only to low socioeconomic achievement but to other marginal statuses and roles associated with age, family, gender and ideology. This perspective recognizes functional alternatives to religion in political activism, communal living and psychotherapy (Demerath, 1974) and it also directs attention away from the *degree* of involvement to the *kind* of involvement. One key assumption here is that persons do not select their affiliations randomly without regard for what is to be participated in:[5] This perspective directs research attention toward identifying the conditions under which various social strains or dislocations become translated into one kind of organizational involvement rather than another; thus research should focus on both the degree and kind of religious and nonreligious participation.

A third issue in this literature concerns which additional controls could further specify the relationship, that is, other variables which may interact with socioeconomic status in its influence on religious participation. Previous studies of the SES-religious participation relationship often have controlled for religious affiliation either by ex-

[4] These studies are reviewed by Stark (1964), Dillingham (1965; 1967), Demerath (1965) and Goode (1966).

[5] This is one of the points of disagreement in the Goode-Demerath debate (Goode, 1967a; 1967b; Demerath, 1967a; 1967b). Goode concluded that because church attendance is to be viewed as a specific instance of the more general concept of voluntary organizational participation, it ought *not* be used as an indicator of religiosity at all (Goode, 1966:111). Demerath, however, argues that sociologists should examine the possibility that religious participation, as well as the religion surrogates, may provide some specific meaning in addition to satisfying the affiliation motive, that "we should not regard the church as sociologically vacuous simply because it is non-traditional or even non-religious" (Demerath, 1967b:276).

amining one religious group, usually Protestants, or by statistical controls. These studies generally do not, however, control for gender, marital status or the presence of young children, all of which have been found to be associated with religious participation. It is generally held, for example, that because of role expectations or social marginality, females participate more frequently than males (cf. Argyle, 1959; Demerath, 1965). Research also indicates that marital status and the presence of young children are associated with religious participation. Church membership varies directly with the presence of children under 18 (Nash, 1968). Other data, however, indicate that the absence of children *and* the absence of a spouse both are associated with higher rates of religious involvement (cf. Argyle, 1959; Glock et al., 1967).

Interestingly, neither the theoretical nor empirical literature suggests the *form* of these specified relationships (interactions). One example of this is that the influence of SES on religious participation may depend on marital status. This possible interaction has not been either discussed or tested despite the documentation pertaining to the higher socioeconomic achievements of married males compared to unmarried males (e.g., Blau and Duncan, 1967) and the evidence that religious participation varies with marital status (Argyle, 1959; Glock et al., 1967). A more general complication is posed by whether one should expect higher order interactions which are implied by the additional types of controls often suggested. For example, does the SES-participation relationship differ across subpopulations such as married Protestant males with young children or unmarried Catholic males without young children or married Catholic females with young children, and so on for the thirteen other combinations of gender, religious affiliation (Protestant and Catholic), marital status and presence/nonpresence of children? We have found no theoretical basis in the literature for hypothesizing different forms of the SES-participation relationship over these sixteen subpopulations. At the same time, however, there is an obvious need to examine critically these matters by exploring the existence of such interactions, especially in light of the broad generalizations characteristic of previous literature. This kind of exploration has been recommended such that these controls are introduced explicitly in the analysis of the SES-participation relationship (Goode, 1966).

Following these considerations, we elected to investigate certain interactions, albeit within a context of discovery rather than hypothesis testing. In adopting this strategy we address two questions: first, does the often reported single measure of association between SES and religious participation accurately describe various subpopulations? second, and if the answer to the first question is "no," can the observed differences across these subpopulations be meaningfully interpreted?

A fourth and final matter concerns the kind of analysis appropriate to this topic. It is generally recognized that the state of knowledge within the sociology of religion has been affected in an important way by the difficulties in obtaining religion data on national populations (Petersen, 1964). In American society, such data generally are collected in national surveys as ancillary demographic items, and much of the research literature necessarily involves secondary analyses and suffers the problems associated with them.[6] Another important, and perhaps related, factor that affects the quality of research in this area is the imprecision of the analytic procedures routinely utilized. Tabular analysis, using typically a nominal or ordinal level of measurement, has dominated here.[7] The statistical literature over the past decade is clear with respect to the advantages of more powerful techniques, especially when the dependent variable is measured at the interval level (Bohrnstedt and Carter, 1971).

DATA AND METHODS

The data we utilized were collected by the Institute for Survey Research at Temple University as a part of the national study conducted for the Commission on Obscenity and Pornography. The data presented here repre-

[6] The data reported here are no exception and the absence of denominational data weakens our contribution.

[7] It may be noted that the religious surveys and analyses conducted by Charles Glock and his associates, now recognized as "the prime mover in bringing the sociology of religion into the age of survey research" (Newman, 1973), have consistently used the tabular analytic approach, a preference which has generated important critical commentary (Middleton, 1973).

sent a subsample of the total data collected via personal, face-to-face interviews within a nationwide probability sample of 2482 adults aged 21 and older in 1970. The data have been weighted to compensate for variations in the sampling procedures at different stages of the survey process and to adjust for differential interview completion rates across various subpopulations. A technical discussion of the sampling, field and weighting procedures is provided by LoSciuto et al. (1971). The population to which our data are generalizable is the U.S. nonfarm and noninstitutionalized population, aged 21-64 in 1970.[8]

The sample sizes for all of the religion-race-gender subgroups are shown in the first column of Table 1. Other data presented in Table 1, as well as the analytic problems posed by the small "n's" obtained for black Protestants, Jews and "religious nones," are discussed in the text which follows.

We used three separate indicators of socioeconomic status (educational attainment, occupational status and income) and conducted the analysis separately for each of these. This was done for two reasons. First, we subscribe to the viewpoint that these variables are conceptually distinct and should be treated as such (Blau and Duncan, 1967; Hodge and Siegel, 1968). Second, we found that much of the previous research on this topic has utilized only one of these indicators; our use of all three permits comparisons with other studies. The imperfect relationships among these three indicators of socioeconomic status is well documented. The income indicator probably comes closest to the economic or "class" dimension, although it does not tap economic "wealth." Occupation, when appropriately scaled, generally is accepted as a measure of social prestige and thus can be expected to be positively, but not perfectly, correlated with the measure of the economic dimension. Education, also positively related with the prestige and economic dimensions, typically is regarded as a determinant of ultimately attained prestige and economic levels (Blau and Duncan, 1967). Thus, education can be viewed as the more distant "cause" of religious participation and may be expected to have less influence on that basis alone. Therefore, the prestige and economic dimensions may be accepted as the most appropriate indicators of socioeconomic position at the time of the interview.

We used the following indicators in this analysis. Educational attainment was measured as the number of years of schooling completed by the respondent. Income is the total family income of the household of the respondent for the year 1969. Occupational status of the household head is a four-category ordinal variable: (1) unskilled and semi-skilled; (2) skilled; (3) clerical and sales workers and (4) professional, business managers, officials and proprietors.

Religious participation was measured by asking the respondent to indicate the number of times he/she attended religious services in the month of January, 1970.[9] Nonreligious participation was measured by summing scores on five items used to measure particular types of social and political participation. The items are as follows: sign a petition; write a letter to a newspaper, a magazine or a political official; try to influence friends and neighbors about some issue; give an opinion at a public meeting; vote against a political candidate because of his stand on an issue. If the respondent reported participation, he received, for each activity, a code of 1; if not, he received a code of 0.[10]

Table 1 shows the means and standard deviations for religious participation and nonreligious participation for the religion-race-gender subgroups. Although the objective of this analysis centers on the SES-participation

[8] These restrictions are placed on the data primarily because our major independent variable, socioeconomic status, cannot be adequately measured for retired people (65 or older) and since the placement of farmers on a socioeconomic status hierarchy has proven to be a sticky problem. Evidence of this difficulty is furnished by comparing the placement of farmers in two of the most often used scales: the NORC prestige scale and the Duncan SEI.

[9] Frequency of attendance at religious services is determined to some extent by different normative prescriptions associated with religious organizations. It has been established that attendance is more frequent among Catholics than among Protestants and is least frequent among Jews (Gallup and Davies, 1972). Our use of religious subgroups may be viewed as a control for this effect.

[10] We recognize that our measure of nonreligious participation does not identify activity in a variety of other organized groups, such as athletic or union organizations. The measure used here is biased toward political participation.

Table 1. Means and Standard Deviations for Religious Participation and Nonreligious Participation by Religion, Race and Sex: Nonfarm Population, Aged 21-64 in 1970

Subgroup	n[a]	Religious Participation Mean	Religious Participation S.D.	Nonreligious Participation Mean	Nonreligious Participation S.D.
White Protestant	1229	2.14	3.10	2.05	1.55
Males	504	1.98	3.10	2.20	1.58
Females	725	2.29	3.10	1.90	1.51
White Catholic	459	2.95	2.36	1.97	1.53
Males	192	2.70	2.29	2.29	1.54
Females	267	3.18	2.41	1.66	1.46
White None	39	1.28	1.97	2.58	1.42
Males	22	1.29	2.00	2.40	1.47
Females	17	1.27	1.96	2.87	1.28
White Jewish	46	0.65	1.34	1.99	1.49
Males	16	0.31	0.80	2.33	1.29
Females	30	0.93	1.62	1.71	1.59
Black Protestant	148	2.24	2.32	1.44	1.42
Males	43	1.38	1.70	1.43	1.05
Females	105	2.68	2.47	1.45	1.58
Total	1921	2.28	2.87	1.99	1.54

[a]This refers to the unweighted number of respondents who reported on religious participation. When significance tests are made, the number of respondents who reported on both religious participation and the SES indicator being examined is used. These are: for education 1919, for income 1846 and for occupation 1698.

relationship and not on testing mean differences in participation across religious groups, these summary data nonetheless are informative. With regard to frequency of religious participation, the data generally show the predictable differences among the religious subgroups and between males and females: Catholics participate more frequently than black Protestants who participate more frequently than white Protestants who participate more frequently then Jews; there is more frequent religious participation by females in all but the "none" category.

The additional control variables for this analysis are gender, race, religious affiliation, marital status and the presence of young children. Religious affiliation is measured as the "religion raised in" and thus is an imperfect proxy for present religious affiliation.[11] Marital status is coded either as presently married or not presently married (never married, widowed, divorced or separated). "Presence of young children" measures whether there were children younger than age 16 living in the respondent's household at the time of the interview.

Our analysis primarily involves regression and correlation techniques. We view religious participation as the dependent variable with socioeconomic status (the analysis is conducted separately for each indicator) as the independent variable of major interest. The zero-order relationship between SES and religious participation is obtained by computing a regression or correlation coefficient between the two variables, whereas the SES-participation relationship with a control for nonreli-

[11]Our measure of affiliation is an admittedly imperfect indicator of current religious affiliation, although Featherman's (1971) research indicates that about 90% of males retain the early religious affiliation at the time of, or recently after marriage.

gious participation is obtained by including nonreligious participation as a second independent variable in the regression model; the partial regression coefficient for SES and religious participation indicates the net relationship after control for nonreligious participation.[12]

As indicated above, we also explored certain interactions implied by the relevant literature. This involves determining whether the SES-participation relationship varies across different subpopulations of interest. As will be described later, SES interactions with race-religious affiliation, gender, marital status and the presence of children are of interest. Multiple regression techniques also are used to test for these possible interactions (Cohen, 1968; Fennessey, 1968). More details are provided in the following text.

FINDINGS

The SES-Religious Participation Relationship

The data in Table 2 show, first of all, the zero-order relationship between socioeconomic status and religious participation for all five race-religion groups combined. The standardized regression coefficients (these are Pearson correlation coefficients in the zero-order case) indicate that the SES-participation relationship is weak for all three indicators. This is especially noteworthy since, as discussed above, our inability to control for church membership can be expected to increase the size of the coefficients (Dillingham, 1965; 1967). In addition, it is important to note that this weak relationship is not found to be the consequence of a nonlinear relationship.[13]

Table 2. Standarized Regression Coefficents for Religious Participation Regressed on the SES Indicators: Nonfarm Population, Aged 21-64 in 1970

SES Indicator	Zero-Order	Partial[a]
Education	.078	.041
Income	.044	.002
Occupation	.095	.074

[a]These represent the partial regression coefficients from a model which includes all three of the SES indicators.

Second, Table 2 shows the results of a single regression model that predicts religious participation from all three SES indicators. The standardized coefficients indicate that the net influence of income is zero and the net influence of occupational status and education is small.

Although the aggregated data in Table 2 suggest a very modest relationship between SES and religious participation, the relationship may be stronger or even negative in particular subpopulations. Our initial interaction tests are for differences in the relationship across subpopulations identified in Table 1, that is, tests for interactions with gender and race-religion.

The testing strategy adopted for these interactions is exploratory in design and is suggested by Cohen (1968). In general terms, it involves a number of tests in which two regression models are compared and the difference in explained sum of squares (R^2) is tested for statistical significance; in comparing any two models, the difference in the R^2s can be attributed to the independent influence of the variables (in this research, interaction terms) in one model but not in the other. The specific procedure is as follows. For each indicator of SES (education, income and occupation), several models are constructed. The first model includes all six independent variables, but without interactions. Thus, when education is the SES indicator, the

[12]We are actually utilizing the general linear model where we assume the dependent variable is measured at an interval level, but the independent variables may be interval or categoric. We have measured religious participation, education, income and nonreligious participation at an interval level. Our occupation measure of SES is an ordinal level variable which we will assume to be interval level for the analysis. Labovitz (1967; 1970) has argued and demonstrated that this is not a serious violation of assumptions but, in fact, should be done when one can assume a monotonic relationship between the measurement scale and the underlying scale. Race-religious affiliation, gender, marital status and presence of children are nominal scale variables and are coded as dummy variables.

[13]We conducted separate statistical significance tests for linearity (Kmenta, 1971:468-70) between religious participation and each of the interval level independent variables: education, income, occupational status and nonreligious participation. Each of these relationships was found to be linear.

model includes education, a gender dummy variable and four race-religion dummy variables. A second model specifies the first-order interactions with the SES indicator and is therefore identical to the first model except that it includes an interaction term for the SES indicator and gender as well as four interaction terms for the SES indicator with each of the race-religion dummies. A third model specifies the second-order interactions with the SES indicator and it is identical to the second model except that it includes four interaction terms for the SES indicator with gender by each of the race-religion dummies. These three models permit the appropriate comparisons for identifying various interactions. Thus, a comparison of the explained sum of squares (R^2) for the third model and the second model will indicate whether second-order interactions with the SES indicator are statistically significant, net of the additive effects of the SES indicator, gender and race-religion as well as the first-order interactions with the SES indicator. In addition, a similar comparison of the first model and the second model provides the overall test for the two first-order interactions—the SES indicator with gender and the SES indicator with race-religion. If this latter comparison indicates significant differences, the second model can then be compared with two additional models: (1) a model that incorporates the variables in the second model but which excludes the four SES indicators by race-religion interaction terms and (2) a model that incorporates the variables in the second model but which excludes the SES indicator-gender interaction term. The first comparison will indicate whether the SES indicator by race-religion interaction is significant, net of the additive influence of the SES indicator, gender, race-religion and the education-gender interaction; the second comparison will indicate whether the interaction of the SES indicator with gender is significant, net of the additive influence of the SES indicator, gender, race-religion and the first-order interactions of the SES indicator with race-religion.

These procedures produced the following results: for all three SES indicators, each analyzed separately, no significant second-order interactions with SES were found. Thus, it is unnecessary to interpret coefficients for all of the race-religion and sex subgroup combinations identified in Table 1. Accordingly, not all of these coefficients are displayed.

Tests for the first-order interactions indicated no significant interactions for education as an SES indicator; thus, the one coefficient (.078) adequately represents all gender-race-religion subgroups.[14] For both income and occupation, however, the relationship between SES and religious participation is significantly different for males and females. To facilitate interpretation, the coefficients for the effect of income and occupation by gender are displayed in the first column ("no control") of Table 3. For both of these SES indicators, the relationship is stronger for males, and since the second-order interaction with race-religion is not significant, we may conclude that this pattern exists across all five race-religion subgroups. In this regard, it should be noted that female respondents reported the occupation of the "household head" and income of the "family"; thus, unmarried females' reported occupation and income do not have the same referent as the corresponding reports of married males and females. In fact, unmarried females residing with relatives may be reporting on the family of orientation or some other kin group with respect to both occupation and income. When direct comparisons by marital status are made (as introduced later), education, which is unaffected by these difficulties, is utilized as the most appropriate SES indicator. For the present comparisons, however, the total number of unmarried females is sufficiently small, relative to the number of married females, so the observed patterns probably cannot be interpreted as artifactual.

The first-order interaction test for whether the SES-participation relationship differs by race-religion is significant only for the occupation indicator. The coefficients in Table 3 indicate that both the magnitude and sign of the relationship vary across these groups. The relationship is negative for white Jews and white "nones" although we are reluctant to attach much substantive significance to this in light of the sample sizes, 46 and 39, respect-

[14] Although we generally refer to standardized coefficients in the text, the unstandardized coefficients are displayed in the tables because they are more appropriate for making subpopulation comparisons (Blalock, 1968; Schoenberg, 1972).

Table 3. Regression Coefficients for the Influence of SES (the Three Indicators) on Religious Participation by Subgroups for which Significant Interactions Were Found: Nonfarm Respondents, Aged 21-64 in 1970

Dependent Variable and Subgroup	Regression Coefficients for Religious Participation Regressed on the Given Independent Variable[a]	
	No Control	Control for Nonreligious Participation
Education		
Total	.078 (.074)	.042 (.040)
Income[b]		
Males	.104 (.427)[c]	.073 (.301)
Females	-.006 (-.022)	-.037 (-.130)
Occupation[d]		
Males	.150 (.400)	.119 (.317)
Females	.058 (.140)	.028 (.067)
White Protestants	.136 (.354)	.107 (.279)
White Catholics	.004 (.008)	-.029 (-.059)
White None	-.155 (-.270)	-.204 (-.356)
White Jewish	-.320 (-.451)	-.328 (-.463)
Black Protestants	.155 (.371)	.146 (.349)

[a]The coefficients not in parentheses are standardized coefficients; those in parentheses are unstandardized coefficients.

[b]As described in the text, this income-sex interaction test was conducted as part of the systematic strategy for identifying significant interactions. It was tested net of the additive influence of income, gender, the four race-religion dummies and the first-order interactions of income with the race-religion dummies. The F-ratio was 5.15 with 1 and 1834 degrees of freedom.

[c]In order to save space, the unstandardized coefficients for income were multiplied by the constant 10,000.

[d]These interactions were also found to be significant by following the systematic testing procedure described in the text. The F-ratio for the occupation-gender interaction was 4.62 with 1 and 1686 degrees of freedom. The F-ratio for the occupation by race-religion interactions was 3.38 with 4 and 1686 degrees of freedom.

ively. For white Catholics the relationship is zero; only for black and white Protestants is the relationship positive.[15]

Control for General Participation

Following Goode's (1966; 1967a; 1967b) argument, we attempted to determine whether the SES-religious participation relationship can be interpreted as reflecting a more general association between SES and social participation. Our preliminary analysis (data not presented here) demonstrated that nonreligious participation is positively related to the three SES indicators, as has been demonstrated in past research; the relation between nonreligious participation and religious participation is, however, considerably weaker and in some subgroups negative in direction. Nevertheless, these patterns suggest that the modest relationship between SES and religious participation may be a function of the greater overall participation of persons at higher educational, income and occupational levels.

Table 3 shows two sets of standardized (and unstandardized) regression coefficients

[15]The same basic patterns by race-religious affiliation categories were observed for the income and education indicators, but the differences are not statistically significant.

for the relationship between each of the SES indicators and religious participation. The first set includes the zero-order coefficients for the subgroups discussed above. The second set, in the column labeled "control for nonreligious participation," includes the partial coefficients for these same subgroups with a control for nonreligious participation. For each subgroup the zero-order coefficient may be compared directly with the corresponding partial coefficient to determine the effect of controlling for general participation.

These comparisons suggest two effects of introducing the control. First, where the original coefficient is negative, the nonreligious control either leaves the original relationship unchanged or it increases the magnitude of the negative relationship[16]. Second, where the original coefficients are positive, the SES-religious participation relation is attenuated, but not eliminated. Indeed, the coefficient is reduced to less than one-half the original size by the introduction of the control in only one instance, among females for the occupation indicator. In general, the shifts in magnitude attributable to this control are not large and the data suggest that the modest SES-religious participation relationship, positive or negative, is not primarily a function of general social participation.

SES, Religious Participation, Marital Status and Children

Because of small sample sizes for three of the race-religion subgroups, the analysis here is restricted to white Protestants and Catholics. In addition, our analysis for interactions with marital status and presence of children is restricted to education as the SES indicator because of the difficulties discussed above concerning reported family income and occupation of household head for unmarried respondents.

Table 4 shows data which, although not directly relevant to our analytic objectives, describe relationships relevant to the family-surrogate hypothesis. The data show, first of all, higher average frequencies of religious participation among the married-with-children (2.69) and the unmarried-without-children (2.12) than among the married-childless (1.98) or the unmarried-with-children (1.75); a similar pattern is observed with regard to nonreligious participation. The importance of these descriptive data is that although the patterns are consistent with Argyle's (1959) summary of the literature as well as Nash's (1968) research, the data also are consistent with the Glock et al. (1967) contention about the "family surrogate" function of religious participation. Indeed, these data indicate that in a situation where the "family surrogate" function might be anticipated, i.e., among never married, widowed, divorced or separated persons with no young children, religious participation remains comparatively frequent.

Our analysis for interactions here followed the strategy described above. The only procedural difference is that the SES-participation relationship is examined over different subgroups generated from combinations of the following variables: religious affiliation (Protestant and Catholic), gender, marital status and presence of young children. All of these were coded as dichotomous variables, producing sixteen possible subpopulations for the relevant comparisons of the SES-participation relationship. As before, we estimated R^2 values for a series of multiple regression models which, when compared, permit the identification of significant interactions. We tested for the highest order interaction (fourth-order) first and successively examined third-, second- and first-order interactions. Results indicated that the fourth-order interaction is not statistically significant, and therefore it is not necessary to examine a sixteen cell table of coefficients. We also found no statistically significant third-order interactions. The overall test for second-order interactions is significant, however, and our analysis of these shows two of the six possible second-order interactions with education to be statistically significant: (1) education interacts with religious affiliation and marital status in its influence on religious participation and (2) education interacts with marital status and presence of children in its influence

[16] This increase in magnitude is a statistical necessity given the signs of the correlations involved. The substantive interpretation is that the negative zero-order relationship is smaller in magnitude (than the partial coefficient) because higher general participation in the upper strata is, indeed, operating to produce religious participation in these strata. Put another way, the control removes this influence which is actually operating counter to the dominant pattern for these subgroups.

Table 4. Means and Standard Deviations for Religious Participation and Nonreligious Participation by Marital Status and Presence of Young Children: White Protestants and Catholics, Nonfarm, Aged 21-64 in 1970

	n[a]	Religious Participation		Nonreligious Participation	
		Mean	S.D.	Mean	S.D.
Married					
Children	873	2.69	3.18	2.11	1.56
No Children	480	1.98	2.57	1.87	1.51
Not Married					
Children	85	1.75	2.34	1.75	1.47
No Children	224	2.12	2.73	2.21	1.51

[a]Because only those who reported religious participation, marital status and presence of children are included in this analysis, the total is somewhat less than indicated in Table 1. This sample size of 1662 is used in the F tests described in Tables 5 and 6.

on religious participation.

Table 5 displays regression coefficients for the religious affiliation by marital status subgroups and thus allows for interpretation of this interaction. The coefficients indicate that the relationship is weaker for Catholics than Protestants and that it varies within the two religious groups by marital status. It is because the pattern of coefficients for married and unmarried persons is different within religion categories that this second-order interaction is significant; for Protestants, the relationship is stronger for the married than for the unmarried, but for Catholics it is stronger for the unmarried than for the married. Table 5 also shows partial coefficients when nonreligious participation is controlled and the data indicate that interaction patterns are accentuated by this control.

Data pertaining to the other significant second-order interaction are presented in Table 6, where the SES-participation relationship is shown for marital status by presence of children categories. Although the relationship is weaker for childless persons, both married and unmarried, the largest difference occurs among married persons. In fact, the relationship is essentially zero among the married-

Table 5. Regression Coefficients for the Influence of SES (Education) on Religious Participation by Religion and Marital Status: White Protestants and Catholics, Nonfarm, Aged 21-64 in 1970[a]

Religion and Marital Status	No Control	Control for Nonreligious Participation
Protestant		
Married	.133 (.140)[b]	.099 (.104)
Not Married	.109 (.097)	.061 (.055)
Catholic		
Married	.018 (.016)	-.037 (-.033)
Not Married	.055 (.037)	.106 (.071)

[a]This second-order interaction was found to be significant following the systematic testing strategy described in the text. It was tested net of the additive influence of sex, education, religious affiliation, marital status and presence of children and the influence of the four first-order interactions with education and the influence of the other five second-order interactions with education. The F-ratio is 4.37 with 1 and 1646 degrees of freedom.

[b]The coefficients not in parentheses are standardized coefficients; those in parentheses are unstandardized coefficients.

Table 6. Regression Coefficients for the Influence of SES (Education) on Religious Participation by Marital Status and Presence of Children: White Protestants and Catholics, Nonfarm, Aged 21-64 in 1970[a]

Marital Status and Presence of Children	No Control	Control for Nonreligious Participation
Married		
Children	.158 (.184)[b]	.091 (.106)
No Children	.016 (.013)	.036 (.029)
Not Married		
Children	.121 (.114)	.081 (.076)
No Children	.074 (.063)	.070 (.059)

[a]This second-order interaction was found to be significant following the systematic testing strategy described in the text. It was tested net of the additive influence of sex, education, religious affiliation, marital status and presence of children and the influence of the four first-order interactions with education and the influence of the other five second-order interactions with education. The F-ratio is 3.85 with 1 and 1646 degrees of freedom.

[b]The coefficients not in parentheses are standardized coefficients; those in parentheses are unstandardized coefficients.

without- children and the relationship among the married-with-children is one of the strongest (.16) that we have observed for any subgroup. The control for nonreligious participation does not affect the ordering of these coefficients, although the differences within marital status categories generally are reduced.

The overall test for the first-order interactions with education is significant, although more detailed tests showed only one of the four possible tests to be significant: education interacts with presence of children in its influence on religious participation. However, since we have already found that the effect of this variable is contingent on marital status, this interaction probably should not be isolated from that context.

The Explanatory Importance of the Predictors

To this point, our analytic purpose has been to specify, by identifying interactions, the relationship between SES and religious participation. We have not attempted to isolate and attribute relative importance to the predictors of religious participation. This latter task deserves attention, however, since in this area of research it is also important to know whether the major social determinants of religious participation have been identified.

We have estimated several regression models to determine the proportion of the total sum of squares (R^2) in religious participation explained by the variables considered in this research (see Table 7). The data indicate that our three SES indicators together explain about one percent of the variance in religious participation. The addition of nonreligious participation increases the R^2 by about .5 percent. Adding religious affiliation and gender increases the explained variance to about five percent and finally, adding marital status and presence of children produces an increment of less than one percent.[17]

SUMMARY AND DISCUSSION

We analyzed these data to determine: (1) the strength of the relationship between socioeconomic status and religious participation; (2) if the relationship is merely a reflection of the socioeconomic status-general social participation relationship and (3) if the relationship varies significantly across the particu-

[17]The introduction of variables into the equation in this order is not predicated on a known or even assumed causal ordering. We acknowledge that different orderings would produce somewhat different estimates of the particular variable contributions, but the general inferences to be drawn remain unaffected. Also, since significant interactions were found, SES explains differential proportions of variation in religious participation across particular subpopulations. These differences may be examined by squaring the zero-order standardized coefficients in the appropriate tables.

Table 7. Predictors of Religious Participation: Explained Sum of Squares (R^2)[a]

Independent Variables	R^2	$\bar{R}^2$[b]
Education, Occupation and Income	.0104	.0089
Nonreligious Participation	.0150	.0129
Religious Affiliation and Sex	.0497	.0452
Marital Status and Presence of Children	.0580	.0526

[a]Each R^2 value represents the total sum of squares explained in religious participation by the corresponding variable(s) and all variables listed above it.

[b]These R^2 values have been corrected for degrees of freedom.

lar subpopulations identified as important in the literature.

With regard to the first objective, our findings indicate a weak positive relationship (for all three SES indicators) for the U.S. nonfarm population aged 21-64 in 1970. With regard to the second objective, we found that this relationship, although reduced in magnitude, is not entirely explainable by the positive relationship of our measure of general social participation with both SES and religious participation. Given our imperfect indicator of general social participation, we recognize that the adequacy of our test may be arguable. Thus, we do not insist that we have demonstrated the unimportance of general participation in explaining this relationship. However, given the major inference we will draw, this is not, we believe, a crucial unresolved issue.

The findings associated with the third objective probably are the most interesting and at the same time the most difficult to interpret. This interpretation difficulty is partly due to the exploratory character of this analysis. Nonetheless, the statistically significant interactions require some interpretation, however speculative.

As shown above, significant interactions were found in the relationship for race-religious affiliation and gender subgroups. For the occupational indicator, the relationship is positive but weak for the largest organized religious group (Protestants); it is essentially zero for Catholics and it is negative for two "minority" groups (Jews and unaffiliated whites).

Our interpretation of these patterns is that frequency of religious participation is, in part, a function of norms which are different or are differentially accepted by Protestants, Jews and unaffiliated whites from divergent SES levels. Thus, although religious participation varies by occupation level for these three subgroups, the relationship is positive for Protestants and negative for Jews and "nones." For Catholics, religious participation norms either do not differ or are not differentially followed at different SES levels. Furthermore, these patterns are not greatly affected by controlling for nonreligious participation and, therefore, we suggest the following qualifier concerning the SES-participation relationship: both the magnitude and direction of the relationship depends on religious affiliation. We also acknowledge, of course, that denominational differences within the broad Protestant category probably exist, although our data are insufficient for examining such differences.

Our analysis also indicated that, regardless of religious affiliation, the SES-participation relationship (for the income and occupation indicators) differs for males and females. Although mean religious participation is higher for females, the frequency of female participation does not covary with socioeconomic status to the degree that it does for males, who actually participate less frequently. Our interpretation of these patterns is that such differences probably are associated with differences in sex-role expectations and/or performance. In any case, we suggest still another qualifier to the general claim: the SES-participation relationship also varies by gender.

Our examination of interactions by marital status and presence of children was, for the reasons given, conducted only for white Protestants and Catholics and for the education indicator of SES. We found that our earlier conclusions about affiliation-specific norms or observance of norms required still further qualification. More highly educated, but unmarried, Catholics participate more often than less educated unmarried Catholics; among married Catholics, however, religious

participation does not vary by education level. For Protestants, the relationship is stronger for the married than unmarried subgroup. Our analysis of the effects of marital status led to the examination of the presence and absence of young children. In this regard, we found that, regardless of religious affiliation (Protestant or Catholic), the more highly educated married-with-children participate more than the less highly educated married-with-children. Among the married-without-children, there is no education-participation relationship. The same pattern (but with differences of smaller magnitude) was observed for the not-presently-married population. Our interpretation of these patterns is that the frequency of religious participation also is, in part, a function of norms which prescribe its importance in child rearing. These norms are, however, specific to levels of socioeconomic status. Thus, higher SES individuals, compared to lower SES persons, more commonly either subscribe to beliefs about the value of religious training for young children or conform to norms which define the correctness of such activity, or, of course, both. Implicit in this interpretation is that parental religious participation also involves the participation of children, although we have no data that bear on this.

Finally, we wish to anchor the observed interactions and our interpretation of them in the more general context of substantive importance. We examined a large number of subpopulations and the strongest positive relationship observed was .16. Thus, much of our analysis of these statistically significant interactions involved explaining differences among coefficients which ranged from zero to .16. Although several of the patterns are consistent with "theoretical sense," we are concerned that such reliance on statistical significance may blind analysts to the possibility of *substantive nonsignificance.* There are indeed differences across subpopulations which cannot be attributed to sampling error, but the substantive importance of these differences must be scrutinized. Our examination of the explained variance reinforces this concern. The combined influence of the three SES indicators was only one percent of the total variance in religious participation; the addition of nonreligious participation, race-religion, gender, marital status and presence of children increased the explained variance another five percent.

In sum, we interpret all of the findings reported above to indicate that socioeconomic status, although exhibiting somewhat different magnitudes of influence across subpopulations, is not an important determinant of religious participation and the considerable attention given it in the theoretical literature is perhaps unwarranted (at least in contemporary American society). We also conclude that certain other variables not accorded the same theoretical prominence or empirical attention are at least as important, if not the more important, determinants of religious participation. For purposes of subsequent research, the inference to be drawn is not that the study of religious participation be terminated. On the contrary, more research is needed, but it should be directed toward identifying other determinants of this variable; socioeconomic status is not a major determining factor.

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RACE AND SOCIOECONOMIC SEGREGATION*

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An investigation of the residential contiguity of socioeconomic status groups in the white and black population of the Chicago SMSA in 1970 shows that although segregation indices between socioeconomic groups were comparable for whites and blacks, residential propinquity between high status and low status persons differed dramatically between racial groups. Black professionals and managers lived in tracts with an occupational composition comparable, on the average, to that of tracts where unskilled white workers lived. The neighbors of white high school drop-outs had educational backgrounds similar to those of black college graduates. Black families with incomes over $25,000 lived in poorer tracts than white families with incomes below $3,000. In comparisons of whites and blacks on any variable affected by neighborhood composition, therefore, control for individual characteristics does not eliminate the effects of differential neighborhood characteristics.

In the introduction to Kantrowitz's book *Ethnic and Racial Segregation in the New York Metropolis,* Nathan Glazer writes:

> Are the poor and the rich blacks forced to live together, as I have written and believed? The evidence from this careful statistical study does not show this. On the contrary, the better off black is as separated from the poorer black residentially as the better off white is from the poorer white. . . (Kantrowitz, 1973:v).

Nathan Glazer need not have yielded so easily to statistical facts. Kantrowitz's study does not show that rich blacks are as separated from poor blacks as rich whites from poor whites, although he does show that rich blacks and rich whites are not distributed randomly throughout the New York metropolitan area.

In any measurement of segregation, there are two issues: (1) the deviation from the equal distribution of populations throughout a geographic area and (2) the residential contiguity and thus likely rate of contact between persons with unlike characteristics. These two concepts are not identical. Deviations from the equal geographic distribution of diverse populations is measured by the index of dissimilarity (Duncan and Duncan, 1955); the likely rate of contact is not.

The residential contiguity or probable rate of contact of diverse subpopulations within smaller areas of a city (e.g., within Census tracts) is contingent not only on the residential segregation of subpopulations, but also on their relative size in the total population. The larger the relative size of any given subpopulation, the greater the likelihood of encounters with members of it, given constant measures of segregation.[1]

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[1] Except where the index of dissimilarity reaches 100; in this case all subareas are completely homogeneous internally, and no intergroup contact within them is possible.

The following example of two hypothetical cities will illustrate this point. In each of these cities, the index of dissimilarity measuring segregation between the rich and the poor is 50. In one city, 50% of the people are poor; in the other, 10% are poor. Both cities have four subareas. The residential distribution of the population of these two cities is shown in Table 1.

Although residential segregation is identical (Table 1:a), if 50% of the population is poor in one but only 10% is poor in the other city, the distribution of rich and poor within tracts is quite different (Table 1:b).

Clearly, the likelihood of a rich person encountering a poor person within each tract is much greater in the city with 50% poor. Within each tract, the probability of a rich man encountering a poor man is simply the proportion of the tract population that is poor. These probabilities can be averaged for the city as a whole, weighting by the proportion of rich people living in each of the areas (from Table 1:a). This index of intergroup contact was proposed by Bell (1954) and discussed by Lieberson (1969) and Farley and Taeuber (1968) and will be called AIC (asymmetric intergroup contact):

$$AIC = \sum_{i=1}^{k} \frac{n_{ij}}{n_{.j}} \frac{n_{im}}{n_{i.}}$$

Table 1. Illustration of Index of Dissimilarity of 50 and Asymmetric Intergroup Contact of Rich and Poor in Two Hypothetical Cities (Percent)

a. Index of Dissimilarity

Subarea	City with 50% Poor		City with 10% Poor	
	Rich	Poor	Rich	Poor
A	10	40	10	40
B	30	20	30	20
C	50	10	50	10
D	10	30	10	30
Total	100	100	100	100
(N)	1,000	1,000	1,800	200
Index of Dissimilarity	50		50	

b. Asymmetric Intergroup Contact

	City with 50% Poor				City with 10% Poor			
	Rich	Poor	Total	(N)	Rich	Poor	Total	(N)
A	20	80	100	500	69	31	100	260
B	60	40	100	500	93	7	100	580
C	83	17	100	600	98	2	100	920
D	25	75	100	400	69	31	100	240
Encounter Probability of Rich with Poor	.36				.07			

where k is the number of subareas,
j refers to the interacting group,
m refers to the group interacted with,
$\frac{n_{ij}}{n_{.j}}$ is the proportion of the interacting group living in tract i, and
$\frac{n_{im}}{n_{i.}}$ is the proportion of the population in tract i belonging to group m.

In the city where 50% of the population is poor, the average rich person will encounter a poor person within his own tract with a probability of .36; in the city with 10% of the population poor, encounter probabilities are only .07. If the population had been randomly distributed throughout the tracts in each city (segregation of zero), encounter probabilities in the two cities would have been .5 and .1, respectively, with each subarea containing the same proportion of poor as the entire city. These probabilities are reduced by 28% by geographic segregation of rich and poor.

The index of dissimilarity allows intercity comparisons of the degree of racial segregation regardless of the percentage of residents who are black. This index is a measure of the degree to which race is a factor in the residential location of citizens. Because of its independence of marginals, it is a poor measure of likely intergroup contact, which is an important characteristic of a social system. Thus, an index measuring encounter probabilities may be more useful in some contexts than dissimilarity (D). Likely intergroup contact is a function both of the residential segregation of populations and their relative size in the total population.

Middle-class blacks are not randomly distributed throughout the ghetto and thus are segregated from the black lower class in this sense. Nevertheless, they live in much closer propinquity with the lower class than do middle-class whites, simply because the black lower class is larger than the white lower class. To the extent that neighborhoods are the functional locus of many institutions, most especially schools, this is of great consequence. The lack of residential segregation of social classes in the black community, in the sense of "contact probability" and the sharing of neighborhood facilities, may be one of the explanations for many of the differences between whites and blacks which remain after controlling for socioeconomic status. In particular, it may be one factor accounting for the low degree of occupational inheritance between high-status black fathers and their sons and the high degree of intergenerational downward mobility among blacks compared to whites (Blau and Duncan, 1967:209-11; Duncan et al., 1972:55-9; Rogoff, 1953:64-74).

THE DATA

In this paper, we shall examine segregation by socioeconomic status for whites and blacks in the Chicago SMSA in 1970. Although most blacks live within the city of Chicago itself, the SMSA was chosen for analysis because much of the socioeconomic segregation of whites is *between* city and suburbs and would be underestimated if computed within the city alone. The geographic subareas chosen for analysis were Census tracts, as socioeconomic data are not available for blocks. Three relevant measures of socioeconomic status were chosen: male occupation, education of adult males and family income; and the more detailed Census categories for each variable were reduced to a more manageable number.[2] The data were obtained from Census tapes for the Chicago SMSA (U.S. Bureau of the Census, 1972).

The index of dissimilarity was computed for all status pairs, for whites and blacks separately. In addition, the likely contact rate of each status group with each of the other status groups within tracts was computed. Unlike the Duncan index, this measure is asymmetric: probability of contact with all groups is computed separately for each group as the interacting group. Each such index is the weighted average percentage of each encountered group living in the same tracts as members of the interacting group. As was D, this measure was computed separately for whites and blacks, thus ignoring interracial contact between status groups. Because of

[2] In the case of education and income, the method of combining categories is apparent from the relevant tables. Occupational categories were combined as follows:

Unskilled manual: nonfarm laborers; farm laborers; private household workers
Semiskilled manual: operatives (transportation and other); service workers
Skilled manual: craftsmen and foremen
Clerical and Sales: clerical and sales workers
Professional and Managerial: professionals; managers and proprietors; farm managers

high racial segregation in the Chicago SMSA, the assumption of no interracial contact has only a very minor effect on measures of inter-status segregation and inter-status contacts (see footnote 7).

FINDINGS

Occupational Segregation

Occupational segregation in the Chicago area is not extremely high. Table 2 shows that the highest segregation, between the professional/managerial group and unskilled manual laborers for whites, is 40. As expected, segregation increases with status differences both for whites and for blacks. Occupational segregation is slightly less pronounced for blacks in the Chicago area than it is for whites.[3] Black professionals and managers are more segregated from black clerical and sales workers than is the case with their counterparts in the white community. However, for all other occupational pairs, the black segregation index is very close to or lower than the white segregation index.

The matrix of asymmetric intergroup contact in Table 3 shows, however, that this similarity hides different experiences for whites and blacks. White professionals and managers live in tracts that average 37% professional and managerial and 19% other white-collar. In these tracts, only 24% of the adult males, on the average, are semiskilled or unskilled. Black professionals and managers, on the other hand, live in residential areas where only 16% of the population are professional or managerial, only 15% are other white-collar workers and 53% are unskilled or semiskilled. It is apparent that a middle-class black person is much more likely to encounter in his neighborhood adults in lower manual occupations than a middle-class white person. In effect, the occupational environment of an unskilled white worker has a higher occupational status composition than that of a professional black: 23% of the adult male neighbors of an unskilled white worker are likely to be professional, compared to only 16% of the neighbors of a professional black; 37% of the male adults in the neighborhood of unskilled whites are unskilled or semiskilled but a majority (53%) of the male adult neighbors of the black professional hold unskilled or semiskilled occupations. The middle-class black child thus lives among a different array of adult male role models than the middle-class white child, despite the similarity in their own parental backgrounds.

Educational Segregation

Education, in the Chicago area, is a somewhat greater factor in segregation than occupation (Table 4).[4] For whites, segregation between college graduates and those with less than a 7th grade education is 57. As in the case of occupation, the black community in Chicago is slightly less segregated, as measured by the index of dissimilarity, than the white community. However, D's between educational groups for the two races do not differ greatly.

In spite of the similarity of segregation indices, the composition of neighborhoods in the Chicago area in which educated whites and blacks live differs considerably (Table 5). A white college-educated male lives in a tract where 46% of the males have at least some college education, but only 25% of the black males in tracts where black educated males live have any college education. Among the white neighbors of white college graduates, 29% have less than a high school education, compared to 49% of the neighbors of black college graduates. The educational composition of the neighborhood of the most highly educated black person is approximately comparable to that of a white high school drop-out.

Neighborhood composition largely determines school composition and, in the city of Chicago, the average Census tract size is less than twice the size of the average attendance area of elementary schools (855 Census tracts and over 500 elementary schools). The population composition of elementary school districts, therefore, should not be much more diverse than that of tracts.

The black middle-class child attending public school thus is at a disadvantage compared to the white middle-class child. The

[3] The unweighted average index of dissimilarity for all pairs of occupations is 26.5 for whites, 24.4 for blacks.

[4] Unweighted average D for education: whites, 31.5; blacks, 30.3.

Table 2. Indices of Dissimilarity between Occupations for Whites and Blacks, Chicago SMSA, 1970

Occupation	Occupation									
	Unskilled Manual		Semiskilled Manual		Skilled Manual		Clerical and Sales		Professional and Managerial	
	White	Black	White	Black	White	Black	White	Black	White	Black
Unskilled Manual	--	--	15	16	20	21	30	25	40	39
Semiskilled Manual			--	--	16	14	27	18	40	33
Skilled Manual					--	--	24	19	35	32
Clerical and Sales							--	--	18	27
Professional and Managerial									--	--

Table 3. Asymmetric Intergroup Contact between Occupational Groups, for Whites and Blacks, Chicago SMSA, 1970 (Percent)

Occupation of Contacted Group	Occupation of Contacting Group											
	Unskilled Manual		Semiskilled Manual		Skilled Manual		Clerical and Sales		Professional and Managerial		Proportion in Population	
	White	Black	White	Black	White	Black	White	Black	White	Black	White	Black
Unskilled Manual	7	14	6	11	6	11	5	11	4	9	6	11
Semiskilled Manual	30	49	30	50	27	48	23	47	20	44	25	49
Skilled Manual	24	15	24	15	25	18	21	15	19	15	22	16
Clerical and Sales	16	13	16	14	17	14	19	16	19	15	18	14
Professional and Managerial	23	8	23	9	25	9	31	10	37	16	29	10
Total	100	99	99	99	100	100	99	99	99	99	100	100

Table 4. Indices of Dissimilarity between Education Groups for Whites and Blacks, Chicago SMSA, 1970

Level of Education	Level of Education											
					High School				College			
	7th Grade or Less		8th Grade		1-3 Years		4 Years		1-3 Years		4 or More Years	
	White	Black	White	Black	White	Black	White	Black	White	Black	White	Black
7th Grade or Less	--	--										
8th Grade	19	16	--	--								
1-3 Years of High School	23	18	13	16	--	--						
4 Years of High School	33	25	22	22	16	17	--	--				
1-3 Years of College	42	39	34	34	30	31	19	23	--	--		
4 or More Years of College	57	53	51	48	49	46	40	39	24	18	--	--

Table 5. Asymmetric Intergroup Contact between Educational Groups for Whites and Blacks, Chicago SMSA, 1970 (Percent)

Level of Education of Contacted Group	Level of Education of Contacting Group													
					High School				College					
	7th Grade or Less		8th Grade		1-3 Years		4 Years		1-3 Years		4 or More Years		Proportion in Population	
	White	Black	White	Black	White	Black	White	Black	White	Black	White	Black	White	Black
7th Grade or Less	18	28	14	24	13	23	10	21	9	19	7	17	11	23
8th Grade	16	13	16	14	15	12	13	11	12	11	9	10	13	12
1-3 Years of High School	20	26	20	26	20	28	18	26	16	24	13	22	18	26
4 Years of High School	25	23	27	24	28	25	30	28	28	27	25	26	28	25
1-3 Years of College	10	7	11	8	12	8	13	9	15	13	16	13	13	9
4 or More Years of College	10	3	12	4	12	4	15	4	21	6	30	12	17	4
Total	99	100	100	100	100	100	99	99	101	100	100	100	100	99

contextual effect of school composition appears to be an important determinant of school achievement independent of the individual student's own socioeconomic background (Coleman, 1966; Jencks, 1972a; 1972b) and parental education would appear to be a crucial variable in shaping the educational environment of a school.[5]

Income Segregation

The income composition of a neighborhood determines the kind of commercial, recreational and cultural services available. In a consumption-oriented society, it will also affect a growing child's views of success and likelihood of achievement.

Income segregation in Chicago in 1970, as shown in Table 6, is quite similar to that in 1960 New York (Kantrowitz, 1973:39-40). As in New York, the index of dissimilarity is somewhat higher for blacks than for whites.[6]

The asymmetric encounter probabilities in Table 7 show, however, that high income blacks are much less isolated from low income blacks than high income whites are from low income whites. White families with incomes over $25,000 live in tracts where 19% of the families have similar incomes and an additional 31% have incomes between $15,000 and $25,000. Thus, half the white families surrounding "rich" whites have incomes over $15,000. In contrast, a black family with an income exceeding $25,000 lives in a tract where only 23% of the black families make over $15,000. White families in the highest income category share their residential neighborhoods with few poor families: only 8% of all families in such tracts have incomes less than $6,000. On the other hand, 24% of the neighbors of the wealthiest black families have incomes less than $6,000. These comparisons between white and black families are consistent throughout Table 7. The most dramatic illustration, however, of the vast difference in neighborhood composition for black and white middle-class families is provided by the comparison of the average tract for white families with incomes under $3,000, that is, under the official poverty level, and for black families with incomes over $25,000. Among the neighbors of the poorest whites, 7% are also below the poverty level compared to 10% of the neighbors of wealthy blacks. Among the neighbors of the lowest income whites, 33% have incomes under $9,000 and 67% have incomes over $9,000. The wealthiest black families live in areas where 44% of the families make less than $9,000 and only 56% make over $9,000. The average white family with income below $3,000 lives in a wealthier neighborhood than a black family with income over $25,000.

In the preceding analysis, the likelihood of inter-status contact *within* racial groups was computed. It could be argued that blacks in higher status groups are more likely to live in white neighborhoods, and the proportion of high status neighbors is underestimated for this group when analysis is restricted to within-race contacts. Racial segregation in Chicago SMSA, however, is so high that the inclusion of whites in the group encountered made very little difference. The income composition of tracts in which high income black families live is almost the same whether computed for black neighbors only or for black and white neighbors combined.[7]

DISCUSSION

These findings show clearly and consistently that the socioeconomic composition of neighborhoods in which white and black middle-class families live in the Chicago SMSA differs dramatically. For the black middle class, residential segregation by socioeconomic status means living in neighborhoods with an occupational, educational and income level above that of all blacks in the Chicago SMSA. It may mean avoidance of the inner ghetto and public housing. It does not mean isolation

[5] The extent to which school composition is determined by immediate neighborhood composition is, of course, dependent on the geographic range of student recruitment. Accessibility of private schools and criteria for establishing school boundaries are other factors which may affect school composition independent of neighborhood composition.

[6] Unweighted average D for income: whites, 29.6; blacks, 33.3.

[7] Contact rates between income groups differ by a maximum of 2% from those presented in Table 7 when the contacted group includes all races, instead of assuming intra-race contact only.

Table 6. Indices of Dissimilarity between Income Groups for Whites and Blacks, Chicago SMSA, 1970

Family Income	Family Income													
	$0-$2,999		$3,000-$5,999		$6,000-$8,999		$9,000-$11,999		$12,000-$14,999		$15,000-$24,999		$25,000 and over	
	White	Black	White	Black	White	Black	White	Black	White	Black	White	Black	White	Black
$0-$2,999	--	--												
$3,000-$5,999	16	15	--	--										
$6,000-$8,999	17	25	15	19	--	--								
$9,000-$11,999	25	35	23	30	16	17	--	--						
$12,000-$14,999	31	44	29	38	23	26	12	17	--	--				
$15,000-$24,999	36	49	35	44	30	32	21	23	14	17	--	--		
$25,000 and over	54	59	53	56	51	46	47	38	42	37	32	32	--	--

Table 7. Asymmetric Intergroup Contact between Income Groups for Whites and Blacks, Chicago, SMSA, 1970 (Percent)

Family Income of Contacted Group	Family Income of Contacting Group															
	$0-$2,999		$3,000-$5,999		$6,000-$8,999		$9,000-$11,999		$12,000-$14,999		$15,000-$24,999		$25,000 and over		Proportion in Population	
	White	Black	White	Black	White	Black	White	Black	White	Black	White	Black	White	Black	White	Black
$0-$2,999	7	20	6	18	6	15	5	13	4	11	4	10	3	10	5	15
$3,000-$5,999	10	23	11	24	9	20	8	18	7	16	7	15	5	14	8	20
$6,000-$8,999	16	22	16	23	16	24	14	22	13	21	12	21	9	20	13	22
$9,000-$11,999	21	15	21	16	21	17	22	19	21	19	19	19	15	19	20	17
$12,000-$14,999	17	9	17	10	18	11	19	13	20	15	19	15	16	14	19	12
$15,000-$24,999	23	8	23	9	24	11	26	13	27	15	30	18	31	17	27	12
$25,000 and over	6	1	6	1	6	2	6	2	7	2	10	2	19	6	8	2
Total	100	98	100	101	100	100	100	100	99	99	101	100	98	100	100	100

from the black lower class. Among blacks, middle- and upper-class people live in closer contiguity, on the average, with the lower class than do lower-class whites with their own class.

To the extent that neighborhood context affects individual behavior and attitudes independently of individual characteristics by circumscribing and channeling individual experiences, the implications of these findings for the study of race and social class are important. Many differences between whites and blacks have been discussed in the literature and few of them disappear when the socioeconomic status of individuals is held constant: victimization by crime (U.S. Department of Justice, 1974:25-6); participation in voluntary organizations (Orum, 1966; Olsen, 1970; Williams, 1973); school achievement (Coleman, 1966) and socioeconomic mobility (Blau and Duncan, 1967; Duncan et al., 1972; Rogoff, 1953). Differences in the socioeconomic composition of the immediate neighborhood in which blacks and whites of the same socioeconomic status live may account for some of these differences.

More generally, these findings highlight the problems of controlling for social class when comparing blacks and whites on any given characteristic. Control for individual social status does not, of course, eliminate the effects of differing socioeconomic neighborhood composition. Control for both neighborhood composition and individual social class in interracial comparisons would, in fact, be difficult to achieve since there are few blacks and whites comparable on both variables, at least in the Chicago SMSA. In the absence of such contextual analysis (Davis et al., 1961), the residual differences in individual characteristics between blacks and whites, after controlling for social class, may be due to the large difference in the socioeconomic distribution of whites and blacks. It is this difference—the presence of a much larger poor population in the black community than in the white community—that accounts for the differences in the socioeconomic composition of black and white neighborhoods at each socioeconomic level. In any event, care should be taken in attributing residual differences between blacks and whites—differences not eliminated by individual-level controls—to race or racial subculture.

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A STRUCTURAL CONTROL MODEL OF ORGANIZATIONAL CHANGE*

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This paper presents a differential equation model of organizational change which focuses upon the mechanisms of change. The model is obtained from positing the notion of structural control. The necessary procedures for estimating the model are developed and applied to data from a panel study of bureaucracies. The model addresses Blau's formal theory of differentiation and both modifies and extends that theory. The relationship between size variables and differentiation variables is more complex than Blau's theory suggests. Moreover, some mechanisms of change are more important than others and this relative importance is detected in the model.

Several theoretical perspectives have been proposed and methodological approaches employed to account for a number of empirical regularities found among the structural components of bureaucratic organizations. Blau's (1970) formal deductive theory of organizational structure is among the better known and more successful attempts to explain these empirical observations. Blau argues that the volume of services demanded of the public bureaucracies he studied determined the size of these organizations. He goes on to argue that size largely determines organizational structure, defined by the horizontal and vertical differentiation of an organization's units or components. Moreover, Blau argued that administrative overhead, the percentage of an organization's employees devoted to administration, decreases with increasing organizational size.

This line of argument has a developmental flavor, yet because Blau's analysis was based on a cross-sectional survey of organizations, the relationships among organizations' size, differentiation and administrative overhead must be viewed as statements of covariation rather than temporal development. It should be noted that Blau was very careful to make this distinction in his work. The core of Blau's theory rests on a causal ordering: Size → Differentiation. It can be argued that the reverse causal ordering (Differentiation → Size) may also be appropriate for studying the development of organizations. For example, administrators may decide to add a new service, thereby increasing horizontal differentiation. Subsequently, people are employed in the new division and the organization grows in size. Similarly, an increase in the hierarchy of the organization could promote growth through a phenomenon commonly known as Parkinson's Law. These alternative assumptions are not raised as criticisms of Blau's work even though either assumption about the causal ordering of the variables can be "supported" within the empirical context of Blau's study. Stated another way, cross-sectional covariational data cannot be used either to discriminate between the alternative ordering assumptions or

*Aspects of this work have been presented previously. The Coleman methodology was applied to Meyer's data in a paper by Teuter, July, 1973. The model in its current form was presented by Hummon at a Mathematical Social Science Board Conference at Arden House, Harriman, N.Y. in May, 1974.

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between hypotheses predicated upon these alternative assumptions.

In response to this dilemma, Meyer (1972a) conducted a study for which data were collected on a population of organizations at two points in time. Meyer's analysis is explicitly in the framework established by Blau with the modification that observations on the variables were naturally ordered by time. In regressing variables measured in 1971 on the same variables measured in 1966, Meyer found that size determined the development of structural differentiation. Thus, Blau's theory appeared to hold in a quasi-dynamic framework.

Administrative initiative still seems a plausible input for organizational growth and development. Also, historical studies of organizational development indicate that the process is more organic than suggested by Blau's theoretical argument (Chandler, 1962). Moreover, the methodologies employed by both Blau and Meyer tend to emphasize conclusions in terms of a particular ordering. The simultaneous incorporation of both causal assumptions does not lend itself to straightforward regression type models. For these reasons and others which will become apparent in the discussion below, we have constructed a model of organizational development that is more organic in its assumptions, is truly dynamic and can be empirically estimated and evaluated. It is a model concerned with the process of change and is not, therefore, a comparative statics model which focuses on comparing two equilibrium situations without inquiring into the actual trajectory of the process.

The Structural Control Perspective

In the previous section we outlined, in a condensed form, some of the history of organization theory and in doing so raised several issues which could be regarded as dilemmas. These dilemmas are both substantive and epistemological. One of these can be viewed as a dilemma as to what level of process is focused upon: should organization theorists start with micro-processes and aggregate them or should they focus on macro-processes and, so to speak, work down? A second dilemma has to do with the nature of causal assumptions a theorist is willing to make and the kind of "understanding" sought. Each of these can, and indeed have been argued at length. However, rather than continue the debate in these terms, we propose a different perspective which we call structural control.

It is conceivable to specify many structures for an organization. For example, if the units of the system are *chosen* to be individuals, the specification of structure could follow the work of sociometrists and lay out the relevant social networks which are operative in organizations. If functional work groups are chosen, the specification of structure might resemble an organization chart. A third specification of organization structure could be made at an even more macro-level and choose only the major elements of an organization, e.g., its size and differentiation.

At first sight, all that is involved here is the level of aggregation to be chosen. However, from a systems perspective these choices radically affect the type of system being studied and the nature of the process being modeled. That is, models and techniques appropriate at one level of structure may not be appropriate at another. Moreover, we suspect that the processes modeled at one level do not simply aggregate into processes operating at another level. More is involved than simple aggregation; the idea that there are emergent theoretical statements is taken seriously. Furthermore, the very nature of the concept of system prohibits study by disaggregation; units disaggregated from other units lose their systemic meanings. It is possible to analyze a system where one focuses on one unit *and* its relations with all other units and it may happen that knowledge generated at that level may be consistent with knowledge generated by another level of study.

The model presented here focuses at a macro-level of structure. We shall couple the idea of structure with that of control to create the notion of structural control. The essence of control is comparison; the current status of some entity is evaluated against a criterion which represents what the status of the entity should be, and the outcome of this evaluation is then used as an input for an attempt to modify the entity in an appropriate manner. Usually there is a system being controlled and a controller of that system. A production system is an obvious example where inputs,

outputs and the operation of the system are continually monitored. As a result of the monitoring, changes can be made by the controller of inputs or operation, or both, and these changes are made exogenously to the actual system.

In a structural control model, however, the locus of control is not in something external. It lies in the actual structure of the system. For organizational change models, we are suggesting that the change is controlled by the structure of the organization itself (defined on the macro-units). This requires elaboration. We need to state what is being compared to what in the control mechanism. In order to do this, we need to depart from the conventional usage of the term "organizational structure" by conceiving of a pair of organizational structures. One of these is the observed structure, defined for whatever macro-units and relations that have been chosen (which is very close to the conventional usage); and the other is a latent, or unobservable structure that corresponds to the observed structure. The correspondence is only in terms of the definitions employed in defining structure; that is, for example, if size and differentiation are used to define the observable structure then size and differentiation are also used to define the unobserved structure. The observed structure we shall call the *realized structure,* and the unobserved is called the *control structure.* The structural control process is the comparison of the realized structure with the control structure, and change is generated as an outcome of this comparison. We note, and expand upon this in the following, that the control structure need not be fixed in time. The use of a realized and a control structure means another extension of the term organizational structure. We need now to include in the meaning of that term the relation between the two structures.

At first glance, an assumption of structural control might seem wholly inappropriate for building a model of organizational development. The administrative process is often thought synonymous with organizational control, and administration certainly involves a high level of control including a consideration of alternative choices, a use of information and planning for the future. However, empirical research on formal organizations has revealed a number of fairly strong relationships among observed characteristics and observed components of organizations. This evidence suggests that while the set of possible actions taken or considered by administrators may be very extensive, certain actions are much more probable. Moreover, it might be argued that the reduction of the possible set to the probable set results from constraints imposed on the administrative process by the structural control process. Certain basic relationships between organizational components may exist that dictate (or constrain) major classes of administrative action. For example, an increase in the number of production employees generally requires an increase in the number of supervisory personnel. In general, a change in a unit's state engenders propagated effects on the rest of the units in the system. These are the macro-level relations which are captured by the notion of structural control. They are not the consequence of simply aggregating micro-level decisions and processes. There is clearly room for argument here, but those who would wish to argue that macro-phenomena are the outcome of aggregating micro-phenomena need to specify the aggregation rules. We choose to specify a macro-level process and analyze the system behavior (at that level) and also assess the consequences for the micro-processes of the operation of the macro-level process.

A Structural Control Model

At the core of our model lies an assumption that formal organizations, particularly bureaucracies, have structures which control the development and change of organizational components. Our first step in constructing the model is to define the variables that make up the state space of the organizational system. We are interested in the empirical realization of two pairs of variables in response to a comparison with a corresponding pair of variables in the control process. The first pair of observable variables represents the productive subsystem of an organization:[1]

[1] It is common practice in organizational research to use the variable size (=P+S) in an analysis of organizational structure. For reasons which will become apparent, we prefer to partition size into number of supervisors (S) and number supervised (P).

(1) P, the number of employees primarily performing the output tasks of the organization, e.g., the production employees;
(2) D, the number of divisions which functionally differentiate the work force.

The second pair of observable variables represents the administrative subsystem of the organization:

(3) S, the number of supervisory employees in the organization;
(4) L, the mean number of hierarchical levels over all divisions.

For each of the observable variables, we define unobservable counterparts, P^*, D^*, S^* and L^*. These unobservable variables represent what the values of the real system variables would be if the configuration of the organization were "properly" structured. With these two types of variables, a structural control process can now be formulated. Let x_i be any of the four observable variables and x_i^* be its structural control counterpart. Then

$$\Delta x_i = c_i \, \Delta t \, (x_i^* - x_i) \qquad (1)$$

where Δx_i is the "correction" value for x_i, c_i a parameter of the process and Δt an increment of time. The parameter c is dimensioned t^{-1} and is a measure of the sensitivity of a variable to structural control.[2] To complete the specification of the control process, it is necessary to state the relationships which determine the values of the structural control variables, x_i^*. These relationships are given by

$$\begin{aligned} P^* &= a_{12}D + a_{13}S + a_{14}L + b_1 \\ D^* &= a_{21}P + \phantom{a_{22}D} + a_{23}S + a_{24}L + b_2 \qquad (2) \\ S^* &= a_{31}P + a_{32}D \phantom{+ a_{33}S} + a_{34}L + b_3 \\ L^* &= a_{41}P + a_{42}D + a_{43}S \phantom{+ a_{44}L} + b_4 . \end{aligned}$$

These equations incorporate one of the important ideas mentioned above: the development of organizational structure is an organic process in that all components of an organization can influence the change of all other components. The first equation (for P^*) does not contain P on the right-hand side. This says that given the values for the other variables at a particular point in time, P^* should be consistent with those values. The control value P^* is not determined by the realized value P. The same argument also applies to the other variables.[3]

When Equations (2) are substituted into Equation (1) and terms are rearranged, we have

$$\begin{aligned} \frac{\Delta P}{\Delta t} &= -c_1 P + c_1 a_{12} D + c_1 a_{13} S \\ &\quad + c_1 a_{14} L + c_1 b_1 \\ \frac{\Delta D}{\Delta t} &= c_2 a_{21} P - c_2 D + c_2 a_{23} S \\ &\quad + c_2 a_{24} L + c_2 b_2 \qquad (3) \\ \frac{\Delta S}{\Delta t} &= c_3 a_{31} P + c_3 a_{32} D - c_3 S \\ &\quad + c_3 a_{34} L + c_3 b_3 \\ \frac{\Delta L}{\Delta t} &= c_4 a_{41} P + c_4 a_{42} D + c_4 a_{43} S \\ &\quad - c_4 L + c_4 b_4 . \end{aligned}$$

When the limit is taken, Equations (3) become a system of linear differential equations. This system can be expressed in matrix form. Let

$$X = \begin{bmatrix} P \\ D \\ S \\ L \end{bmatrix}, \quad Q = \begin{bmatrix} -c_1 & c_1 a_{12} & c_1 a_{13} & c_1 a_{14} \\ c_2 a_{21} & -c_2 & c_2 a_{23} & c_2 a_{24} \\ c_3 a_{31} & c_3 a_{32} & -c_3 & c_3 a_{34} \\ c_4 a_{41} & c_4 a_{42} & c_4 a_{43} & -c_4 \end{bmatrix}$$

and

$$r = \begin{bmatrix} c_1 b_1 \\ c_2 b_2 \\ c_3 b_3 \\ c_4 b_4 \end{bmatrix} .$$

[2] c is required to be greater than 0. If $x_i > x_i^*$, then the control process demands a decrease in x_i in the time increment Δt and if $x_i < x_i^*$, then the control process demands an increase in x_i in Δt.

[3] We note that with all variables included on the right-hand side of each equation, we would face an identification problem in that the c_i and a_{ij} could not be retrieved from estimates of Q (of Equation 4).

Then the differential equation system can be written

$$\dot{X} = QX + r \tag{4}$$

where $\dot{X}$ is the vector of rates of change of the individual variables with respect to time, e.g., $\frac{dX(t)}{dt}$. This has the same form as a model suggested by Coleman (1968). The solution of (4) is

$$X = e^{Qt} X_o + (e^{Qt} - I)\ Q^{-1} r \tag{5}$$

where X_o is the vector of initial conditions. We now define

$$\tilde{Q} = e^{Qt} \text{ and } \tilde{r} = (e^{Qt} - I)\ Q^{-1} r$$

for known t, and rewrite (5) as

$$X = \tilde{Q} X_o + \tilde{r}. \tag{6}$$

Equation (6) expresses the relation between the set of variables at time t as a linear function of the same set of variables at time $t = 0$. This is our basic estimating equation. The empirical values of $\tilde{Q}$ and $\tilde{r}$ can be estimated if observations are available for two points in time and the time lapse is known. Having estimated $\tilde{Q}$ and $\tilde{r}$, both Q and r can be computed (see Appendix), which in turn yields numerical values of c_i, a_{ij} and b_i, the parameters of the basic structural control equations of the system.[4]

When the parameters of the basic structural control equations have been estimated, it is possible to explore the dynamic behavior of the system. Of particular interest for systems of this form are the eigenvalues of Q. The eigenvalues, λ_i, are called the time constants of the system and their values are crucial for determining whether the system is stable, unstable or oscillatory. If the real part of any λ_i is positive, the system is unstable, that is, it will grow without bounds; if the λ_i have non-zero imaginary parts, the system is oscillatory; and if the λ_i have negative real parts, the system is stable.

To expand on these concepts, consider the identities $Q = V \Lambda V^{-1}$ and $e^{Qt} = V \Phi V^{-1}$ (given in the Appendix) where

$$\Phi = \begin{bmatrix} & 0 \\ 0 & e^{\lambda_i t} \end{bmatrix}.$$

Ignoring the non-homogenous part of (6),

$$X_t = V \Phi V^{-1} X_{t-\Delta t}.$$

We now define a new vector, Y, such that $Y = V^{-1} X$ where V is the non-singular matrix of the eigenvectors of Q.

Now, $X = VY$ and hence, $\dot{X} = V\dot{Y}$.
For the homogenous part of (4) $\dot{X} = QX$, and

so $V\dot{Y} = QX = QVY = V\Lambda V^{-1} VY = V\Lambda Y$.

Therefore,

$$\dot{Y} = \Lambda Y \tag{7}$$

and $Y_t = \Phi\ Y_{t-\Delta t}$.

The system represented by Equation (7) has been obtained from the system of Equation (4) through a (similarity) transformation of the state variables. In this sense, they are equivalent representations yet the former are "decoupled" because Φ is a diagonal matrix. Thus, it can be seen that if any λ_i is positive in its real part, the corresponding y_i grows exponentially. Moreover, because X is a linear function of Y, an exponentially increasing element in Φ will cause some, and in general all, elements in X to grow without bound. Such systems are unstable.

To summarize, we are primarily interested in two properties of the model: (1) its dynamic characteristics, with particular emphasis on stability properties and (2) the structural control functions. We turn now to explore the model in the context of the data of Meyer's (1972a) empirical study.

An Empirical Application of the Structural Control Model[5]

As we have already noted, the data requirements for the model are two sets of observations on the variables recorded at

[4] The diagonal elements of Q give estimates of c_i; hence, dividing the ith row elements of Q by c_i and dividing the ith element of r by c_i gives a_{ij} and b_i, respectively.

[5] Marshall W. Meyer kindly provided us with the data used in this empirical example. For a more detailed description of the data, confer Meyer (1972a; 1972b; 1968).

different (and known) points of time. The panel study reported by Meyer (1972a) on the size and structure of city, county and state departments of finance satisfies these requirements. Meyer collected data from 194 finance departments in both 1966 and 1971. The variable definitions for P, D, S and L are taken from Meyer's study, although Meyer, in accordance with general convention, employed size as a variable rather than the size of the productive component.

In applying Equation (6) to this empirical context, X is considered as a vector of dependent variables whose values correspond to the observations on P, D, S, L in 1971. X_o then is taken as a vector of independent variables whose values correspond to the observations in 1966. The ordinary least squares estimates derived from regressing X on X_o yield estimates of $\tilde{Q}$ and $\tilde{r}$. The ordinary least squares procedure was used because, as Meyer noted (1972a), the time lapse of five years is sufficiently long so that the problems of temporal auto-correlation in the residuals are minimal.[6] Problems of estimation were compounded by the presence of outliers in the data set. The results reported are for a subset of cases from which the outliers have been removed.[7] The regression coefficients, $\tilde{Q}$ and $\tilde{r}$, are reported in Table 1.

Using these values, the structural parameters and eigenvalues of the model can be computed. The dynamic properties of the system are described by Λ, where

$$\Lambda = \begin{bmatrix} -.028 & & & 0 \\ & -.311 & & \\ & & -.208 & \\ 0 & & & -.254 \end{bmatrix}.$$

[6] At first sight, a joint Generalized Least Squares procedure (Theil, 1971; Ch. 7) would be preferable. However, the conditions for this procedure to yield no improvement over OLS are satisfied by the form of the model (Theil, 1971:302).

[7] Outliers were selected for one of two reasons: (1) they appeared to constitute bad observations and (2) they could not be considered as members of a set of organizations governed by a coherent process. An example of the former was an organization whose supervisory personnel declined from 74 to 23 while divisions increased from 8 to 14. An example of the latter was an organization that shrank in number of productive employees from 269 to 59, whose supervisory personnel declined from 60 to 11 and whose divisions plummeted from 22 to 3.

It is immediately apparent that, as dynamic systems, these finance organizations are stable and non-oscillatory as no eigenvalue is positive in the real part. The second property is reassuring in evaluating the validity of the model; there is little, if any, evidence that the development of these formal organizations is oscillatory. We should note that one eigenvalue, while negative, is close to zero. While such a system is stable in the formal sense, it is less stable than one where the largest (negative) eigenvalue is further from zero. It is conceivable, then, that a small change in the parameterization of the process could render it unstable.

The numerical solutions for Q and r are presented below and we discuss them prior to the structural control parameters, c_i, a_{ij} and b_i.

$$Q = \begin{bmatrix} -.097 & .97 & .28 & 6.017 \\ -.0006 & -.24 & .011 & -.021 \\ .0259 & .60 & -.23 & .94 \\ .0007 & .037 & -.0016 & -.23 \end{bmatrix}$$

$$r = \begin{bmatrix} -23.64 \\ 1.28 \\ -4.92 \\ .57 \end{bmatrix}$$

Our model of organizational change is concerned with rates of change, and a first step for interpreting our results is to examine the signs of the coefficients in Q. For example, in asking how the various organizational components effect changes in the productive component (P), we see that divisions, supervisors and levels all have positive effects which are countered by the negative feedback loop of P on itself. A summary of all the signs of Q are represented in Figure 1. All the ways in which the components relate to changes in each of the other components are represented—which is precisely one of the features of a model which does not impose a specific direction of causality.

We can go on to inquire into the major mechanisms of change and control and we do this through an examination of the structural control parameters. The numerical values for c_i are: $c_1 = .097$; $c_2 = .244$; $c_3 = .230$; $c_4 = .230$. Recall that the c_i measure the sensitivity of system variables to structural control. Thus, we see that P, the number of

Table 1. Unstandardized Regression Estimates of $\tilde{Q}$ and $\tilde{r}^*$

	1971			
1966	P	D	S	L
Size of Productive Component (P)	.69	--	.064	.0015
Number of Divisions (D)	1.43	.32	1.28	.060
Size of Supervisory Component (S)	.71	.017	.38	--
Mean Number of Levels (L)	15.40	--	2.33	.33
Intercept	-65.54	3.46	-10.36	1.90
R^2	.72	.23	.72	.25
F**	111.4	26.78	109.2	18.83

* The criterion for including variables in the regression was whether or not the t-ratio was greater than 1.0, as t>1.0 entails that the corresponding variable when included will raise the corrected coefficients of determination, $\bar{R}^2$ (see Haitovsky, 1969). A partial joint F-test indicated that it was appropriate to drop those variables for which no coefficient is reported.

**If N is the number of cases and k the number of regressors then the degrees of freedom for F are (N-k-1, k). After outlier removal, N = 178 and k is given by the number of reported regression coefficients in each column of the table.

production employees, is about 2.5 times less sensitive to structural control than the other variables.

The numerical values of a_{ij} and b_i are given in the basic structural control equations for the finance departments:

$$P^* = -10.00\,D - 2.49\,S - 62.18\,L + 244.27$$

$$D^* = .0024\,P - .044\,S + .088\,L - 5.25$$

$$S^* = -.11\,P - 2.60\,D \quad 4.08\,L + 21.40$$

$$L^* = -.0032\,P - .16\,D + .0068\,S - 2.48.$$

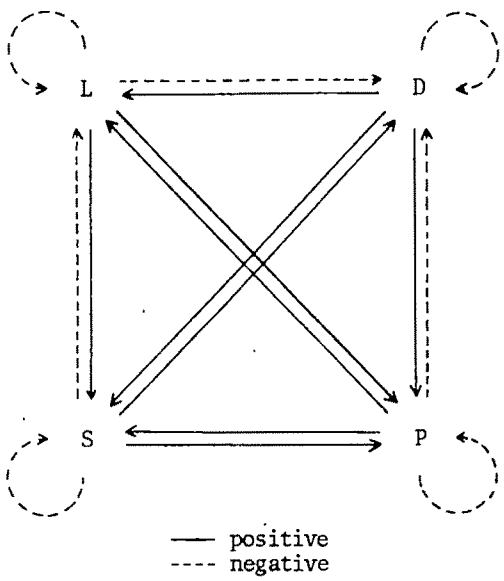

Figure 1. Schematic Summary of the Directions of Impacts on Growth of Organizational Components

These equations could, in principle, be used to generate values of X* (for each component of each organization). In this sense, there is a realization also of the "unobservable" variables, X*. The model is *not* saying that organizations evolve to a fixed "optimal" structure; rather, it says that the process of structural control is fixed in the short term. The actual (realized) structure of an organization places constraints on that which is realizable. The model is truly dynamic in that the control structural configuration has a time trajectory. This is due to the fact that it is contingent, in part,

Table 2. Basic Organizational Data (N = 178)

Variable		Mean	Standard Deviation	Median	2nd Quartile Range
Size of Productive Component (P)	1966	71.8	96.3	42.5	18.5
	1971	82.8	109.3	46	18
Number of Divisions (D)	1966	5.75	2.66	5.5	1.5
	1971	5.61	2.19	5	1
Size of Supervisory Component (S)	1966	19.7	21.1	12.5	2.5
	1971	18.2	19.8	12	5
Mean Number of Levels (L)	1966	3.92	.802	3.8	.5
	1971	3.65	.822	3.5	.3

on the observable structural configuration. The stability properties obtained through an analysis of the observable components carries over to the control configuration. This is to be expected as the control process is "hard wired" into the organization. It also follows, formally, from the fact that $X^* = AX + b$ where A is a non-singular matrix. We already have that

$\dot{X} = QX + r$ and now $A^{-1} X^* = X + A^{-1}b$ gives the transformation from X^* to X. We require a differential equation representation for the X^*. Hence, $A^{-1} \dot{X}^* = Q (A^{-1} X^* - A^{-1}b) + r$ or, $\dot{X}^* = AQA^{-1} X^* + A(r - QA^{-1}b)$, which by writing $M = AQA^{-1}$ and $s = A(r - QA^{-1}b)$ can be expressed as $\dot{X}^* = MX^* + s$. This is a differential equation model of the state space defined by the variables of X^*. The similarity transformation given by A preserves the eigenvalue structure of Q. Indeed, this eigenvalue structure is an important invariant property of such linear systems.

To facilitate further interpretation of these equations, we have computed the change in the time derivatives of the observable variable due to a "standardized" change in an observable variable. It is common to use the standard deviation of a random variable in such comparative calculations. Yet, because the distributions of two of the variables are skewed (see Table 2), we use an alternative standardization unit, namely, the range of the second quartile of the distribution of each variable. Table 3 presents these results.

A comparison of the relative magnitudes in each row of Table 3 yields the following conclusions:

(1) Inertial (diagonal) effects are important for all variables.
(2) These negative inertial effects are the most important effects for the two differentiation variables.
(3) Both differentiation variables, D and L, have important positive effects on

Table 3. Change in Time Derivatives of Variables Due to a 2nd-Quartile-Range Change in Observable Variables

Rate of Change of the	Observable Variable			
	P	D	S	L
Size of Productive Component (P)	-1.81	1.46	.71	3.05
Number of Divisions (D)	-.011	-.36	.027	-.011
Size of Supervisory Component (S)	.48	.90	-.58	.47
Mean Number of Levels (L)	.014	.056	-.004	-.12

the rates of change of both size variables, P and S.

(4) The number of divisions, D, has an important positive effect on the rate of change in the number of levels, L.

(5) The number of productive employees, P, has an important positive effect on the rate of change in the number of supervisory employees, S.

These major mechanisms of change are summarized in Figure 2, where the less important mechanisms of Figure 1 have been omitted. When the inter-relationships between the variables are considered in Figure 2, one feature is immediately apparent. The pattern of the effects on the rates of change of the variables is recursive: changes in the number of divisions have an important effect upon the rate of change of all other variables; changes in the number of levels have an important effect upon the rate of change of both size variables; changes in the size of the productive component have an important effect on the rate of change of the number of supervisory employees. However, each of these effects is strongly dampened by the direct negative feedback of each variable on its corresponding rate of change.

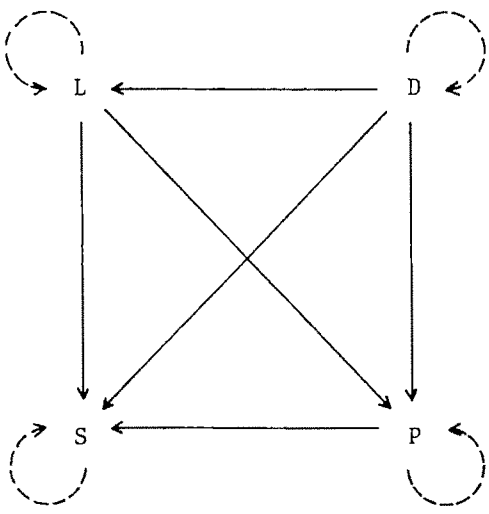

Figure 2. Summary of Major Structural Control Mechanisms

Blau's Theory Reconsidered

Our discussion thus far has focused on a set of results evolving naturally from an examination of a dynamic model. However, we began with a discussion of Blau's work where he was concerned with the relationships among variables at a point in time and so the question arises: does the dynamic model extend or modify the relationships in Blau's theory?

We observe that had we been following Blau more closely, we would have used the variable set (Z, D, S, L) rather than the set (P, D, S, L), where Z refers to organizational size (P+S). It is necessary, therefore, to consider the consequences of this change—which we do by regarding the change in terms of a linear transformation. Let our basic set of state variables be $X' = [P, D, S, L]$ and we will regard the other set as a linear transformation of this. The four variable set with variable Z replacing P can be obtained from $W = GX$, where

$$G = \begin{bmatrix} 1 & 0 & 1 & 0 \\ 0 & 1 & 0 & 0 \\ 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & 1 \end{bmatrix}$$

If $W = GX$, then $X = G^{-1}W$. The basic structural control equation system can then be expressed as

$$\dot{W} = GQG^{-1}W + Gr$$
$$= HW + Gr, \text{ with } H = GQG^{-1}.$$

This will be the structural control equation system for a state space defined in terms of W. The matrix H is readily obtained from the estimated Q and the defined G, as G is non-singular. The actual estimated values of H can be interpreted and incorporated into a theoretical argument in exactly the same way as the estimated values of Q. It is unnecessary to perform separate stability analyses for the state space defined by W as H is obtained from Q via a similarity transformation. When H was computed, its sign pattern was identical to Q's and an analysis in the transformed state space revealed the same major mechanisms.

Blau's first proposition states that "increasing size generates structural differentiation in organizations along various dimensions at decelerating rates" (1970:204). This proposi-

tion can be interpreted as a statement about the signs in the first and second derivatives of the structural differentiation variables with respect to size, namely, the size coefficients in the first derivative being positive and in the second derivative being negative. In the context of the dynamic model, we are interested in the contributions of the components of size (P and S) to the time derivatives of D and L. From Figure 1 we note that P has a positive effect on $\dot{L}$, and S has a positive effect on $\dot{D}$–relationships that are consistent with the first part of Blau's proposition. Yet, we also note that P has a negative effect on $\dot{D}$, and S has a negative effect on $\dot{L}$, findings that appear to be at odds with Blau's theory. However, we are dealing with the consequences of different components of organizational size and, in this sense, we have a refinement of Blau's theory. The positive effects are relatively stronger than the negative effects, so focusing on these alone the model and Blau's theory are not inconsistent. However, according to Figure 2, none of these are among the major change mechanisms in the dynamic model. While size does generate differentiation, increasing differentiation also generates increasing size and these are more important in the dynamics of bureaucracies.

The second part of Blau's proposition, "at decelerating rates," is more interesting and, moreover, is naturally part of a dynamic model. The second time derivative of Equation (4) is

$\ddot{X} = Q\dot{X}$ and substituting for $\dot{X}$ yields

$$\ddot{X} = Q^2 X + Q r, \qquad (8)$$

an equation which describes the acceleration of the system. When Q^2 is computed, we have the following pattern of signs:

	P	D	S	L
$\ddot{P}$	+	+	-	-
$\ddot{D}$	+	+	-	+
$\ddot{S}$	-	-	+	-
$\ddot{L}$	-	-	+	+.

When the pattern of signs for Q^2 is compared with the pattern for Q, it is immediately observed that the pattern for Q^2 is exactly the reverse of the pattern for Q with one exception (which refers to the effect of D on $\ddot{P}$). This means that all but one of the statements about the first time derivatives of the system apply to statements about the second time derivatives of the system, *except* with reversed signs; and therefore, if size generates structural differentiation, the model states that these growth rates are decelerating. Thus, Blau's decelerating proposition for size on differentiation is also obtained from the dynamic model.

While accepting Blau's first proposition, we would propose that increasing structural differentiation in organizations generates increasing size of productive and administrative components. In general, these too will be at decelerating rates. The one exception is indeed, quantitatively, a minor one.

These observations already incorporate Blau's second major proposition: "Structural differentiation in organizations enlarges the administrative component" (1970:213). An examination of Figure 2 indicates that both differentiation variables have strong positive effects on the rates of change of *both* size variables. Structural differentiation does indeed enlarge the administrative component, but it also enlarges the productive component. The dynamic model supports, but again subsumes Blau's proposition.

In responding to one final proposition of Blau, namely, that "organizations exhibit an economy of scale in management" (1970:210), we shall discuss the through time variation of the administrative ratio, recognizing that this is not what is meant by economy of scale in management.

Let the administrative ratio be defined as $A = S/P$. Then, $\dot{A} = \frac{P\dot{S} - S\dot{P}}{P^2}$ and the sign of $\dot{A}$ is given by $P\dot{S} - S\dot{P}$. The proposition states that $\dot{A}$ should be negative and we consider the conditions under which this holds. It is reasonable to assume that the size of the productive component is greater than the size of the administrative component, i.e., $P > S$ and, of course, they are both positive. From the above argument:

$\dot{A} < 0$ if and only if $P\dot{S} - S\dot{P} < 0$. That is, if and only if $P\dot{S} < S\dot{P}$, or if and only if $\frac{\dot{S}}{S} < \frac{\dot{P}}{P}$.

Now if $\dot{S}$ is negative and $\dot{P}$ is positive, the conditions are satisfied automatically and

trivially. If $\dot{S}$ is positive and $\dot{P}$ is negative, the conditions are not satisfied and $\dot{A}$ will be greater than zero, but this is trivial also. We consider now the cases where both $\dot{S}$ and $\dot{P}$ are positive; that is, both size components are growing or those instances where both $\dot{S}$ and $\dot{P}$ are negative, that is, both components are shrinking. It is not possible to establish that the necessary and sufficient inequality holds without additional empirical evidence. Using the estimated values for the parameters of the model, we computed $\dot{P}/P$ and $\dot{S}/S$ over a 20-year period and examined those cases where $P > S$ and $\dot{P}$ and $\dot{S}$ have the same sign (with $\dot{P}$ and $\dot{S}$ also being computed). For the simulation, we took as initial conditions the mean values of each of the size and differentiation variables. The results are consistent with Blau's proposition.

Concluding Remarks

In this paper, we have asked the question of whether the dynamic model allowed us to extend or modify the statements of Blau's theory. It is clear that the answer to this question is yes, but we would not argue that this is the only possible way to do this. Our explicit intention was to model a particular social process. That is, given an observation that organizations do change through time, is it reasonable to assume that this change is coherent and not random or arbitrary? A process is at work, and our conjecture is that an appropriate process for modeling this change is one of structural control. Given such a formulation, we were able to reach a statement of a simultaneous linear differential equation model which described organizational change. The outcome of this analysis was interpretable and permitted an explanation of certain empirically observed relationships. These interpretations can be viewed as alternative explanations of organizational change. The conclusions and explanations which followed from this model permitted a natural integration with the extant theory. Clearly, the task is not complete[8] although we would argue that a structural control process model provides a sound basis for future research into the dynamics of organizational change.

[8]One large statistical problem must be on the agenda we suggest. The estimated matrices Q and r are obtained from Q and $\tilde{r}$ through rather complex non-linear transformations. We suspect there are serious distributional problems with such transformation based estimates. To the extent that this modeling approach is viable, as we believe it to be, this presents a serious statistical challenge. Alas, it is one we were unable to tackle—yet.

APPENDIX

Estimation of Q and r

Suppose Q has distinct eigenvalues, then there exists a matrix V such that $V^{-1} Q V$ is the diagonal matrix Λ with the eigenvalues of Q in the main diagonal. In fact, V is a matrix whose columns are the eigenvectors of Q. Thus,

$$Q = V\Lambda V^{-1}$$

which, on multiplication by t, becomes

$$Qt = V[\lambda_i t]V^{-1}.$$

By exponentiation, we have

$$e^{Qt} = V\,[e^{\lambda_i t}]\,V^{-1}$$

Consider $\tilde{Q}$. By a similar argument,

$$\tilde{Q} = \tilde{V}\,[\tilde{\lambda}_i]\,\tilde{V}^{-1}$$

where $\tilde{\lambda}_i$ are the eigenvalues of $\tilde{Q}$, and $\tilde{V}$ the matrix of eigenvectors of $\tilde{Q}$. Now, $\tilde{Q} = e^{Qt}$ and so $V = \tilde{V}$. Hence, $\tilde{\lambda}_i = e^{\lambda_i t}$ $(1 \leqslant i \leqslant 4)$ or, equivalently, $\lambda_i t = \ln(\tilde{\lambda}_i)$. This gives $\lambda_i = \ln(\tilde{\lambda}_i)/t$ which is used as an estimation equation for the eigenvalues of Q. Thus, given estimated values of $\tilde{Q}$ and $\tilde{r}$, the numerical values of Q and r can be computed from the following relations:

$$Q = V \begin{bmatrix} & & 0 \\ & \ln(\tilde{\lambda}_i)/t & \\ 0 & & \end{bmatrix} V^{-1}$$

$$r = Q(\tilde{Q} - I)^{-1}\,\tilde{r}\,.$$

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COMMENTS

PRODUCTIVITY AMONG SCIENTISTS: A REPLICATION AND ELABORATION* (COMMENT ON ALLISON AND STEWART, ASR AUGUST, 1974)

In a recent article, Allison and Stewart (1974) try to determine whether accumulative advantage—also called "Matthew effect"—has an important role as a determinant of productivity among scientists. Productivity was assessed by obtaining citation counts and self-reports on number of publications for a sample of 1,947 biologists, mathematicians, chemists and physicists. Using a mathematical formulation developed by Simon (1957), Spilerman (1970) and others, Allison and Stewart (1974:598) arrive at the premise that, if accumulative advantage does exist, "the variances of productivity, esteem, and resources will all increase over time. . . ." The highly skewed character of the two measures of productivity necessitates the use of the Gini index as a measure of variability, and when this index is correlated with number of years since receiving the Ph.D. for eight cohorts of scientists (data are aggregated and N=8), the results tend to support the hypothesis of increased variability in productivity with increasing age. It should be noted in passing that since Allison and Stewart have cross-sectional data it is impossible to vary age without varying cohort membership, and vice versa, so that the effects of these two variables cannot be assessed independently.

Allison and Stewart state early in their article that the accumulative advantage hypothesis envisions the possibility of a reciprocal causal process in which productivity at time t might be influenced by access to resources at time t and by "esteem" at time t-1; access to resources at time t might be influenced by esteem at time t-1; esteem at time t-1 by productivity at time t-2, etc. If such a process operates, then at any point in time we would expect productivity to be associated with access to resources, and the covariance of these two variables presumably would increase as a cohort ages; Allison and Stewart find just such associations. Using path analysis, they find also that variability in access to research resources has a substantial impact on the Gini index for productivity, and this result is adduced as further support for the accumulative advantage hypothesis. Again in passing, it should be noted that the equations defining a possible reciprocal causation process seem to be applicable to individual scientists, whereas the analysis conducted throughout most of the article applies to age strata, and this anomaly would seem to create a danger of the ecological fallacy. The authors reveal an appropriate skepticism regarding their own tentative conclusions when they say that the "evidence in support of the accumulative advantage hypothesis does not disconfirm the heterogeneity hypothesis" (Allison and Stewart, 1974:605). The same difficulty was encountered by Spilerman using a similar approach to the study of urban riots, and his preference for the heterogeneity hypothesis was based on extraneous considerations (Spilerman, 1970:637) having nothing to do with the increased variance argument and made possible only by the fact that he had longitudinal data.

Using data from the 1973 American Council on Education survey of American college and university faculty members,[1] I

*The 1972-73 faculty data were collected by the American Council on Education's former Office of Research under a grant from the RANN Division of the National Science Foundation. Data access was achieved through the Council's Division of Educational Statistics, Washington, D.C. I wish to thank the Computer Center of the College of William and Mary for the use of data processing facilities.

[1] The ACE survey involved random sampling of faculty members within a disproportionately stratified cluster sample, where each cluster consisted of an individual institution. The final sample size was N=53029. The answers of each respondent must be weighted according to the sampling ratio used for his particular type of institution. Statistical data for any table can be taken as representative of the entire

Table 1. Publication* Inequality by Length of Time Since Ph.D.** and Field, U.S. College and University Faculty Members, 1973

	Variance for Publications			
Length of Time Since Ph.D.	Social Sciences	Humanities	Biological Sciences	Physical Sciences
1	.595	.541	.812	.565
2	1.586	.872	1.109	1.659
3	1.101	.834	1.854	1.585
4	.722	.962	1.651	1.616
5	1.153	.902	1.853	1.831
6	1.217	1.478	1.500	1.456
7	.820	.890	2.366	2.425
8	.466	1.031	1.896	1.211
	Regression of Variance on Time Since Ph.D.			
b	-.047	.064	.154	.097
a	1.169	.649	.933	1.105
r	-.309	.602	.776	.451
(Weighted) N =	7847	14675	4684	5006

*Questionnaire item: How many of your professional writings have been published or accepted for publication in the last two years? None = 1; 1-2 = 2; 3-4 = 3; 5-10 = 4; more than 10 = 5.

**Questionnaire item: Year of highest degree now held
1971 through 1973 = 1; 1968 through 1970 = 2;
1964 through 1967 = 3; 1960 through 1963 = 4;
1955 through 1959 = 5; 1950 through 1954 = 6;
1940 through 1949 = 7; 1939 or earlier = 8.

(Categories based on original ACE questionnaire.)

correlated career age with the variance in recent scholarly productivity for a large sample of faculty members in the social sciences, humanities, biological sciences and physical sciences. As shown in Table 1, the correlations for the four groupings were–.31, .60, .78, and .45, respectively. These results are generally supportive of Allison and Stewart's conclusions, although I am at a loss to explain the negative correlation for social scientists and the positive correlation for humanities professors, the only group of non-scientists in the sample. On the assumption that accumulative advantage would be more pronounced among research-oriented scholars, separate correlations were obtained for professors at "research universities" or "doctoral-granting universities,"[2] and the results were somewhat more commensurable with those of Allison and Stewart (Table 2): .47 for social scientists; .42 for humanities professors; .02 for biologists and .68 for physical scientists. Allison and Stewart found also that biologists did not entirely conform to the increased variance model.

It is easy to concur with Allison and Stewart's contention that merely presenting evidence of increasing variability in productivity with career age does not clinch the argument on accumulative advantage. At best, it can be said that neither the Allison-Stewart study nor the present study has succeeded in *eliminating* the accumulative advantage hypothesis. In fact, I have strong misgivings about the assumption that an accumulative advantage process would necessarily bring about

national population of faculty members. Tables 1 and 2, however, are based on a fifteen percent random sample from the larger file.

[2] "Research universities" were among the 100 leading universities in terms of federal financial support and awarded at least 50 Ph.D.'s in 1969-70, or were among the leading 50 institutions in terms of total number of Ph.D.'s awarded between 1960 and 1970. "Doctoral-granting universities" awarded at least ten Ph.D.'s in 1969-70.

Table 2. Publication* Inequality by Length of Time Since Ph.D.** and Field, U.S. Faculty Members Affiliated with Research Universities and Doctoral-Granting Universities, 1973

	Variance for Publications			
Length of Time Since Ph.D.	Social Sciences	Humanities	Biological Sciences	Physical Sciences
1	.379	1.081	1.093	--
2	1.226	1.273	1.627	1.676
3	1.207	1.011	1.636	1.265
4	.826	1.621	1.008	1.355
5	1.054	1.307	1.959	1.845
6	1.390	2.024	1.338	1.349
7	1.355	.958	1.725	1.929
8	.937	1.685	1.047	2.714
	Regression of Variance on Time Since Ph.D.			
b	.063	.064	.002	.158
a	.761	1.081	1.417	.941
r	.466	.417	.018	.678
(Weighted) N =	3309	5182	2615	2316

*See note to Table 1.
**See note to Table 1.

increased variability in productivity. Spilerman's model, based on a negative binominal distribution, assumes that the occurrence of a race riot necessarily would have to increase the probability of a subsequent riot for us to conclude that a reinforcement process has taken place (Spilerman, 1970:636). If δ represents "disorder proneness" at time t, then $\delta + \mu$ represents the probability of a riot at time t + 1 (following the occurrence of a first riot) and the probability of, say, a tenth riot is $\delta + 9\mu$. Such a process would place many cities on an upward incline in the number of riots experienced over successive time intervals, and the variability in number of riots among cities would necessarily increase. However, *a positive reinforcement process could operate within given cities from time t to t + 1 without producing a greater probability of a riot at time t + 1 than at time t.* Similarly, accumulative advantage could influence scientific productivity without producing an increasingly greater probability of publication with each succeeding publication or for each succeeding time interval. The point can be made by means of a least squares approach incorporating "reciprocal causation" equations similar to those introduced by Allison and Stewart (1974:598). To simplify matters without sacrificing plausibility, we shall assume that productivity at time t influences esteem at time t, and that esteem at time t influences access to resources at time t and that access to resources, with some delay, eventually brings about an increase in productivity. We then have the following equations, with X_1 = productivity, X_2 = esteem and X_3 = access to research resources, in standard form:

$$X_{1t} = \beta_{13} X_{3,t-1} + U_{1t}; \quad (1)$$

$$X_{2t} = \beta_{21} X_{1t} + U_{2t}; \quad (2)$$

$$X_{3t} = \beta_{32} X_{2t} + U_{3t}. \quad (3)$$

Substituting equation (2) for the X_{2t} term of equation (3), we obtain

$$X_{3,t-1} = \beta_{32}\left[\beta_{21} X_{1,t-1} + U_{2,t-1}\right] + U_{3,t-1}. \quad (4)$$

Substituting equation (4) for the $X_{3,t-1}$ term of equation (1), we obtain

$$X_{1t} = \beta_{13}\beta_{32}\beta_{21} X_{1,t-1} + \beta_{13}\beta_{32} U_{2,t-1} + \beta_{13} U_{3,t-1} + U_{1t} \quad (5)$$

or, in non-standard form,

$$X_{1t} = A_1 + b_{13}b_{32}b_{21}X_{1,t-1} + b_{13}b_{32}U_{2,t-1} + b_{13}U_{3,t-1} + U_{1t}. \quad (6)$$

In other words, productivity at time t is a function of itself at time t-1 and a series of error terms. If we take the risky step of ignoring the error terms, it turns out that the probability of increased productivity at time t is a function of the degree to which the product $b_{13}b_{32}b_{21}$ exceeds unity. For an aggregate of scientists, if the variability in productivity at time t is σ, then the variability in productivity at time t_n is $\sigma(b_{13}\ b_{32}\ b_{21})^n$, where n is the number of time intervals through which the reciprocal causation process is allowed to operate, i.e., the number of "cycles" it is allowed to pass through. If σ^2 at time t_n is to be greater than σ^2 at time t, then at least one of the regression weights defining the reciprocal causation process must be greater than 1.0. Again in passing, it should be pointed out that ignoring the error terms in equation (6) is a dangerous procedure, particularly since it must be assumed that the error terms for X_1 at time t are independent of those for X_1 at time t-1. For independent variables of the sort usually invoked under the heterogeneity hypothesis—e.g., "native ability"—this assumption is glaringly unrealistic. In a sense, then, the least squares procedure shows the importance of gaining statistical control over the variety of independent variables which could be introduced under the "heterogeneity" rubric.

Arguments developed by Allison and Stewart, as well as by the present author, have a syllogistic structure that should be described explicitly as a means of clarifying the exact sense in which these arguments are fallacious[3] and therefore not highly compelling:

(1) If Matthew effect obtains, then, by definition, there is a positive feedback process linking scholarly productivity at time t with productivity at time t+1, with at least one intervening variable involving "reputation."

(2) If such a positive feedback process exists, then, by definition, there is at least one independent causal linkage represented, e.g., by $b_{13}b_{32}b_{21}$, such that the algebraic product of this linkage is greater than zero.

(3) If the product $b_{13}b_{32}b_{21}$ is greater than zero, then this product has a greater than chance probability of being greater than 1.0.

(4) If the product $b_{13}b_{32}b_{21}$ is greater than 1.0, then the variance in scholarly productivity will increase as a cohort moves through its career.

Having established such an increase in variance, having committed the fallacy of asserting the consequent and having retained the qualification in the third premise, we conclude that there is a better than chance probability that Matthew effect obtains. The second premise, which is definitional, is of substantial importance in that the establishment of a positive feedback process requires that a linkage such as $b_{13}b_{32}b_{21}$ exist independently of a number of extraneous factors, and neither Allison and Stewart nor I have demonstrated such a linkage.

An ingenious study by Stephen Cole (1970) attempts to show a causal nexus between a scientist's "professional standing" and the probability that his published papers will receive early recognition (through citation) independently of the "quality" of such papers as assessed by their long-term citation rates. It was found that recognition accorded papers of equal quality is influenced favorably by a scholar's reputation from past work and by his current institutional affiliation; conversely, a number of other measures of "eminence" were unrelated to citation rates. Cole's study is a prototype of the sort of inquiry essential to an accurate determination of the degree to which Matthew effect exists within the stratification system of science. It is a study of limited scope, merely showing that reputation influences one form of deference independently of "quality." Concepts such as "productivity," "esteem" and "access to resources" subsume a wide variety of phenomena amenable to study using both non-experimental and experimental techniques. Indicators of "esteem," including one's name (or one's sex or ethnicity insofar as these traits are reflected in one's name), one's graduate department, one's institutional affiliation or one's honorific recognition, could be assessed either non-experimentally or (in a few instances) experimentally in relation to indicators of access to resources such as time (or space) available for scholarly endeavors, academic appointments, receipt of grants, availability of research assistants, and so forth. An interesting variation on Cole's methodology, for instance, would consist in comparing publications of equal quality (as assessed, for instance, by long-term citation rates) to determine whether the scholarly repute of their authors had an impact on the likelihood of their acceptance for publication

[3] Blalock (1972:112) argues that the fallacy of asserting the consequent is an inherent element of the research enterprise.

by journals in fields where acceptance rates are relatively low and where there may be significant departures from anonymity in the review process. A similar variation was once attempted by Crane (1967), who, using time-series data, found a consistent pattern of concomitance between the prestige of editors and that of authors for a number of academic journals. Crane, however, used no controls for "quality," and therefore her study is limited to the method of agreement.

A feedback nexus such as that represented by the expression $\beta_{13}\beta_{32}\beta_{21}$ involves lag and implies a certain pattern of change in variables over time; time-series data, then, become appropriate, and in the present instance synthetic cohorts have been used in the hope of replicating such data. Variables involved in positive feedback are mutually reinforcing, and the scalar values of such variables increase indefinitely over time until, eventually, an upper limit is reached beyond which an essentially explosive process is not permitted to continue. Even the most powerful instances of Matthew effect cannot extend beyond the lifetime of a single scholar unless one entertains the possibility of "intergenerational" transmission of scholarly eminence which, according to Zuckerman (1967), seems to exist among Nobel prize recipients "sponsored" by earlier Nobel recipients. In any case, we are not confined to the analysis of individuals as the unit of observation, and future studies of Matthew effect should examine the possibility that organizations such as universities and research institutes may receive rewards based less on performance than on repute. A unique feature of such organizations, of course, is that they are not subject to an inevitable mortality, and therefore Matthew effect, over time, could generate an extremely intense concentration of rewards.

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REPLY TO FAIA

Faia has presented evidence tending to corroborate our conclusion that productivity becomes distributed more unequally as scientists grow older. Nevertheless, he argues that our formulation of the accumulative advantage hypothesis does not really imply this increasing inequality. In other words, we seem to have gotten good predictions from a bad deductive theory. Specifically, Faia shows that a system of simultaneous equations relating productivity, resources and esteem does not imply an increasing variance in productivity with time unless the product of certain coefficients in the system is greater than one. Since our theory does not specify the values of these coefficients, the deduction breaks down.

Faia is quite correct about the necessary constraints on the coefficients in his simultaneous equation system, but he has neglected to consider some aspects of our line of reasoning. In particular, he has imputed a much greater degree of model specification than we had intended. Before presenting the simultaneous equation system, we briefly discussed several stochastic models which might plausibly be taken to represent the accumulative advantage or reinforcement hypothesis. We added, however, that "an indefinite number of such models could be constructed, and those simple enough to be tractable usually require unrealistic assumptions." Without opting for any particular model, we noted that models involving positive reinforcement generally imply increasing dispersion with time.

Our prediction that the dispersion in productivity ought to increase with age was based, then, on a survey of several different

models, among which we did not feel there was sufficient basis for choice. We introduced the system of simultaneous equations not to *deduce* an increase in the variance, but to (a) show how the accumulative advantage hypothesis might be formulated in terms of reciprocal causal relationships and (b) to suggest that the *covariances* among productivity, resources and esteem might also increase. We realized that our example of a possible equation system needed some constraints before it implied increasing variances and covariances. However, we were not willing to commit ourselves to any particular model because many models are reasonable and each might require different constraints to imply increasing variances over time periods. For instance, if Faia had used the more realistic model presented in our paper, he would have derived a different set of necessary constraints than those obtained by using his simpler model. Following Faia's steps and making his admittedly dubious assumptions about error terms, it is easy to show that the quantity, $b_1 b_4 + b_2 b_3 b_4$ (in our notation), must be greater than one before our equation system implies increasing variances. Faia's model's constraints are clearly more restrictive.

Turning now to other aspects of Faia's replication, we can find some methodological reasons why his results differ from those we presented. His measure of productivity is based on grouped categories and is truncated at ten or more papers. This will alter the measured variance in two ways: highly productive scientists will have a reduced impact on the variance and the groupings are such that a more reliable measurement of the variance occurs at lower levels of productivity. It is clear that both disciplines and cohorts will vary in average levels of productivity and in the percentage of members with ten or more publications. That these factors must be important is illustrated by comparing Faia's Tables 1 and 2. Table 2 is based on a more homogeneous subsample (respondents in research or doctoral-granting universities) of the cohort members in Table 1. Therefore, we would expect all the variances in Table 1 to be larger than the corresponding variances in Table 2. In fact, only one-third are larger. In addition, we correlated our inequality measures with a weighted mean years since Ph.D., while Faia used an ordinal scale with values from one to eight.

Faia also warns that, since our model applies to individuals and our analysis is done on aggregates, "this anomaly would seem to create a danger of the ecological fallacy." The term "ecological fallacy" usually refers to the use of correlations over aggregates to estimate correlations among individuals (Robinson, 1950). In our analysis, however, there is no corresponding correlation at the individual level. While increasing inequality may be deduced from models referring to individuals, inequality itself is only a property of aggregates and must be analyzed as such.

Next, we note Faia's criticism that "at best it can be said that neither the Allison-Stewart study nor the present study has succeeded in *eliminating* the accumulative advantage hypothesis." Such a criticism can be applied to almost any research in any area of science. "Scientific knowledge" usually refers to those hypotheses that have repeatedly failed to be eliminated by demanding tests. The appropriate question, therefore, is not whether a piece of research proves a hypothesis, but whether it fails to eliminate that hypothesis while reducing the likelihood of compelling alternatives.

Finally, Faia observes that "even the most powerful instances of the Matthew effect cannot extend beyond the lifetime of a single scholar" and suggests that we look at inequality among persistent organizations, such as universities. The suggestion is a good one, but Faia errs in equating the Matthew effect proposed by Merton (1968) and the accumulative advantage hypothesis discussed by Cole and Cole (1973). In Merton's formulation, scientific work of equal merit is more likely to be recognized when the author is already well known, i.e., esteem begets further esteem independently of productivity. Accumulative advantage, on the other hand, means that the flow of resources to highly esteemed scientists may further increase their productivity relative to others. While the two processes may be combined into a single feedback system, the causal dynamics are quite different at each stage of the system.

Since the Matthew effect depends on the reading and citing habits of other scientists rather than on the behavior of the focal individual, it is clearly not limited to a scientist's lifetime. For example, the reputations of deceased sociologists probably become more unequal with each passing year. While the great names of Marx, Weber and Durkheim continue to be highly cited, others slip slowly but surely from view (Westie, 1973).

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IS THERE A SUBCULTURE OF VIOLENCE?*

(COMMENT ON BALL-ROKEACH, ASR DECEMBER, 1973)

A recent major study (Ball-Rokeach, 1973) claims to have rejected the existence of a subculture of violence, especially as formulated by Wolfgang and Ferracuti (1967). The primary data source was a national survey sample of adult Americans administered by the National Opinion Research Center in 1971. Data on value differences among Michigan prison inmates originally collected by Cochrane (1971) also were evaluated. Ball-Rokeach (1973:747–8) concludes that "the relatively weak association between attitudes and violent behavior... taken together with the fact that there is little or no evidence of a relationship between values and violent behavior. This... applies to both culture of violence thesis is at best incomplete and at worst invalid as an explanation of violent behavior. This... applies to both interpersonal violence and violent crime."

However, the study's indicators of attitudes, values and violent behavior raise serious questions of validity. This paper presents a critical reanalysis of the national survey data set. The measurement of violent behavior will be investigated first.

*I am very indebted to Professor Sandra J. Ball-Rokeach for permission to reanalyze her proprietary data and to Professor Milton Rokeach for permission to study in greater detail his Value Survey data. I wish to thank Professors Richard M. Stephenson and Jackson Toby for their personal encouragement and valuable substantive criticisms. The editor also contributed a useful suggestion. This project was supported in part by grants from the Anna Toby Memorial Fund (Department of Sociology, Rutgers) and from the Rutgers Center for Computer and Information Services.

Ball-Rokeach (1973:743) reports that, for both sexes combined, socioeconomic status is unrelated to participation in interpersonal violence ($g = .04$ between education and violence; $g = .07$ between income and violence). But for the more pertinent *male* sample, additional analysis shows a considerably stronger, consistent positive relationship between social class and interpersonal violence: the more education and income, the higher the self-reported participation in several types of violent situations (Table 1).[1] It is unlikely that random measurement error could account for the pattern revealed in the table. (The somewhat anomalous position of the "knife" item will be discussed presently.) A nine-point index constructed of the violence items, including frequency of participation, has identical associations of $r = .12$ with income and education.[2]

Race, not examined in the Ball-Rokeach analysis, is unrelated to the violence index ($g = .04$).[3] Furthermore, from among all males in the sample, the *least* amount of violence is reported by lower-class blacks. Table 2 documents this result for various combinations of race and socioeconomic status.

How do these results compare to previous research on the social distribution of violent behavior?

Although crime in general may not be related to social class, there is considerable evidence that *violent* crime is primarily a lower-class phenomenon (Wolfgang and Ferracuti, 1967:261; Mulvihill and Tumin, 1969:95). The source of this evidence is not limited to official statistics.[4] Wolfgang (1970:30) has concluded that "the increasingly methodologically refined studies of

[1] Traditional significance tests are avoided in this paper because (a) requisite level of measurement assumptions are not always met and because (b) my argument depends less on the statistical significance of a single correlation than on a consistent pattern of results. Pearson correlations are employed as pragmatic measures of association (cf. Acock and Martin, 1974).

[2] (a) Positive responses to the "yes-no" items in Table 1 were followed by the question: "Was that just once or more than that?" Answers were recorded as "once" or "more than that." (b) $r = .97$ between my nine-point index and Ball-Rokeach's (1973:737–8) five-point index of participation in violence.

[3] The positive correlation indicates that whites report (negligibly) more violence than blacks. Ten "other" non-whites are excluded from the analysis.

[4] Recent work by Skogan (1974) supports the validity of official statistics.

Table 1. Income and Education by Participation in Violence[a]: Males (Gamma and Pearson r)

	Income		Education	
	g	r	g	r
Have you ever been punched or beaten by another person?	.17	.13	.22	.14
Have you ever punched or beaten another person?	.20	.13	.19	.13
Have you ever had to use your fists, a knife, or some other weapon to defend yourself from another person?	.14	.11	.12	.08
Have you ever been threatened or actually cut with a knife by another person?	.00	.00	.05	.02

[a]Violence items were answered "yes" or "no."

N = 690 - 719, depending on number of missing cases.

hidden delinquency have not clearly and consistently reported a significant reduction in the disparity of social classes for crimes of violence" (e.g., Myerhoff and Myerhoff, 1964; Fannin and Clinard, 1965; Gold, 1970).

Similarly, blacks appear to be proportionately more involved than whites in violent crime (Short and Strodtbeck, 1965; Wolfgang and Ferracuti, 1967:263; Mulvihill and Tumin, 1969:87–94).

It is noteworthy that official crime rate differentials between social classes and between races are substantially *greater* for physically aggressive than for solely property

Table 2. Percent Reporting High Participation in Violence[a] by Income[b] and Race and by Education[c] and Race (Males)

Low Income Black	Low Income White	Medium Income Black	Medium Income White	High Income Black	High Income White
28 (50)	44 (183)	61 (41)	52 (284)	57 (7)[d]	56 (93)
Low Education Black	**Low Education White**	**Medium Education Black**	**Medium Education White**	**High Education Black**	**High Education White**
21 (38)	44 (70)	52 (51)	50 (329)	75 (12)[d]	52 (175)

[a]Violence index scores of 5 - 8 are defined as "high participation" in violence (see Table 3).

[b]Low income: under $5,999; Medium income: $6,000 - $14,999; High income: $15,000 and over.

[c]Low education: 0 - 7 years; Medium education: 8 - 12 years; High education: 13 and more years.

[d]N too small for stable percentages.

offenses (Wolfgang et al., 1972).[5] This fact is difficult to explain under the typical assumption of a numerically constant bias in the production of official crime statistics. In fact, since the seriousness of an offense is related to the probability of police intervention (Black and Reiss, 1970; Black, 1970), the relative importance of police bias in offense recognition might instead be expected to decrease as the seriousness of the offense increases.

There are nevertheless some delinquency studies which report no differences or inconsistent differences in assaultive behavior between social classes and races (e.g., Empey and Erickson, 1966; Shanley et al., 1966; Gould, 1969; Erickson, 1973).

Physical abuse within the family also seems more prevalent among the economically disadvantaged, though again some investigations disclose no association with social class (Steinmetz and Straus, 1975:8, pass.).

The weight of prior research thus supports the contention that there is probably a negative correlation, or at best no correlation, between social class and violence and between race and violence. In contrast, the Ball-Rokeach data reveal an unexpected positive correlation between social class and violence and a similar interaction effect between race, class and violence. Before we can accept the accuracy of these results, they must be submitted to rigorous methodological scrutiny.

There are two discernable sources of systematic measurement error in the survey: interviewer-respondent interaction and the concept validity of the survey questions themselves. Each problem will be considered in turn.

The National Opinion Research Center relies heavily on white, middle-class, female interviewers. Williams (1964) has demonstrated that the importance of interviewer-respondent status discrepancy is directly related to the amount of threat posed by the inquiry. Fortunately, the substantive nature of much survey research is rather innocuous. In comparison, the present interrogation on personal involvement in violence could well produce defensiveness among those least familiar with social research techniques and among those with the greatest social distance from the interviewer.[6] Underreporting by lower-class males in general and lower-class black males in particular may be understood in this context.[7]

Secondly, the doubtful validity of the self-report index is illustrated by its surprising frequency distribution. Ball-Rokeach (1973:740) reports that fully 54% of the males fell into the category of "high participation" in violence, while 28% were coded as "moderate" participants and 18% disclaimed any involvement whatsoever. The subculture of violence thesis certainly was never intended to account for such an epidemic of violence: "Only a few of those who are in the lower social class, who have below average I.Q.'s, whose families are disrupted, who live in poor neighborhoods... kill others or even act aggressively" (Wolfgang and Ferracuti, 1967:269). Toch's (1969:191) interpretation is that "violence-proneness is restricted to the select minority within the subculture who have fully assimilated its violence-prone teachings and live by them."

The large amount of reported violence in this study appears to be an artifact of the violence measures themselves. Some involvement in fist fights during youth is sufficient to warrant affirmative responses to three out of the four violence items. Moreover, the frequency of participation item has low discriminative power (cf. footnote 2). Table 3 shows that the frequency distribution of the violence index is roughly bi-modal and is skewed toward the higher scores. The distinction revealed seems to be between a relatively candid majority and a more cautious, and thus reticent, minority whose members perceive some threat in the interview situation. As discussed, this minority is disproportionately of lower social class and black.

It is significant that the "knife" item, which *does* refer to relatively uncommon

[5] Wolfgang et al. (1972:72-3) report that the non-white rate is 3.8 times the white rate for property offenses, but it is 6.7 and 20 times as great for assaults and robberies, respectively. For SES levels the corresponding rate differences are 3.5, 4.7 and 6.1, with lower SES boys in each case committing the most offenses.

[6] The primary concession to response bias in the survey was the use of black interviewers for Southern black respondents. Of course, this does not eliminate social class- and sex-related response biases.

[7] The supporting literature on this subject is copious; e.g., Katz (1942) and Cosper (1972) determined that persons are more willing to admit questionable behavior to members of their own, rather than another, social class. Racial response biases are documented by Lenski and Leggett (1960) and Pettigrew (1964). Biases associated with sex are investigated by Benney et al. (1956), Kirsch et al. (1965) and Cosper (1972). Hyman (1954) presents a wealth of relevant material.

Table 3. Frequency Distribution of Violence Index Scores (Males)

Score[a]	Percent	(N)
0	20	(140)
1	6	(42)
2	9	(63)
3	4	(27)
4	11	(77)
5	5	(37)
6	24	(162)
7	12	(81)
8	8	(56)
Total	99[b]	(685)

[a]Respondents scoring "0" reported no participation in violence; respondents scoring "8" claimed multiple involvement in all four violent situations (see Table 1).

[b]Rounding errors.

experiences, is the only violence item clearly unrelated to socioeconomic status (Table 1).[8] Evidently higher status respondents could honestly deny involvement in this marginal situation, while lower status respondents again failed to reveal the full extent of their involvement.

Consideration of the questions on attitudes towards violence provides further evidence in support of a response bias hypothesis (Table 4). Except for the lack of relationship between education and item (2), higher SES males exhibit greater approval of violence as measured by the global attitude items (1) and (2).[9] But a remarkable and consistent shift occurs on the extent of provocation items: the sets of correlations become either negligible or negative. That is, socioeconomic status and attitudes are now either unrelated, or in the case of negative correlations, *lower* SES males demonstrate greater approval of violence. This tends to dispute Ball-Rokeach's (1973:743) report of a positive correlation between socioeconomic status and approval of violence. For race, a similar pattern appears on the four teenage punching items: the degree to which whites approve violence more than blacks *decreases* as we move from the global item (1) to the three specific situations.

The apparent contradictions can be resolved by suggesting that whites and higher SES males had different interpretations of, or were meticulously honest in answering, the ambiguous global attitude items: but when confronted with relatively specific situations, these men gave responses more representative of their actual attitudes. In support of this explanation, it can be shown that the correlational shift on the extent of provocation items is due almost entirely to changes in *higher* status male responses. Unfortunately, there is no direct way of determining the extent to which lower SES and black males masked their true opinions.[10] In sum, this analysis indicates that improved measurement techniques would demonstrate greater approval of violence among lower status males. Since lower status males are also apparently more involved in violent behavior, the subculture of violence concept as originally conceived is certainly not discredited.

Finally, the measurement of "values" in the study seems unsatisfactory. Ball-Rokeach (1973) compared the Rokeach Value Survey responses of men varying in their self-reports of participation in violence and of men imprisoned for violent versus non-violent

[8] 16% of males reported being threatened or cut with a knife once, while only 9% experienced it more than once. In contrast, a majority of males reported more than one involvement on each of the other items.

[9] *Global items*

"Are there any situations you can imagine in which you would approve of:

(1) a teen-age boy punching another teen-age boy?"

(2) a public school teacher hitting a student?"

Extent of provocation items (followed respective global items)

"Would you approve if:

the teen-age boy had been ridiculed and picked on by the other boy?"

the teen-age boy had been challenged by the other boy to a fist fight?"

the teen-age boy had been hit by the other boy?"

the student had been repeatedly disobedient and uncooperative?"

the student had destroyed school property?"

the student had hit the teacher?"

Respondents' answers in each instance were recorded as either "yes" or "no."

[10] Toby (1959) has suggested that the differential cooperativeness of respondents in self-report studies of deviance could result in spurious relationships.

Table 4. Approval of Violence by Income, Education, and Race: Males (Pearson r)

	Income	Education	Race[a]
1. Would you approve a teenager punching another teenager?	.25	.26	.25
If ridiculed?	.05	.01	.12
If challenged?	.07	.00	.08
If attacked?	.06	.02	.11
2. Would you approve a teacher hitting a student?	.14	.04	.04
If disobedient?	-.09	-.15	.00
If destructive?	-.09	-.18	.02
If combative?	.01	-.05	.04

[a]Positive correlations indicate greater approval of violence by whites.

N = 700 - 723.

crimes: no convincing differences were found. However, apart from the validity of the violence measures themselves, this procedure cannot yield information appropriate to a test of the subculture of violence hypothesis. Instead the analysis seems best suited to ascertain the discriminative power of the Rokeach Value Survey for presumed violent and non-violent groups. While this may be a legitimate purpose in itself, an adequate test of the subculture of violence thesis requires more explicit data on *perceived social expectations for physical aggression* as a measure of daring, courage or defense of status (Wolfgang and Ferracuti, 1967:150–63, pass.). That is, the relevant issue is not whether violence-prone men place more emphasis on being "Courageous" and "Independent," but whether *physical combat* is the normative expression of courage and independence for them.

The use of convicts creates an additional difficulty. Incarcerated men familiar with the tactics, and committed to the impression management, of psychiatrists, social workers and parole boards are unlikely to volunteer unusual or deviant responses on officially sanctioned questionnaires.[11] In contrast, Toch (1969) felt compelled to use carefully planned peer interviews to elicit valid information on attitudes and behavior from violence-prone prisoners.

In summary, the substantial questions of validity in this study render premature any definitive statements about the worth of the subculture of violence thesis. Hepburn (1971) found a sufficient empirical basis in the literature for the subculture of violence concept to justify its use in future research. That conclusion remains unchanged.

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[11]Cochrane (1971) claims that "the nature of the [Rokeach] value survey makes it difficult to choose socially acceptable responses." If this is true, then how can the instrument be used to identify, as he did, "specifically *criminal* value systems" or to identify those individuals who have rejected or failed to internalize "the *modal values* of society"? (emphases added).

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REPLY TO MAGURA

ISSUES AND NON-ISSUES IN TESTING A SUBCULTURE THESIS

Magura seems to miss the key theoretical contention of any subculture of violence thesis: the primary determinant of violence is a commitment to some set of subcultural values. He strays from the essential empirical challenge that must be met in order to demonstrate the tenability of any subculture of violence proposal; namely, the hypothesized set of subcultural values significantly differentiates between groups varying in degree of participation in violence. Rather, he goes to considerable effort to re-analyze the N.O.R.C. national sample data with new indices focusing almost exclusively upon the relationships between SES and violence. While I will respond to his major contentions in this regard, it should be noted that my presentation of the SES-violence findings came after the main body of findings under the subtitle "Supplementary Findings" (Ball-Rokeach, 1973a). Failing to find support for any of the subculture of violence theses examined in the main analyses of values and violence, the negligible SES-violence associations became relatively trivial insofar as the central issue addressed in the research was concerned. One could, for example, have a perfect SES-violence association and still not have the

necessary evidence to claim validity for a subculture of violence thesis.

Magura's assessment of the validity of the interpersonal violence index is based upon a failure to take note of a basic conceptual distinction made in my research, namely that interpersonal violence is a general category defined as "the threat or exertion of physical force between two or more persons that could cause bodily injury." As made clear in the article, it is not codeterminous with violent crime, which is more limited in scope to illegal and presumably illegitimate violence. This confusion may explain why Magura is disturbed about the lack of correspondence between class and race correlates of violent crime as determined from official statistics and class and race correlates of interpersonal violence as assessed by self report data. He mistakenly assumes that such lack of correspondence must be due to the different methods employed, completely ignoring the conceptual and phenomenological differences between violent crime and interpersonal violence.

Research that is focused more on violence than on crime, such as Steinmetz and Straus' (1974) research on family violence, Blumenthal et al.'s (1972) study of American male attitudes toward violence or Erlanger's (1974) examination of parental punishment of children would have provided Magura with a more appropriate base of comparison. Moreover, the SES-violence associations reported in such research are not markedly different from those obtained for interpersonal violence.

Many kinds of interpersonal violence are not defined by power holders as social problems (e.g., routine sports violence). It is my view that the classical social problems orientation to violence should be abandoned because it forces a false *a priori* overlap between violence and deviance. It is this belief that led me to conceptualize violence as a phenomenon which pervades all major spheres of social action (e.g., war, recreation, discipline, coercion, protest, pornography, etc.). With this conceptualization in mind, it does not seem too surprising to find that a majority of males have had interpersonal violence experiences nor, for that matter, that SES has a *negligible* association with approval of, or participation in, interpersonal violence.

The validity of Magura's interpretation of the self report data with regard to a greater response bias on the part of lower SES respondents seems unlikely. As stated in my article, I can think of as many reasons why middle-class males might underestimate their participation as reasons why lower-class males might underestimate their participation. Given the conceptual differences between violent crime and interpersonal violence, it is logically fallacious to suggest that because interpersonal violence indices do not show the same patterns of association with SES and race as those found for violent crime, that we can assume the validity of Magura's hypothesis that low SES respondents concealed their prior violence experience while middle SES respondents gave accurate reports. My response to the self report data was not to assert its validity, but to acknowledge its potential for error and, thus, move to a second method—known group comparisons of the values of prisoners incarcerated for violent as opposed to non-violent crimes.

The overriding issue examined in the "Values and Violence" article was whether certain values that have been identified as distinctive of a subculture of violence were given higher priority by those reporting high as compared with low participation in interpersonal violence on the one hand, and those incarcerated for violent as opposed to non-violent crimes on the other. In addition to Wolfgang and Ferracutti's "machismo" subculture which Magura focuses upon, the proposition that violence is a consequence of conformity to "male" culture, and the proposition that violence is a consequence of membership in a lower-class subculture were also examined. *Both* with respect to interpersonal violence and violent crime, the value differences found between relatively violent and non-violent groups were small and few in number and did not closely correspond to "machismo," or "male," or "lower-class" values. It was, therefore, concluded that there was little indication in the findings that any subcultural approach to violence holds much promise.

Magura's only response to the values and violence findings is to dismiss them with the assertion that the Value Survey (1967) "cannot yield information appropriate to a test of the subculture of violence hypothesis." Why not? There is more than ample evidence of its "discriminatory power," that is, systematic relationships between the value rankings so obtained and such attitudes or behaviors as racism, sexism, drug use, alcoholism, homosexuality, political activism, petty theft, religiosity, life style and occupation; (Cross et al., 1970; Rokeach and Parker, 1970; Tate and Miller, 1971; Bishop et al., 1972; Rokeach, 1973; Ball-Rokeach, 1973b; Toler, 1974). It is difficult for me to comprehend on what grounds one would anticipate that a valid value measure would not discriminate between persons character-

ized as violent and non-violent.

Magura's statement that "the relevant issue is not whether violence-prone men place more emphasis on being 'Courageous' and 'Independent,' but whether physical combat is the normative expression of courage and independence for them," is simply another way of saying that violent behavior is a "normative expression" or consequence of certain subcultural value priorities. To say otherwise, would signify a departure from, if not a rejection of, the subculture of violence thesis as I understand it.

In conclusion, I would reiterate a suggestion made in my article. Perhaps we need to determine if meaningful value differences emerge when more refined categories of violence are employed (e.g., violence for social control, change, punishment, power, national security, pleasure, etc.). Certain values may act to promote and justify one type of violence, while other values may play the same role for other types. If one is only interested in the more limited arena of violent crime, then such refinements might also prove useful—i.e., violent crime in the service of conflict resolution, revolution, oppression, economic gain, national "honor," self-defense, racism and anti-racism.

In any case, if future research is to provide confirmation of a subculture of violence thesis, it will do so by demonstrating that a particular kind of violence is the consequence of a particular subset of human value priorities. SES-violence correlations—even big ones—do not a subculture of violence make.

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"AN EXPERIMENTAL ANALYSIS OF SOME STRUCTURAL DETERMINANTS OF EQUITABLE AND INEQUITABLE EXCHANGE RELATIONS:"*

(COMMENT ON BURGESS AND NIELSEN, ASR JUNE, 1974)

Burgess and Nielsen (1974) present experimental evidence which they indicate determines the conditions under which individuals will participate in "equitable" and "inequitable" exchange relations. These conditions, consisting of variations in the rewards and alternative rewards inherent in a given exchange situation, are presented as accounting for individual behavior, with indications that the power of a norm of equity or distributive justice to account for that behavior is negligible. This comment argues that such conclusions are unwarranted by the data presented by Burgess and Nielsen, that in fact, many of their experimental situations are structured so that it is not possible for a subject to behave in accordance with a norm of equity, and that there is no evidence to indicate that subjects are even able to determine whether various conditions could

*I would like to acknowledge Richard M. Cohn and especially Michael Flynn for their incisive comments on an earlier draft of this comment.

produce equitable or inequitable results.

Equity and distributive justice theory (Adams, 1965; Homans, 1974) posit the existence of a norm in society concerning expectations of correctness in the distribution of resources in a given situation. Homans' norm of "distributive justice" and Adams' norm of "equity" deal with the proportionality of inputs and outputs of a given individual in relation to some relevant reference other or reference group. In other words: there exists in society a set of sanctions (norms) governing distributions of resources so that particular distributions (or individual behaviors in a distributive situation) are positively reinforced or punished. A given individual's distributive behavior may also, of course, be reinforced by many other factors, including the value of the resources themselves, involvement of the distributor himself in the outcomes of the situation, or other norms such as altruism and self-interest. Furthermore, if the individual is acting in an experimental situation his or her behavior may come under the reinforcing influence of still other factors including a desire to please the experimenter, or, on the other hand, to outwit the experimenter and second guess his purposes. In other words, an individual may act for a number of reasons when confronted with the need to distribute resources between two individuals (one of whom could be himself). One of these reasons, but by no means the only one, may be the reinforcing capability of a norm of "equity."

Burgess and Nielsen have developed a situation in which individuals must decide about the distribution of resources between themselves and a partner. It's a complex decision for these individuals, however, because they can choose either to work for their partners, so that a partner receives a given amount of resources (in an exchange situation), or they can work only for themselves. Furthermore, the amount of resources they or their partners can receive by either of these methods is manipulated by the experimenters, causing the subjects to take into account the various ratios of effort to reward before making their decisions on how they are to work in the situation. Finally, in some instances, one of the subjects can choose to change the amount of resources attained for a given amount of work.

The value of all this, according to the authors, is that the subjects are put in a situation involving the operationalization of concepts from two theoretical explanations of behavior: the behavioral model of man as embodied in exchange theory and the normative model of man as developed in equity theory.[1] Exchange theory, with its notions of the alternative resources available to the actor (Thibaut and Kelley, 1959) can be a powerful predictor of behavior in this type of experimental situation and can thus "help explain the emergence and maintenance of inequitable and equitable transactions" (Burgess and Nielsen, 1974:427). The authors' intention is clear: to determine if, as they suspect, the exchange theoretic concepts of value of reward and availability of alternatives will be the primary factors governing individual behavior in the experimental situation, even if that behavior is not in line with that which would be predicted by proponents of the reinforcing capability of a norm of equity.

It is widely agreed that the determination of the types of situations in which individuals follow a norm of equity, and the situations in which they don't, is a much needed addition to the growing body of equity studies in the literature (Meeker, 1971; Flynn, 1974; Shapiro, 1975). It will be seen, however, that Burgess and Nielsen do not go far in meeting this aim. Their research does provide important evidence relating to exchange theoretical predictions that individuals take into account the value of resources and alternative sources of resources when behaving. The authors do not, however, provide any evidence for determining variations in the reinforcing capabilities of norms of equity or distributive justice. Burgess and Nielsen's statement (p. 438):

> The occurrence of continued inequitable exchange [as seen in these experiments] directly challenges equity and balance theories which predict an effort to reduce inequity even though the effort may be costly to the participants.

is inappropriate. It will be seen that, for several reasons, the individual who is intent on behaving equitably in the reported experimental situations will, in fact, be unable to determine what behavior is equitable and will therefore be unable to engage in efforts to reduce inequity. The conclusion of the authors about the significance of the lack of such efforts is therefore unwarranted.

There are two ways of approaching the authors' experimental situations and analysis of subject behavior. One approach is to assume that equitable and inequitable *ex-*

[1] Both of these, it can be argued, stem from identical operant conditioning principles (Scott, 1971). The point remains the same regardless of one's terminology, however.

change behavior is the only crucial datum to be considered, and that the structural aspects of the situations (rewards and their sources) are the primary determinants of such behavior. Thus it follows that in some situations these structural variables lead to the subject's participation in inequitable exchange. This type of determination does not by itself, however, allow any conclusions to be reached concerning the efficacy of a norm of equity, as nothing has been indicated about whether or not alternative equitable behaviors were possible, yet foregone. All that can be said is that conditions can be created in which subjects will engage in inequitable exchange.

The second way of approaching the situations is to assume that the subject's *individual* behavior is also part of the equity formulations, such that the subject must consider the monetary rewards of both behaviors *and* the degree of equity involved in both types of behavior. Given this perspective, it would be possible to create situations in which the reinforcing capabilities of both factors could be compared, along with the conditions under which they become more or less important in controlling behavior. It is this perspective which Burgess and Nielsen adopt when commenting on the "challenge" to equity theory they see their data as producing (1974:438, 440, 441). What will be shown here is that the conditions as reported by the authors unfortunately do not allow such determinations to be made, primarily because information about the Individual Ratios involved is so incomplete. Subjects may exchange inequitably; but without the possibility of perceiving that a more equitable solution exists, nothing can be said about an equity norm and its reinforcing capabilities. Any challenge to equity theory must show that other factors are more important than equity considerations in controlling behavior, *in a situation in which equitable behavior is, at minimum, perceived as a possible behavioral choice.*

There are two major factors in these situations preventing the subject from determining the relative equity of the non-exchange behaviors.

(1) Individuals in the experiment do not know what return on investment their partner can make from punching his (the partner's) button individually (Burgess and Nielsen, 1974:433). This means that each subject sitting behind the console is confronted by four relevant pieces of information: what the exchange ratio is for him and his partner; what he (the subject) can make by punching individually; whether or not his partner is working for him and how much he and his partner have made exchanging. The subject is not able to determine if his partner is better off in the exchange or better off in his (the partner's) individual ratio.

(2) Even though the subject can tell how much money he and his partner have made by exchanging, the subject cannot tell how much money his partner has made by using the individual button. Hence, a subject is unable to balance the total exchange earnings of the two individuals, because the other subject's earnings are not known to him.

In sum, although the subject knows that a given exchange ratio is inequitable (in the sense that equal effort by both individuals will result in unequal rewards), the subject does not know how equitable or inequitable the alternative individual ratios would be and also does not know how inequitable the amount of money actually earned by the two on the individual ratio buttons is. Hence, both subjects cannot make judgments concerning the comparative equity of a given behavior, and hence cannot expend the "additional effort" posited as necessary by predictions derived from equity theory.[2]

It will be useful to examine each of the nine conditions of the experiment as reported by Burgess and Nielsen. In each condition, (1) the number of subjects and number of sessions they participated in will be reported; (2) the situation confronting the subjects as they participated will be discussed and finally (3) the results will be discussed with particular reference to any possible evidence concerning the reinforcing capacity of the norm of equity in each situation.

(1) Condition A (3:3/9:9)[3]

1. Eleven dyads for a total of 26 sessions (1 hour each).

2. Both subjects, A and B, saw an even, low effort Exchange Ratio and a high effort Individual Ratio.

[2] It is interesting to note, for example, that Leventhal (1969; 1970; 1971), who has done numerous studies on the importance of the equity norm in recent years, reports data on his post-experimental questions. This allows him to show that subjects in his experiment are well aware of differences in inputs of themselves and other subjects and of differences in outcomes, yet still persist in their behaviors. Burgess and Nielsen make no such inferences about their subjects' awareness of what is or is not equitable.

[3] The first ratio refers to the Exchange Ratios for Subject A and Subject B, respectively, the second to the Individual Ratios for the two subjects, in the same order (Burgess and Nielsen, 1974:431).

3. A stable pattern emerged for the majority of the subjects: they exchanged. Nothing is indicated in this condition contrary to the predictions which would be made from an equity theorist positing a strong reinforcing capability of a norm of equity.

(2) Condition B (9:9/3:3)

1. Eleven dyads for a total of 11 sessions.

2. Each subject saw a high effort Exchange Ratio and a low effort Individual Ratio.

3. A stable pattern emerges for the majority of the subjects: they worked individually. Neither subject knew the other's Individual Ratio. The subjects left an equitable situation (9:9) for a situation in which the other's earning abilities and actual earnings were not known. This does not allow one to say that equity behavior is not reinforcing, however, because one has no knowledge of the individual subject's perceptions or assumptions about his partner's individual earning capacity. Subjects may have felt that the Individual Ratios were identical (as was actually the case in this condition).

(3) Condition C (3:3/9:3)

1. Six dyads for a total of 6 sessions.

2. Subject A saw a high effort Individual Ratio and a low effort, balanced Exchange Ratio. Subject B saw a low effort Individual Ratio and a low effort, balanced Exchange Ratio.

3. No stable pattern emerges. The 12 individuals involved behaved in different ways. Little is determined from this condition since behavior seems to depend on individual differences, as Burgess and Nielsen (p. 435) recognize. Subject B, of course, did not know whether the 3:3 Exchange Ratio is more equitable than the Individual Ratio, which for him is also 3. The individual seeking to behave according to a norm of equity in this condition, would, again, not know what behavior to choose.

(4) Condition D (3:3/9:2)

1. Seven dyads for a total of 24 sessions.

2. Subject A saw a high Individual Ratio and a low effort Exchange Ratio. Subject B saw a low effort Exchange Ratio, and an even lower effort Individual Ratio.

3. The results show a tendency *not* to exchange, although dyads #'s 2, 3, 6 and 7 *do* exchange some in the different sessions. Subject B, who could be expected to be the subject to use the Individual Ratio behavior, could not know whether this was equitable. He may well have thought that A, too, had the Individual Ratio of 2.

(5) Condition E (3:3/9:2) changeable by Subject A to (1:3/9:2)

1. Six dyads for a total of 16 sessions.

2. Subject A saw a high effort Individual Ratio and a low effort Exchange Ratio. He was also confronted with the ability to reduce his Exchange Ratio during the experiment, to a situation where the Exchange Ratio was more advantageous to his partner. Subject B saw a low effort Exchange Ratio and an even lower effort Individual Ratio. But, during the course of the experiment, B was confronted with a new Exchange Ratio (as soon as A changes the ratio), which was beneficial to him.

3. The key datum in this condition is the fact that all of the Subject A's used the "Reduce Ratio" option. Burgess and Nielsen indicate that this choosing of the optional ratio, and the subsequent exchange behavior, is a demonstration of the lack of reinforcing capability of a norm of equity, since the new ratio is imbalanced. But, given that Subject A *wanted* to adhere to a norm of equity, what could he have done? What "effort" could he have engaged in?

(a) Subject A could have refused to use the Reduce Ratio option. It is reasonable to assume here that B would have worked individually in this instance (as Burgess and Nielsen indicate [p. 437]), leaving A with the option of working individually himself. A, though, not knowing B's Individual Ratio, could not know how equitable the results of this behavior would be.

(b) Subject A could use the Reduce Ratio option (which is what most Subject A's did). A could not know what B was making individually, so he did not know the exact amount of B's total earnings. More importantly, as has been seen above, A's choice of the Reduce Ratio option is really the only alternative available to him. None of the others allowed an equitable solution. Burgess and Nielsen's "efforts to reduce inequity," which they suggest are necessary to support equity theory predictions in this condition, are impossible.

(6) Condition F (3:2/9:9)

1. Four dyads for a total of 4 sessions.

2. Each subject saw an imbalanced Exchange Ratio with a high effort Individual Ratio.

3. Stable Exchange developed. Members of the dyad did not know their partners' Individual Ratios and hence did not know whether the obviously imbalanced Exchange Ratio is more inequitable than the Individual Ratio. Again, the structure of this condition

does not allow an individual subject to engage in equitable behavior.

(7) Condition G (3:2/9:9) changeable by Subject A to (2:2/9:9)

1. Four dyads for a total of 7 sessions.
2. Same as Condition F, except that A saw the Reduce Ratio option.
3. Stable exchange, high percentage of A's used the Reduce Ratio option; no contradiction to equitable behavior.

(8) Condition H (3:2/9:5)

1. Four dyads for a total of 8 sessions.
2. A's perceptions identical to those in Condition F. B saw an imbalanced, low effort Exchange Ratio and a medium effort Individual Ratio.
3. Exchange ratio used most often, although there were varied behaviors. This condition is essentially the same as Condition F.

(9) Condition I (3:2/9:2)

1. Two dyads for a total of 2 sessions.
2. A saw a situation identical to that of Condition F. B saw a low effort Individual Ratio and an imbalanced Exchange Ratio.
3. A varied pattern of behavior emerges, but with only 2 dyads for 2 sessions, it is difficult to find any significance in the reported behavior.

In sum, it has been seen that Burgess and Nielsen's experiments do not allow any conclusions to be reached concerning the existence of a norm of "equity" or concerning the conditions under which the norm becomes important in controlling behavior and conditions under which its value is minimal. Burgess and Nielsen state in their conclusions:

> Data from these experiments indicate that subjects will continue to exchange no matter how inequitable the exchange, if the exchange is more profitable than other sources of reinforcement. . . .
>
> Variation in our two independent variables, (1) resource value and (2) the availability of valuable alternatives, determined whether exchange occurred and whether the exchange tended to be equitable or inequitable. (p. 441)

The authors are indicating that if subjects were concerned with equity, they would cease to exchange and engage in some other, more equitable behavior. The condition by condition examination presented above, however, indicates that the situations did not allow subjects to determine if any such alternative behavior was, in fact, equitable. Subjects did not know how much their partners could make or were actually making on their individual buttons. It was not possible, therefore, for subjects to perceive that working individually would produce equitable results. In no instance do Burgess and Nielsen show evidence which indicates that subjects are aware that there is an equitable alternative to the inequitable behavior they are choosing. Lacking such evidence, the importance of the reinforcing capability of a norm of equity or distributive justice remains unresolved.

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REPLY TO NEWPORT

Several years ago, C. P. Snow (1959) wrote of the emergence of two cultures within a culture. He was, of course, referring to the existence of two literate audiences: the lay or humanistic audience on the one hand and the scientific on the other. These islands of noncommunication have been joined by others, at least within the scientific community. The sometimes Topsy-like development of increasingly complex research methodologies has taxed the ability of scientists within, as well as across, disciplines to communicate accurately and effectively.

Clearly, the acceptability of any particular scientific conclusion must rest upon the validity of the research and logical procedures used to obtain that conclusion. Unfortunately, it is often the case that the only persons capable of easily determining the validity of the procedures used are those persons actively engaged in the use of those procedures. While others also may and, indeed, must evaluate the work, it very often requires especially careful and multiple readings and, in extreme cases, direct communication with the scientists in question.

Our article detailing a series of five experiments exploring the determinants of equitable and inequitable exchange relations may be one of those special cases requiring clarification due to the use of a highly developed and specialized set of methodological procedures. For these reasons, then, we welcome the opportunity to respond to the Comment by Frank Newport.

Perhaps we should begin by stating explicitly what we did not attempt to do in our study. First, we did not try to test the "reinforcing capability of the norm of equity." Second, we never meant to imply that the "norm of equity" is negligible or unimportant. What we did try to do was to specify some of the conditions under which people behave either equitably or inequitably in direct exchange relations. We had no interest in proving that people are either equitable angels or inequitable devils. Clearly, people often do behave fairly and equitably and we tried to specify, experimentally, some of the conditions under which this behavior occurs. It should also be clear that people often behave unfairly and inequitably. Again, we simply tried to specify experimentally the conditions under which this behavior is likely. Parenthetically, we might add that a theory should be able to describe the conditions under which a behavior will and will not occur. Apart from our criticisms (pp. 428-9) of research cited in support of equity theory, it might be added that equity theorists have failed to give adequate attention to the occurrence of inequitable or, in Homans' terms, unjust behavior. While we may agree that modern societies increasingly strive to be just and equitable, we would also maintain that they still fall short of the mark all too often.

Third, we never meant to imply that the determination of equity-inequity in our experiments was to be based upon combined exchange and individual earnings, i.e., the relative total earnings of the subjects. Newport claims that we do take this approach. As we stated on page 433, however, "earnings on the individual task do not enter into the equity-inequity equation. . . ." The presence or absence of equity applies only to the exchange relation itself. On pages 438, 440 and 441, and elsewhere, we do refer to the subjects' individual and total earnings but only to emphasize how the value of one's alternatives to an exchange relation can affect that relation. Consequently, Newport's points one and two on page 840 are off target, apparently due to a misunderstanding.

Newport (p. 840) does rightly say that:

> Any challenge to equity theory must show that other factors are more important than equity considerations in controlling behavior, *in a situation in which equitable behavior is, at a minimum, perceived as a possible behavioral choice.*

This requirement was met in all of our experimental conditions. Subjects could always see their relative exchange earnings.

Moreover, it is simply not true, as Newport (p. 839) asserts, that:

> the individual who is intent on behaving equitably in the reported experimental situations will, in fact, be unable to determine what behavior is equitable, and will therefore be unable to engage in efforts to reduce inequity.

Not only could subjects determine at all times whether their relations with their partners were equitable or inequitable, they also always had available the ability to exchange equitably.

Yet, Newport notes on page 838 and elsewhere that in many of the experimental conditions "it is not possible for a subject to

behave in accordance with a norm of equity. . . ." This is a serious charge and, were it true, our results would be very questionable indeed. By design, however, this is simply not true. Perhaps the best way to document our assertion is to discuss the relevant experimental conditions in turn. Before doing this, however, we should note that this point of contention may be due, at least partly, to our use of a technique seldom seen in sociological literature, the free-operant response. As we noted on page 432 under "Apparatus," to assure some standardization for the use of different reinforcement schedules, a timing device was placed in the subjects' consoles to prevent emission of responses greater than two per second. Pretesting had shown that only the most dexterous subjects could respond at such a rate of 120 responses per minute for any length of time. Consequently, subjects had a considerable range of responding to select from—all the way from not responding at all (zero per minute) to responding at some moderate level such as 60 responses per minute to an upper limit of 120 per minute. The subjects could always raise or lower the amount they were giving to their partners simply by altering their rate of responding, and this happened all the time. As we noted (p. 434) in describing the results of Experiment 1:

> Usually, if one subject gave his partner more than he received, he would turn temporarily to the individual task, alternate between working independently and exchanging, or simply lower his rate of exchange until his partner's exchange inputs matched his own.

The point is that, under certain experimental conditions, the maintenance or attainment of equity *in the relation* became secondary to maximizing rewards. To clarify the issue, we will discuss briefly four of the experimental conditions commented on by Newport.

Condition D (3:3/9:2). This is an imbalanced condition due to Subject B having a valuable alternative to the exchange relation which Subject A did not have. The outcome of this condition was that all groups eventually ceased exchanging despite the fact that "all A subjects made numerous periodic attempts to exchange" (p. 435). While Newport may be right in suggesting that Subject B "may well have *thought* [italics ours] that A, too, had the Individual Ratio of 2," he certainly *knew* that A, for some reason, wanted to continue the exchange.

Condition E (3:3/9:2 → 1:3/9:2). This condition gave Subject A the additional behavioral alternative of making exchanging with him potentially more valuable to B than B's individual alternative. The major outcome of this condition was that A subjects used this new alternative successfully to induce B subjects to exchange with them. This restoration of the exchange relation was accompanied by the production of inequitable exchange earnings. On the average, B subjects earned two and one-half times more than A subjects in this condition.

"But," says Newport (p. 841), "given that Subject A *wanted* to adhere to a norm of equity, what could he have done?" There are, in fact, a number of things A could have done were he principally motivated by equity. One, he could have reduced his rate of exchanging so as to give B no more than B gave him; i.e., he could try to match his input to B to his outcome from B. Some A subjects attempted this from time to time, at least in the beginning. The trouble was, this tactic was typically met by B opting out of the exchange relation until A increased his rate of exchange. Two, A could have stood on principle and simply have refused to exchange with B under these circumstances. The data indicate, however, that A subjects were willing to tolerate the inequity to achieve the greater rewards available in the exchange relation. These data, along with those from Experiments 1 and 2, show that the subjects were responding to the value of the rewards received. Three, A could have used the Reduce Ratio option to entice B back into exchanging and then either reduced his rate to equalize exchange earnings or used the option only infrequently. While certain A subjects attempted these tactics for a time, they always met with failure. B subjects had the power advantage and they used it.

Newport did not discuss B subjects under this condition. Yet, what could B have done if he wanted to adhere to a norm of equity? At the very least, B could have behaved equitably (and he clearly saw the inequitable earnings) by exchanging at a higher frequency and/or by not requiring A to use the Reduce Ratio option. Yet all B subjects did exchange contingent on A subjects using the Reduce Ratio option and:

> with the exception of the last two sessions of Group 6, all B subjects augmented their exchange earnings by working periodically on their individual tasks (pp. 437-8).

While those subjects with the power advantage (B) clearly had the capability of restoring equity to their relation with A, they failed to do so. To paraphrase Newport, B subjects failed to take efforts to reduce the inequity

even though they easily could have done so.

Condition F (3:2/9:9). This is an imbalanced condition and, as with the other conditions, it permitted equitable as well as inequitable behavior. Newport's comment (p. 841) that "members of the dyad did not know whether the obviously imbalanced Exchange Ratio is more inequitable than the Individual Ratio" is irrelevant. And his comment (p. 841) that "the structure of this condition does not allow an individual subject to engage in equitable behavior" (p. 842) is wrong. All that was required for the relation to be equitable was that A increase his rate of exchanging, B decrease his, or both. That this was, in fact, possible can be seen by examining the data produced by Condition G, which did result in equity.

Condition I (3:2/9:2). This is an imbalanced condition which actually resulted in equitable behavior for one of the two dyads exposed to it. Newport (p. 842) concludes that "with only 2 dyads for 2 sessions, it is difficult to find any significance in the reported behavior." This brings us back to the beginning of this reply where we discussed the inherent difficulty of ascertaining the acceptability of scientific conclusions. Newport failed to understand the intent of Experiment 5, of which Condition I was just one of four conditions. As we noted on page 439, the strategy used was to make the independent task increasingly valuable to the B subjects until they would begin to match their exchange inputs to A to their outcomes from A, i.e., until they all behaved equitably. This is experimentation used to its fullest capacity. The technique is called control by constancy and is designed to eliminate variation in the dependent variable across subjects experimentally. Its alternative is statistical control which does, in fact, require attention to sample size but which is, at the same time, a less powerful technique.

CONCLUSION

We have tried to show that the criticisms of our study of the determinants of equitable and inequitable exchange relations discussed by Newport are unwarranted. Our study, as does any, certainly has its limitations, but they are not those presented in his Comment.

There are a number of features of our study which made such misinterpretations highly possible. One, the study was one of the first to analyze direct exchange relations experimentally. Such exchange relations are particularly difficult to explore experimentally because the subjects themselves control the actual distribution of rewards. This leads to complexity in experimental design as well as in data analysis. Two, the use of the reversibility design is still rare in sociological research though it is increasingly seen in social psychological, developmental and applied research. Three, the concepts used in this seemingly simple experimental situation such as exchange, resources, reinforcers, equity, balance and power possess definitional ambiguities and operational difficulties. Four, to discuss briefly yet accurately five experiments with nine experimental variations is a very difficult task without risking considerable confusion and misunderstanding.

Finally, our entire approach to the issue of equity was unconventional, given that we were as interested in the determinants of inequity as we were in equity; we explored the problem within a direct exchange situation; we were often questioning equity theory rather than accepting it; and we were working from an explicit behavioral orientation. These factors alone could lead to possible misinterpretation. Again, we thank Frank Newport and the *Review* for giving us the opportunity to clarify some of the more obvious sources of misunderstanding.

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AN UNTESTED HYPOTHESIS: THE EFFECT OF SIZE OF PUBLIC ASSISTANCE BENEFITS ON MIGRATION

(COMMENT ON LONG, ASR FEBRUARY 1974)

Long (1974) maintains there is no evidence which empirically supports the controversial belief that migrants are attracted to cities or states with high average welfare payments. Utilizing 1970 census data on six of the nation's largest cities, he proposes to test the hypothesis that high average welfare payments are positively related to the

immigration of welfare recipients. His paper focuses primarily on black migration, since this migration is most often linked to the welfare issue. Two general conclusions emerge from his analysis. First, black migrants have a lower probability of being welfare recipients than do blacks who are native to the state. Second, among whites no consistent relationship is evident between migration status and receipt of welfare assistance. However, welfare recipients and poverty status are slightly more prevalent among Southern born whites. The hypothesis that high public assistance payments encourage immigration is thus rejected by Long.

Certain weaknesses in Long's paper make his conclusions questionable. First, the failure to include data on the size of public assistance payments limits Long to only an indirect test of his hypothesis. Second, Long assumes that the percentage of migrant families receiving public assistance will exceed that for indigenous families, if his hypothesis is correct. The inaccuracy of this assumption means that even Long's indirect test is invalid. These issues are examined in detail below.

Long maintains that the purpose of his study is to test the relationship between welfare payment size in the six selected cities and migration. However, data on welfare payment levels are not actually included as a variable in his study. Instead, each of these cities is assumed to provide similarly high welfare benefits, making between-city variation in average payments unimportant.

In place of a direct test of his hypothesis, Long makes the assumption that if high welfare benefit levels attract migrants seeking those benefits, then the percentage of families receiving public assistance in a given city will be higher for migrant families than for nonmigrants. He proceeds to test his main hypothesis by comparing the two percentages within each of the cities. Unfortunately, the conclusions he reaches are questionable because the assumption on which they are based is unwarranted.

Migration is a selective demographic process altering not only the population characteristics in the cities of destination, but also altering the characteristics in the cities of origin. The source of Long's mistake is his failure to consider the types of people who migrate out of the cities he selected, plus the types of people who are nonmobile. Those persons most likely to move from a given metropolitan area, whether they be black, white, male or female, are young adults of higher educational, occupational and economic status (Bogue, 1969; Himes, 1971; Price, 1971; U.S. Commission on Population Growth and the American Future, 1972; Bacon, 1973; Tilly, 1974). Increasingly these people participate in a national job-market. This market includes managers and professionals who work for large corporations having branches throughout the nation. Such corporations hire people, educated and working in one part of the country, and then move them wherever they are needed. If they should change jobs, such persons will not ordinarily limit their consideration to the place where they presently live. The poorly educated and occupationally unskilled individual, who has a greater potential for becoming a welfare recipient, is unable to participate in this market.

Welfare recipients native to an area where average benefits are high have an additional incentive to remain there. If the hypothesis is correct that high welfare benefits attract recipients to an area, it should also be true that those same benefits will hold recipients already in the area. The uneducated and unskilled native living where welfare benefits are high has little to gain through migration. First, few occupational opportunities exist elsewhere, and jobs requiring unskilled labor tend to be filled by unskilled indigenous residents (Bogue, 1969:771). The unskilled, welfare dependency prone population may find few employment opportunities in the immediate area, but fewer opportunities exist elsewhere. Second, the welfare recipients in a city where benefits are relatively high cannot move where benefits are appreciably better. Their best alternative is to stay where they are since little can be gained through migration. The welfare function and its payment benefits are thus suggested to have a negative effect on the decision of an indigenous welfare dependent population to migrate from an area where benefits are high, since their basic option elsewhere would be welfare, perhaps at lower levels.

These comments strongly suggest that Long made an unwarranted assumption. If it is true that high welfare benefits attract recipients to an area, it should also be true that those benefits retain native recipients. The hypothesis which emerges is that the percentage of population receiving welfare benefits will increase with the size of the average welfare benefits in both the migrant and nonmigrant segments of the population. There seems to be no basis at this time for predicting which group will have a higher proportion of welfare recipients.

Clearly the issue surrounding the effect of welfare payment levels on immigration to an

area was unresolved by Long's analysis. To provide some test of the relationship, we calculated the average welfare payments per recipient family in each of Long's selected cities using data obtained from the Department of Health, Education and Welfare's publication (1974). Average payment levels exhibited sizeable variation across cities and had a positive correlation with the percentage of families receiving public assistance among both migrants and nonmigrants.[1] Because the correlations are derived from aggregate data for only the six cities utilized by Long, the results are highly tentative. Without additional empirical analysis, no decisive statements are possible regarding the relationship between public assistance and migration.

The relationship could be tested more reliably by utilizing census data on migration status and receipt of public assistance for all metropolitan areas. This data set, coupled with payment size data derived from the Department of Health, Education and Welfare publications, would correct for the small number of cities analyzed by Long. However, conclusions derived from such a study would still be based upon aggregate data.

Ultimately additional data must be collected to answer the questions raised. These data must come from cities which vary in the size of welfare payments and must include measures of migration patterns and receipt of public assistance for individuals living there. Payment size at the origin location versus the destination could then be examined as a possible influence on the migration decision.

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[1] The data, methods of analysis and related tables will be supplied by the authors upon request.

REPLY TO PHILLIBER AND SEUFERT

As is frequently the case in these exchanges, the real issue—often obscured by a hypercritical tone of writing—involves not who is right and who is wrong but what is the question. Most of the issues raised by Philliber and Seufert are not really at odds with the conclusions I reached (Long, 1974).

Philliber and Seufert cite numerous studies showing that for the nation as a whole long-distance migration rates are highest for persons with high levels of education and in professional occupations. A more detailed investigation of the interrelationships among education, occupation and migration propensities at the national level may be found in Long (1973).

The controversial issue of the 1960s and early 1970s, however, concerned not national migration rates according to educational level but the migration of poorly-educated persons from rural areas in general and from the South in particular. An untested hypothesis during the 1960s was that these migrants were unable to overcome the handicaps of their limited educations and contributed disproportionately to poverty and welfare dependence in the largest cities, where the "urban crisis" was most acutely concentrated and where violence erupted in the late 1960s.

In the welfare paper (Long, 1974), I tried to review the literature and use 1970 census data to show that in the 1960s black migrants to the nation's largest cities experienced a period of initial difficulty but thereafter were less likely than black urban natives to have incomes below the poverty line or to be dependent on welfare. Subsequent work by Lynne Heltman and me (Long and Heltman, 1975; Long and Heltman, forthcoming) has demonstrated that after a few years in Northern cities Southern black migrants earned higher incomes in the 1960s than blacks native to Northern cities in spite of the fact that the Southern migrants had lower educational levels. Interestingly, the Southern migrants were found to have taken lower-status and lower-paying jobs than Northern-born blacks, but were able to earn higher incomes because proportionately more of the migrants were in the labor force and working.

This work reinforces the general conclusion of the welfare paper (Long, 1974), whose purposes included showing that an increase in migratory response was not a factor in the increase in the 1960s in the number and proportion of persons on welfare in the largest cities. South-to-North migration of blacks (and poor persons in general) did not go up in the 1960s and the black South-to-North migrants in this period actually came to have lower rates of poverty and welfare dependence partly because of their greater attachment to the labor force. As I have tried to document, many persons did, in fact, think that migration was a major contributor to an assortment of urban problems and that an important policy implication was to attack these problems at the "source" which was assumed to be the South (Long, 1974; Long and Heltman, forthcoming).

Philliber and Seufert appear to think that the purpose of my welfare article was to test the hypothesis that, *ceteris paribus*, cities with high average welfare payments attract more low-income migrants than cities with low average welfare payments. This hypothesis may well be true and does not necessarily contradict the findings of my own work. Furthermore, it would be interesting to see to what extent (if any) welfare eligibles might balance off a high probability of getting on welfare in exchange for a lower size of payment in choosing a destination.

Neither Philliber and Seufert nor I have data to answer such questions. Philliber and Seufert make reference to some correlations they derived from aggregate data. Their footnote states that "The data, methods of analysis and related tables will be supplied by the authors upon request." How am I (or how is anybody) to evaluate their work without seeing the data or being told the size of the correlations, the methods of analysis or the definitions of terms such as migrant and nonmigrant?

Philliber and Seufert reach the definitive conclusion that "Without additional empirical analysis no decisive statements are possible regarding the relationship between public assistance and migration." Other researchers have been able to reach conclusions. Without the benefit of having read my article in ASR, researchers at the Rand Corporation (DeFerranti et al., 1974) published a recent study which the *New York Times* carried under the headline: "STUDY DISPUTES WELFARE 'MYTH': FEW NEWCOMERS REPORTED COMING HERE TO SEEK AID" (Kihss, 1974). Over a year earlier, the *New York Times* reported my own study under the headline: "WELFARE STUDY MINIMIZES IMPACT OF MIGRANT BLACKS" (Kneeland, 1973).

Also, from a review of the literature and from their own correlations of aggregate data, DeJong and Donnelly (1973:344) were able to state: "In summary, the findings are not entirely conclusive, but from both aggregate and individual field study frameworks the weight of the evidence rejects the thesis that differential level of welfare payment is a *direct* cause of nonwhite migration to cities." (Emphasis in original.)

Taken together, these studies do suggest ways of answering a question which, as a Government statistician and social scientist I am often asked: "Do poor people migrate to America's large cities in order to get welfare?"

If, as I have argued, migration cannot account for the rise in the number and proportion of population on welfare in the 1960s, then what is the explanation? The evidence indicates simply that a much larger proportion of eligible persons applied and were accepted on the rolls. Why and how did this come about? According to Steiner (1974:65): "Had there been no civil rights movement and no Economic Opportunity Act, there would have been no expansion of public assistance in the 1960s just as there had been none in the 1940s and 1950s." As many sociological studies have shown, migrants, because they are newcomers, are less likely to be highly integrated in community affairs and therefore were probably less likely to be exposed to the forces which encouraged more persons to apply for welfare in the 1960s.

Philliber and Seufert suggest that an important effect of welfare is to lower migration potential among recipients. That is, welfare recipients, because they cannot afford

to move to areas where payments are higher and are not generally allowed to accumulate the savings to finance a job search in an area where employment opportunities are better, may become "trapped" in the cities where they receive welfare. I think that there is much truth in this point of view, and I am somewhat perplexed by Lansing and Mueller's tentative conclusion (1967:331) that: "Dependency on such aid [public assistance] does not appear to reduce geographic mobility." Nor can I fully reconcile my expectation (and presumably Philliber and Seufert's) of lower rates of migration among welfare recipients with the recent work of DeJong and Ahmad (1975). To study these questions more closely, Lynne Heltman and I currently have in process a study of migration of the welfare and non-welfare poor, based on new tabulations which we have prepared from Current Population Surveys.

A final point on which I agree with Philliber and Seufert is the difficulty—but necessity—of working with published census data. The questions with which we are concerned cannot be answered from the Public Use Samples and illustrate that on many issues social researchers must continue to rely on published tabulations prepared from the detailed census records. My ASR article was based on data published in the 1970 census report on *Mobility for Metropolitan Areas*, and I, too, wish that this census report had been done differently. Suggestions for the 1980 census tabulations on the topics featured in this report are being sought and may be sent to the person who is identified in the 1970 report as having designed it.

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ITEMS (Continued)

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ERRATUM

In the article "Validating Conflict Data" by M. Herbert Danzger (October, 1975), columns labeled "R Square" in Tables 1-4 should read "R Square Change."

1975

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O'Laughlin, Bridget M.
Olesen, Virginia L.
Olsen, Marvin E.
Olsen, Stephen M.
Olzak, Susan
Oppenheimer, Valerie K.
Otto, Luther B.
Ouchi, William G.
Page, Charles H.
Parsons, Talcott
Patchen, Martin
Pearlin, Leonard
Perrow, Charles B.
Perrucci, Robert
Peterson, Richard
Peterson, Warren
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Rains, Prudence
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Reynolds, Janice
Ritzer, George
Roby, Pamela A.
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Rosenberg, William
Rossi, Peter H.
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Roth, Julius A.
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Sanders, Joseph
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Schuessler, Karl F.
Schwirian, Kent P.
Scott, W. Richard
Seeman, Melvin
Sewell, William H.
Short, James F.
Shumsky, Marshall E.
Siegel, Paul M.
Simpson, Richard L.
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Skinner, G. William
Skolnick, Jerome H.
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Snyder, David R.
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Spady, William
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Spilerman, Seymour
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Stewman, Shelby
Stinchcombe, Arthur
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INDEX

AMERICAN SOCIOLOGICAL REVIEW

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CONTENTS OF VOLUME 40, NUMBERS 1-6

ARTICLES

Alexander, Karl & Bruce K. Eckland Contextual Effects in the High School Attainment Process 402
Alwin, Duane F. & Robert M. Hauser The Decomposition of Effects in Path Analysis 37
Aronoff, Joel & William D. Crano A Re-Examinaton of the Cross-Cultural Principles of Task Segregation and Sex Role Differentiation in the Family 12
Bengtson, Vern L. Generation and Family Effects in Value Socialization 358
Berger, David G. See Peterson, Richard A. 158
Bidwell, Charles E. & John D. Kasarda School District Organization and Student Achievement 55
Camilleri, Santo F. See McMahon, Anne M. 616
Carroll, Michael P. Revitalization Movements and Social Structure: Some Quantitative Tests 389
Chase, Ivan D. A Comparison of Men's and Women's Intergenerational Mobility in the United States 483
Chase-Dunn, Christopher The Effects of International Economic Dependence on Development and Inequality: A Cross-National Study 720
Chiricos, Theodore G. & Gordon P. Waldo Socioeconomic Status and Criminal Sentencing: An Empirical Assessment of a Conflict Proposition 753
Chirot, Daniel & Charles Ragin The Market, Tradition and Peasant Rebellion: The Case of Romania in 1907 428
Cohen, Jere, Lawrence E. Hazelrigg & Whitney Pope De-Parsonizing Weber: A Critique of Parsons' Interpretation of Weber's Sociology 229
Cohen, Jere See Pope, Whitney 417
Cook, Karen S. Expectations, Evaluations and Equity 372
Coser, Lewis A. Presidential Address: Two Methods in Search of a Substance 691
Crano, William D. See Aronoff, Joel 12
Danzger, M. Herbert Validating Conflict Data 570
De Fleur, Lois B. Biasing Influences on Drug Arrest Records: Implications for Deviance Research 88
Dickinson, Peter J. See Hauser, Robert M. 279
Dickinson, Peter J. See Hauser, Robert M. 585
Doreian, Patrick See Hummon, Norman P. 813
Eckland, Bruce K. See Alexander, Karl 402
Erbe, Brigitte Mach Race and Socioeconomic Segregation 801
Featherman, David L. See Otto, Luther B. 701
Freeman, John & Michael T. Hannan Growth and Decline Processes in Organizations 215
Frisbie, W. Parker & Dudley L. Poston, Jr. Components of Sustenance Organization and Nonmetropolitan Population Change: A Human Ecological Investigation . 773
Hannan, Michael T. See Freeman, John 215
Harris, Anthony R. Imprisonment and Expected Value of Criminal Choice: A Specification and Test of Aspects of the Labeling Perspective 71
Hauser, Robert M. See Alwin, Duane F. 37

Hauser, Robert M., John N. Koffel, Harry P. Travis & Peter J. Dickinson Temporal Change in Occupational Mobility: Evidence for Men in the United States 279
Hauser, Robert M., Peter J. Dickinson, Harry P. Travis & John N. Koffel Structural Changes in Occupational Mobility among Men in the United States 585
Hazelrigg, Lawrence E. See Cohen, Jere 229
Hazelrigg, Lawrence E. See Pope, Whitney 417
Hewitt, John P. & Randall Stokes Disclaimers 1
Hochschild, Arlie Russell Disengagement Theory: A Critique and Proposal 553
Hope, Keith Models of Status Inconsistency and Social Mobility Effects 322
House, James S. & William M. Mason Political Alienation in America, 1952-1968 .. 123
Hummon, Norman P., Patrick Doreian & Klaus Teuter A Structural Control Model of Organizational Change 813
Hunter, Albert The Loss of Community: An Empirical Test through Replication .. 537
Kasarda, John D. See Bidwell, Charles E. 55
Koffel, John N. See Hauser, Robert M. 279
Koffel, John N. See Hauser, Robert M. 585
Keyes, Jane See Lantz, Herman R. 21
Lane, Angela The Occupational Achievement Process, 1940-1949: A Cohort Analysis 472
Lantz, Herman R., Jane Keyes & Martin Schultz The American Family in the Preindustrial Period: From Base Lines in History to Change 21
McMahon, Anne M. & Santo F. Camilleri Organizational Structure and Voluntary Participation in Collective-Good Decisions 616
Mason, William M. See House, James S. 123
Marwell, Gerald Why Ascription? Parts of a More or Less Formal Theory of the Functions and Dysfunctions of Sex Roles 445
Meyer, Marshall W. Organizational Domains 599
Mueller, Charles W. & Weldon T. Johnson Socioeconomic Status and Religious Participation 785
Ogmundson, Rick Party Class Images and the Class Vote in Canada 506
Otto, Luther B. & David L. Featherman Social Structural and Psychological Antecedents of Self-Estrangement and Powerlessness 701
Pearlin, Leonard I. Status Inequality and Stress in Marriage 344
Peterson, Richard A. & David G. Berger Cycles in Symbol Production: The Case of Popular Music 158
Pope, Whitney See Cohen, Jere 229
Pope, Whitney, Jere Cohen & Lawrence E. Hazelrigg On the Divergence of Weber and Durkheim: A Critique of Parsons' Convergence Theory 417
Poston, Dudley L., Jr. See Frisbie, W. Parker 773
Ragin, Charles See Chirot, Daniel 428
Rosenbaum, James E. The Stratification of Socialization Processes 48
Rossi, Peter H. See Sampson, William A. 201
Roth, Guenther Socio-Historical Model and Developmental Theory 148
Sampson, William A. & Peter H. Rossi Race and Family Social Standing 201
Samuel, Yitzhak See Yuchtman, Ephraim (Yaar) 521
Schultz, Martin See Lantz, Herman R. 21
Snyder, David Institutional Setting and Industrial Conflict: Comparative Analyses of France, Italy and the United States 259
Sørensen, Aage B. The Structure of Intragenerational Mobility 456
Spenner, Kenneth I. "The Internal Stratification of the Working Class": A Reanalysis 513
Stewman, Shelby Two Markov Models of Open System Occupational Mobility: Underlying Conceptualizations and Empirical Tests 298

Stokes, Randall See Hewitt, John P. 1
Stolzenberg, Ross M. Occupations, Labor Markets and the Process of Wage Attainment 645
Terrell, Kermit See Treiman, Donald J. 174
Teuter, Klaus See Hummon, Norman P. 813
Townsend, J. Marshall Cultural Conceptions, Mental Disorders and Social Roles: A Comparison of Germany and America 739
Travis, Harry P. See Hauser, Robert M. 279
Travis, Harry P. See Hauser, Robert M. 585
Treiman, Donald J. & Kermit Terrell Sex and the Process of Status Attainment: A Comparison of Working Women and Men 174
Waldo, Gordon P. See Chiricos, Theodore G. 753
Yuchtman, Ephraim (Yaar) & Yitzhak Samuel Determinants of Career Plans: Institutional versus Interpersonal Effects 521

COMMENTS

Acock, Alan C. & Melvin L. De Fleur Reply to Susmilch, Elliott and Schwartz 687
Allison, Paul D. & John A. Stewart Reply to Faia 829
Babchuk, Nicholas See Williams, J. Allen, Jr. 117
Ball-Rokeach, Sandra J. Reply to Magura 836
Burgess, Robert L. & Joyce M. Nielsen Reply to Newport 843
Chauncey, Robert L. Comment on "The Labelling Theory of Mental Illness" 248
Clemente, Frank & William J. Sauer Voluntary Associations and Minority Status: Comment and Extension 115
Cohen, Jere Moral Freedom through Understanding Durkheim 104
Cohen, Jere, Lawrence E. Hazelrigg & Whitney Pope Reply to Parsons 670
De Fleur, Melvin L. See Acock, Alan C. 687
Elliott, Gregory C. See Susmilch, Charles E. 682
Faia, Michael A. Productivity among Scientists: A Replication and Elaboration 825
Fischer, Claude S. The Myth of "Territoriality" in van den Berghe's "Bringing Beasts Back In" 674
Form, William H. "The Internal Stratification of the Working Class": A Reanalysis . 532
Gove, Walter R. The Labelling Theory of Mental Illness: A Reply to Scheff 242
Hazelrigg, Lawrence E. See Cohen, Jere 670
Johnson, David R. See Williams, J. Allen, Jr. 117
Long, Larry H. Reply to Philliber and Seufert 847
Magura, Stephen Is There a Subculture of Violence? 831
Mazur, Allan Cross-Species Comparisons of Aggression 677
Moberg, Dennis J. A Comment on van den Berghe's Biosocial Model of Aggression . 676
Newport, Frank "An Experimental Analysis of Some Structural Determinants of Equitable and Inequitable Exchange Relations" 838
Nielsen, Joyce M. See Burgess, Robert L. 843
Parsons, Talcott Comment on "Parsons' Interpretation of Durkheim" and on "Moral Freedom through Understanding Durkheim" 106
Parsons, Talcott On "De-Parsonizing Weber" 666
Philliber, William W. & Robert Seufert An Untested Hypothesis: The Effect of Size of Public Assistance Benefits on Migration 845
Pope, Whitney Parsons on Durkheim, Revisited 111
Pope, Whitney See Cohen, Jere 670
Sauer, William J. See Clemente, Frank 115
Scheff, Thomas J. Reply to Chauncey and Gove 252
Schwartz, Shalom H. See Susmilch, Charles E. 682
Seufert, Robert See Philliber, William W. 845

Stewart, John A. See Allison, Paul D. 829
Susmilch, Charles E., Gregory C. Elliott & Shalom H. Schwartz Contingent Consistency and the Attitude-Behavior Relationship: A Comment 682
Van Den Berghe, Pierre L. Reply to Fischer, Moberg and Mazur 678
Williams, J. Allen, Jr., Nicholas Babchuk & David R. Johnson Compensatory and Ethnic Community Theories: Are They Generalizable? 117

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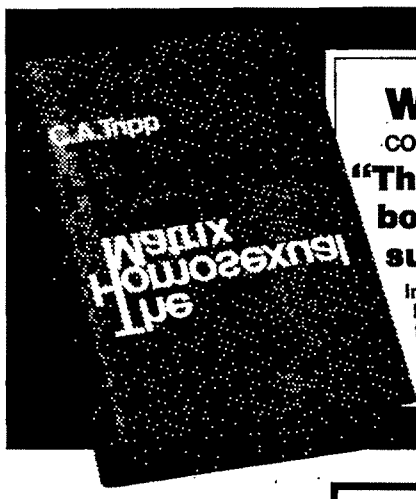

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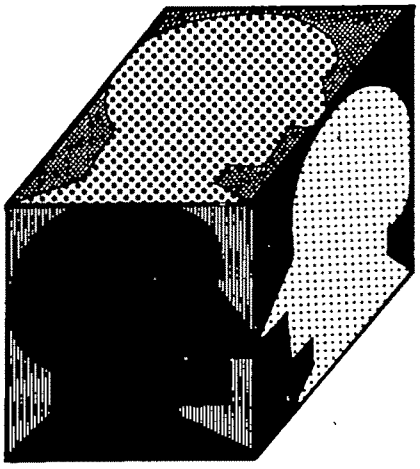

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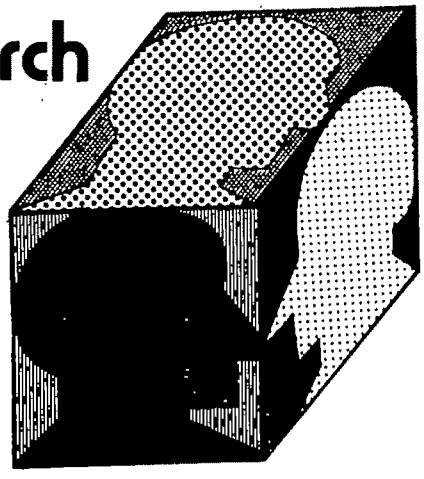

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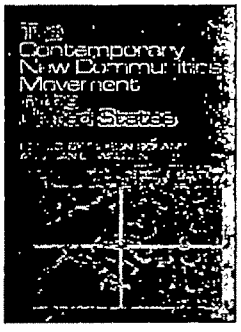

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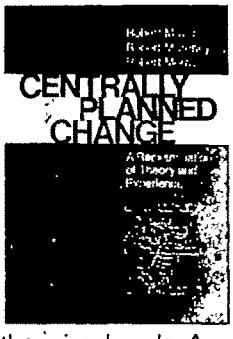

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